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ESSAYS IN APPLIED DEVELOPMENT ECONOMICS

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Abstract

This thesis is a collection of three independent papers using applied microeconomics and focusing on various development topics in India.

The first paper studies the impact of women empowerment policy in politics on women in the household in India. Reserving political offices for women challenges social and cultural norms in India's patriarchal society. The study estimates the indirect effect of the political inclusion of women in local councils on marriages across Indian states by using a staggered difference-in-differences design. I find that the policy induces women to marry husbands with higher education and that they enter into marriages with a lower age gap. I also document an improvement in various measures of female autonomy, most likely explained by selection of a mate. Despite the rise in women's autonomy, however, my results highlight a no effect on intimate partner violence. A plausible mechanism at play is the change in bargaining power within the parents' belief in spouse selection for their daughter.

The second paper looks at the causal effect of gender difference in ownership and in leadership on firm performance, simultaneously. Despite a growing literature investigating the effect of gender diversity (among CEOs) and firm performance, the answer is still unclear. In this study, I exploit a unique data from more than 9,000 Indian firms by using propensity score matching techniques. Findings reveal a positive significant association between female CEO and firm performance but no definite association between the female owners and firm performance. The effect is more prominent in progressive and high-sex ratio states in India. Third, the evidence suggest potential payoffs to firms that adopt gender–inclusive policies intended to increase the share of female CEOs in India. Results confirm no bias from unobservable covariates.

The last paper investigates the impact of district–level alcohol bans on crime in India. The analysis exploits the variation in alcohol prohibition imposed across districts from

1972 to 2016. The quasi–experimental structure of the policy facilitates a difference–in–differences design to estimate the causal impact of the policy on crime outcomes. I use the largest-to-date unique annually archived administrative data on district crime statistics. Results uncover that bans on liquor in districts lead to a significant reduction in criminal activities, particularly for violent crimes, crimes against women, and property crimes in India. The mechanism in play indicates that crimes are primarily perpetrated by individuals who suffer from alcoholism and the effect is substantially driven by high poverty states in India.

Biographical Sketch

The author was born in India. She did her schooling in Jamshedpur (*Steel City of India*), India and graduated from University of Calcutta, Kolkata (*City of Joy*), India with a Bachelor of Science (Honours) degree in Economics in 2012. She subsequently earned a Master of Science degree in Economics (High Distinction) from Symbiosis International University, Pune (*Oxford of the East/ Deccan Queen*), Maharashtra, India in 2014. On completion of her masters, she worked at CUTS International for almost a year as a research assistant in the Center for International Trade, Economics & Environment in Jaipur (*The Pink City*), Rajasthan, India. She then began her doctoral studies in Economics at The University of Adelaide, Australia in 2016. Her research interests lie in applied microeconomics focusing on gender, crime, labour, and development economics. Besides, the author holds a diploma degree with distinction in classical vocals and an advanced diploma degree with distinction in painting.

Note: Indian cities have nicknames (click here) based on their unique characteristics.

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Declaration

I certify that this work contains no material which has been accepted for the award of any other degree or diploma in my name, in any university or other tertiary institution and, to the best of my knowledge and belief, contains no material previously published or written by another person, except where due reference has been made in the text. In addition, I certify that no part of this work will, in the future, be used in a submission in my name, for any other degree or diploma in any university or other tertiary institution without the prior approval of the University of Adelaide and where applicable, any partner institution responsible for the joint-award of this degree.

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Signature

Aditi Roy *Date*

Dedication

To My Parents,

My Beautiful Mum, Sampa (Lipi) Roy, in Heaven

My Dad, Ajoy Chandra Roy, who is Courageously Fighting Cancer

Chapter 1

Empowering Women in the Household: Evidence from Political Inclusion in India

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Statement of Authorship

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Overall percentage (%)	100 %		
Certification:	This paper reports on original research I conducted during the period of my Higher Degree by Research candidature and is not subject to any obligations or contractual agreements with a third party that would constrain its inclusion in this thesis. I am the primary author of this paper.		
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Co-Author Contributions

By signing the Statement of Authorship, each author certifies that:

i. the candidate's stated contribution to the publication is accurate (as detailed above);
ii. permission is granted for the candidate in include the publication in the thesis; and
iii. the sum of all co-author contributions is equal to 100% less the candidate's stated contribution.

Name of Co-Author			
Contribution to the Paper			
Signature		Date	

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Contribution to the Paper			
Signature		Date	

Please cut and paste additional co-author panels here as required.

Abstract

Reserving political offices for women challenges social and cultural norms in India's patriarchal society. This study estimates the indirect effect of the political inclusion of women in local councils on marriages across Indian states by using a staggered difference-in-differences design. I find that the policy induces women to marry husbands with higher education and that they enter into marriages with a lower age gap. I also document an improvement in various measures of female autonomy, most likely explained by selection of a mate. Despite the rise in women's autonomy, however, my results highlight a no effect on intimate partner violence. A plausible mechanism at play is the change in parents' belief in spouse selection for their daughter.

Keywords: marriage, women's autonomy, domestic violence, female politicians

JEL-Codes: J12, J16, K16, O12

1.1 Introduction

Stereotypes and adverse social norms are static and hinder progress toward development and gender equality. Cultural norms, for example marriage traditions, affects various demographic and socioeconomic outcomes (Sen, 1990, Vogl, 2013, Jayachandran, 2015). Such cultural institutions and their potential impact on women's autonomy is particularly important in the developing world. However, it has been difficult to establish causal links in these setting owing to no such policy that can capture the causal framework nature of norms but of course nor hence can provide any other sources that can impact marriages and women's autonomy in the household.

Interestingly, one prominent women empowerment policy has been implemented across the states of India. The policy reserves one-third of local council seats for women. There are several notable studies in the growing literature on this policy. However, thus far no study has evaluated how this policy affects marriages and autonomy. Besides, there is a different strand of literature understanding the drivers of women's empowerment and intimate partner violence in a historical context (Bhalotra et al., 2018, Khalil and Mookerjee, 2019, Menon, 2020). Thus, my study contributes to the literature by investigating the effect of the women inclusion policy on marriage outcomes, women's empowerment, and violence in the household.

Specifically, in this paper I study the impact of the women inclusion policy in the local councils on marriages, evaluating it as a role model effect that can change beliefs and empower women in the household. I take advantage of this natural policy experiment, implemented across states in India, to study the causal effects in how the policy impacts the choice of mate. Further, I look into women's autonomy (intra–household bargaining power) and intimate partner violence in marriage. The policy was implemented by different states at different points in time. The variation in the timing of the policy intervention across states (Iyer et al., 2012) make it a good setting to address these questions because it provided an exogenous shock to marital outcomes. This creates potential variation in the outcome distribution across states and marriage cohorts.

For this study, I use two waves of nationally representative cross–sectional data from the National Family Health Survey, NFHS–3 and NFHS–4. Using variation across the states and marriage year cohorts, my identification strategy uses a difference–in–differences approach. One difference compares data across states and the other difference compares data across the marriage year cohorts. Overall, I consider up to two decades of data on married women's year of marriage, from 1985–2007, across India. The setting of the study allows for a staggered difference–in–differences methodology. Furthermore, the identification strategy controls for state fixed effects, year fixed effects, interview fixed effects, women's demographic covariates, time-varying state–level characteristics, and state–specific time trends.

I find that on average women who married a year after the policy was implemented in their states married husbands with higher education and a smaller age difference. For example, they are more likely to marry husbands with 0.26 more years of education, compared to their cohorts from before the policy change. Further, the age difference between the spouses fell by 0.19 years post the policy, signifying a less intense power struggle in the household (Jensen and Thornton, 2003). I also document that within their marriages, women acquired relatively more freedom of movement or intra–household bargaining power after the policy. Freedom of movement as measured by visits to the family and friends increases by 2.7 percentage points. They are considerably more able to make health care decision by 3.6 percentage points. However, the policy change had no effect on intimate partner violence. This may be because the empowerment of women prompted an offsetting effect in the form of male backlash (Gangadharan et al., 2016, Benjamin et al., 2010).

These results are robust to a battery of checks. The baseline estimated effect remains unchanged after controlling for other concurrent state policies, such as the progressive states' adoption of an improvised version of the 1956 Hindu Succession Act. In fact, the estimated effects become stronger, even after dropping the progressive states. This strong response to robustness checks gives credence to the idea that including women in the local councils indeed helps to change parents' perceptions during spouse selection for their daughters, and also empowers the women in the household by increasing their intra–household bargaining power in the marriage. The results also pass the falsification test in Section 1.4.2. Furthermore, the timing of the exposure does matter for choice of partner and women's autonomy. However, there is no long–term impact on intimate partner violence. Although marriages in India are still mainly driven by parental choices (Banerji, 2008, Prakash and Singh, 2014), however, many parents have started to give more weight to their daughters' choices (Allendorf, 2013). Further, it is not known in the data if the daughters or their parents are making the spouse selection. Thus, the potential mechanism in play is how the exposure to women in leadership positions changes parents belief in spouse selection for their daughter.

Prior empirical evidence suggests that political reform increases women's status in India and changes gender attitudes, while improving men's reception of female leadership (Chattopadhyay and Duflo, 2004, Beaman et al., 2009, 2012, Pande and Ford, 2012). Studies have found significant improvements in welfare caused by the compulsory reservation for women of one–third of the seats on local councils. With an increase in health facilities, child mortality rates have fallen (Kumar and Prakash, 2017). Over time, investment in women's education has likewise increased (O'Connell, 2018b) and the policy has reduced the educational aspiration gap between genders (Beaman et al., 2012). Further studies reveal that more women are likely to report crimes (Iyer et al., 2012). Recent work has shown that female–owned businesses in the informal sector (Ghani et al., 2014) and women's political candidacy (O'Connell, 2018a) have both increased as a result of the policy. Interestingly, prenatal sex selection also dropped drastically (Kalsi, 2017). In other words, findings from Kalsi (2017) suggest that the policy helps change parents' beliefs that they should abort baby girls.

This paper contributes to the growing literature on the women inclusion policy implementation by focusing on marital outcomes and women's roles in their households. Specifically, it enriches previous work on marriage outcomes (in terms of spouse selection), empowering women in the household (focusing on women's intra–household bargaining power), and intimate partner violence.

The rest of the paper is organized as follows. Section 1.2 explains the policy background and describes the data. Section 1.3 explains the design of this study. Section 1.4 reports the results, Section 1.5 represents the pre–trends, and Section 1.6 provides robustness checks. Section 1.7 concludes the paper.

1.2 Background and Data

1.2.1 Policy Background

India uses a federal republic form of a parliamentary government system. India has a bicameral legislature which consists of the president and the two houses–Rajaya Sabha (upper house) and Lok Sabha (lower house). The head of the central government is the prime minister. Similarly, the head of the state government is known as the chief minister. India's Constitution specifies which areas come under the jurisdiction of the central government, which are reserved for the state governments, and which feature shared jurisdiction. However, after much debate on the national government's failure to provide public goods to local areas, in 1993 the country decided to decentralize its government (see Chaudhuri, 2003).

The 73rd and 74th Amendment Acts officially reserved council seats for women in local elections. These amendment acts also required each state to establish a three–tiered local government structure, better known as Panchayati Raj (PR). The PR uses a pyramid-shaped structure (see Figure A.1), where the base is the village level, the middle is the town level (sub–district), and the apex is the district level (see Kalsi, 2017, Iyer et al., 2012, O'Connell, 2018b). It was a bottom–up quota approach that could reach the masses. The PR requires one-third of the seats to be reserved for women at each level. The candidates were elected locally for a tenure of five years. However, the quota system of seat reservation was only applicable to the local elections, not to the state or national elections.

The reservation policy in the local councils was implemented at different times across the Indian states (see Figure 1.1). Iyer et al. (2012) first highlighted the plausible exogenous nature of this policy implementation. First, state–level elections are held at different times across India. Moreover, as the elected members have to serve a fixed period

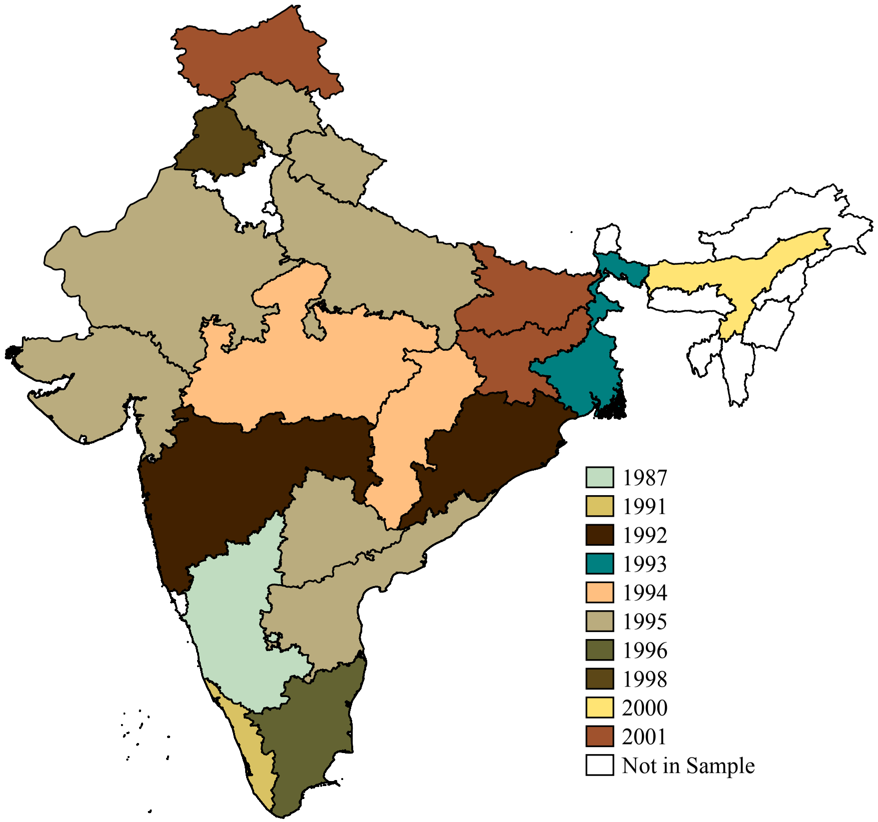


Figure 1.1: Indian States According to Year of Law Implementation

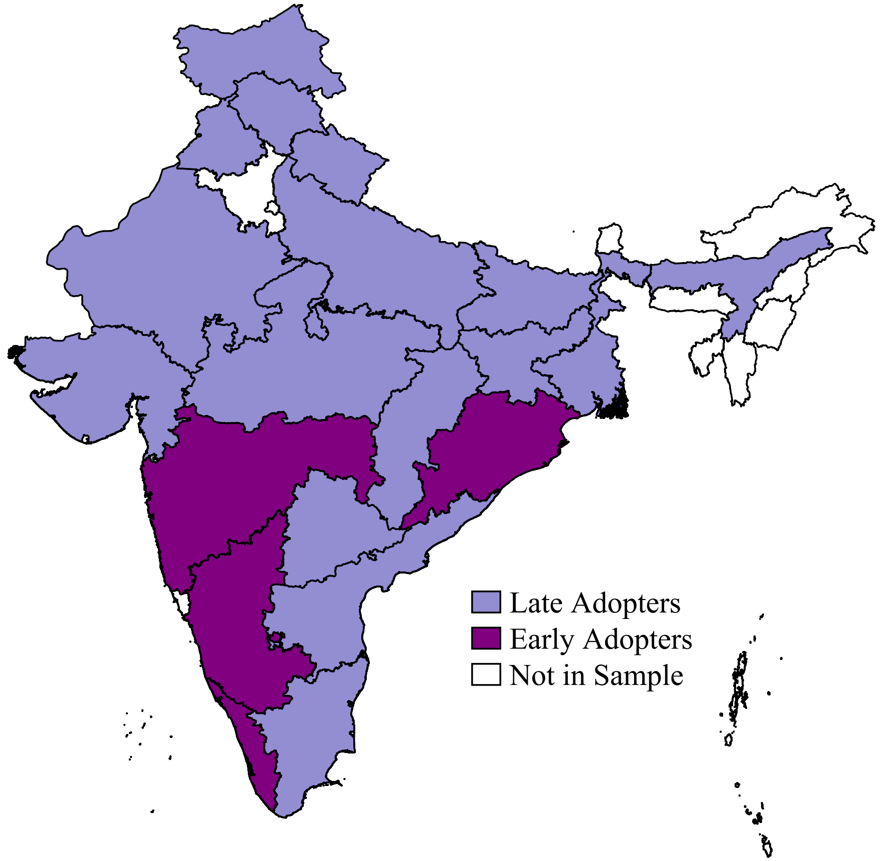


Figure 1.2: Early and Late Adopters

Notes: The Figure 1 illustrates the year in which states across India implemented women inclusion policy in the local councils. In Figure 2 "Early adopter" means a state passed the law for women reservations before the idea took the form of a constitution amendment and "Late adopters" otherwise. To follow sample units over time in Figures 1 and 2, newly created states are merged with their parent states. In 2001, Chhattisgarh was carved from Madhya Pradesh, Jharkhand from Bihar, Uttarakhand from Uttar Pradesh, and Telangana from Andhra Pradesh (in 2014).

of five years, some states had to wait for the previous tenure cycle to end before announcing a fresh election (Iyer et al., 2012) with one–third of the seats reserved for women. Second, following the act, a large number of women became politically active as candidates at the local level (Vyasulu and Vyasulu, 1999). Those women did not have experience in politics (Mathew, 1995) and were unprepared for the responsibilities of governance, and it took the state some time to prepare them (Kalsi, 2017). Third, a lawsuit challenging the act delayed the implementation of the policy in one of the states (see Iyer et al., 2012). The timing of the quota reservation across all Indian states is important for the identification strategy I use. The timing of the policy implementation is independent from the dependent variable, once I control for all the covariates and fixed effects. Furthermore, there are few progressive states (see Figure 1.2) who implemented the women inclusion policy before it was announced as a national policy. I explore on these progressive states[1] further in Section 1.6.2.

1.2.2 Data

Given the state–by–state, staggered nature of this policy's implementation, the analysis requires data with detailed information on women and their marriages, broken down by state and year. To get such data, the study uses a nationally representative data set. I employ two cross-sectional waves from the National Family Health Survey (NFHS) of India, conducted in 2005–6 (NFHS-3) and 2015–16 (NFHS-4).

This study analyzes the section of the survey only completed by women; all of the respondents were married. The survey captures rich information on marriages, including the year in which respondents got married, indicators concerning autonomy measures such as their freedom of movement, participation in household decision–making, occurrences of and views on intimate–partner violence (IPV), and demographic and socio-economic indicators. The questions on intimate–partner violence were asked toward the end of the survey, after developing rapport and trust. Further, women had the option to decline to answer the questions on domestic violence. For homogeneity purposes, in this analysis I

[1] "Progressive states" and "early adopters" are used interchangeably.

restrict the observations to those women who answered the domestic violence questions. The state–level covariates were obtained from Iyer et al. (2012).[2]

1.2.3 Estimation Sample

There are 29 states in India, a few of which have faced decentralisation over time. Some states were carved out of their parent states in 2001, such as Chhattisgarh from Madhya Pradesh, Jharkhand from Bihar, Uttarakhand from Uttar Pradesh, and Telangana from Andhra Pradesh (in 2014). This resulted in 21 states, further I restrict the sample to 17 states as the timing of the policy implementation is known for these many Indian states. To enhance the comparability of states over time, new states are merged with their parent states.[3] Further, Haryana was dropped from the analysis because the state adopted the quota reservation policy at the same time it adopted another policy regarding girls that would disrupt the data.[4] This resulted in a sample of 16 states.

In this paper, the analysis is of women in cohorts. The cohorts are created based on the women's year of marriage. For example, all women married in 1985 are considered as one cohort. Following Iyer et al. (2012), I restrict the analysis to the period 1985 to 2007. To facilitate comparisons, the sample is also restricted to women who are married to their first spouse, as the divorce rate is low in India. Further, the sample is limited to women who were old enough to marry legally[5] because the underlying mechanism of this paper is about making choices through changes in perception. Traditional values, poverty, and dowry payments are all reasons for child marriages (Corno et al., 2017). Therefore, parents often marry their daughters off young and do not have a set of groom candidates for selection. Overall, the sample consists of more than 20,000 observations of women in 23 cohorts across 16 states.

Table 1.1 reports the summary statistics. On average partner had nine years of formal

[2] The data for the state–level control variables were constructed from the Indian censuses of 1981, 1991, and 2001 by interpolation.

[3] Because of aggregation, measurement errors might be present.

[4] Haryana is the state with the largest gender imbalance in India (Sinha and Yoong, 2009). To improve the situation, a policy was implemented known as "Apni Beti, Apna Dhan" (Our Daughter, Our Wealth). This policy provided families with financial assistance if a girl was born. Further, savings bonds were redeemable when the daughter turned eighteen, provided she was still unmarried (Sinha and Yoong, 2009).

[5] Women in India can legally marry after the age of 18 and men can marry after the age of 21.

education. The mean age gap between spouses is approximately five years, while the mean age of women at marriage is twenty-five years. The table also demonstrates that self-reported intimate partner violence is quite high for India.

Table 1.1: Summary Statistics

Variables	Mean	SD
Partner outcomes		
Education (years)	8.93	5.08
Age difference (husband - wife)	5.05	3.45
Autonomy outcomes		
Own healthcare = 1	0.75	0.43
Visiting family and relatives = 1	0.75	0.43
Intimate partner abuse outcomes		
Physical violence = 1	0.23	0.42
Sexual violence = 1	0.04	0.20
State characteristics		
Woman chief minister = 1	0.15	0.35
Per capita state GDP	1.71	0.73
Fraction in farming	0.18	0.05
Fraction literate	0.53	0.11
Fraction of woman literate	0.43	0.13
Female-male ratio	0.94	0.04
Family background		
Caste/Tribe = 1	0.24	0.43
Hindu = 1	0.78	0.41
Urban = 1	0.47	0.50
Male household head = 1	0.94	0.23
Asset index (middle income group) = 1	0.17	0.37
Women's demographics		
Literate = 1	0.78	0.42
Age	24.51	7.11

Notes: The number of observations is 24,655. Author's tabulation of 2005–6 and 2015–16 NFHS.

1.3 Identification Strategy

Given that different states implemented the policy at different points in time, the setting does not facilitate a standard difference-in-differences (DD) methodology. Hence, a staggered difference-in-differences design is implemented, where one difference comes from the implementation of the policy across states and the other difference comes from marriage cohorts, with this specification:

$$Y_{ist} = \alpha + \beta_1 MAP_{is} + \gamma_1 X_i + \gamma_2 X_{st} + \lambda_q + \theta_s + \theta_t + \theta_s \times t + \varepsilon_{ist} \tag{1.1}$$

Here Y_{ist} is the outcome variable for individual women i, living in state s, belonging to marriage year cohort t. The outcomes of interest are the husband's years of education, age difference between the spouses, women's autonomy, and incidence of intimate partner violence. MAP_{is} is an indicator variable of an interacted term (i.e. *Married After*$\times$*Policy*) that takes the value 1 if a woman gets married a year or more after the policy was implemented in her state, the treated cohorts, and otherwise 0 (non-treated cohorts). On average, it takes a year to find a prospective groom and get married (Banerjee et al., 2013). Therefore, I impose the treatment a year after the policy was implemented.

As states adopted the policy at different times, I am able to compare married women with and without the policy exposure based on their state of residence and year of marriage. The parameter of interest is β_1, the effect of having women in leadership roles on marriages after the reform, which is identified from variation within states across marriage–year cohorts. Furthermore, Eq. (1.1) controls for state fixed effects, θ_s; cohort (year of marriage) fixed effects, θ_t; interview-year fixed effects, λ_q; and state-specific time trends, $\theta_s \times t$, to account for changes in a state over time that might affect marriages, such as a general change in gender norms over time. Additionally, I control for women's characteristics and family background in the vector X_i, such as their age, their literacy, whether they are Hindu, living in an urban area, if they belong to a caste which is considered to be of low social status,[6] if they were born into a male-headed household,

[6] Scheduled Caste and Scheduled Tribes are considered the lowest in the caste system of India.

and their amount of household assets.[7] I also control for time-varying state-level characteristics X_{st}, such as whether a state has a female chief minister, the female-male ratio, the state's GDP per capita, the share of the population working in agriculture, the fraction of the population that is literate, and the literate fraction of the female population.

Since the treatment varies at the state level, all models are estimated using clustered standard errors at the state level (Bertrand et al., 2004). To deal with the issue of having few clusters, I use the wild bootstrap-t procedure, following Cameron et al. (2008).[8]

The validity of the difference–in–differences methodology depends on the parallel trend assumption. In order to test if the women inclusion policy had no effects before the policy was implemented, I perform a placebo test in Section (1.4.2) to see if the treated and non–treated cohorts follow a similar trend in the absence of the policy. To test the pre-treatment trends and to see if the policy exposure timing matters, I perform an event study analysis in Section (1.5). The fulfillment of this condition in Section (1.4.2) and Section (1.5) should strengthen the causal inference of this study.

1.4 Results

1.4.1 Baseline Estimates

The baseline estimated results are reported in Table 1.2, Table 1.3, and Table 1.4. These three tables contain the results for the three different outcome variables. Each column in the baseline tables represents the estimated coefficients of the policy reform. To facilitate comparison, Column (1) reports the estimates from including state and year-of-marriage fixed effects. Column (2) adds state-specific time trends. Column (3) includes state-level covariates. Column 4 expands the covariates by including individual-level (women) controls, individuals' family background, and interview fixed effects.

[7] Prior findings on this policy revealed increases in women's education and employment. Therefore, this study does not control for women's education in years and whether they are employed, as these would be bad controls.

[8] The standard errors from the wild bootstrap-t procedure are not reported because we do not know how the t-statistics are distributed, whether they produce asymptotic refinement or not (Cameron et al., 2008). Therefore, in the results table p-values from the wild bootstrap-t procedure are reported for inference purposes for a small number of clusters using the "boottest" command in STATA.

A. Choice of Partner

To investigate the effect of the women inclusion policy on choice of partner, the study uses two key variables: partner's quality of education and age difference between the partners.

Table 1.2: Effects on Partner Choice

Outcome	(1)	(2)	(3)	(4)
Panel I: Partner's education in years				
Married After×Policy	0.389***	0.348**	0.415***	0.262**
	(0.132)	(0.139)	(0.148)	(0.123)
	{0.004}	{0.016}	{0.004}	{0.037}
Panel II: Age difference between partners				
Married After×Policy	-0.245*	-0.149*	-0.200**	-0.191**
	(0.110)	(0.078)	(0.077)	(0.072)
	{0.058}	{0.087}	{0.041}	{0.037}
State FE	Yes	Yes	Yes	Yes
Year-of-marriage FE	Yes	Yes	Yes	Yes
State-specific time trends	No	Yes	Yes	Yes
State covariates	No	No	Yes	Yes
Additional covariates	No	No	No	Yes

Notes: Additional covariates include the women's demographic controls and interview fixed effects. Standard errors clustered at the state level are reported in parentheses. As a robustness check, I use the wild bootstrap-t method with 1,000 repetitions (following Cameron et al., 2008) and report the p-value in braces {.} for Married After Policy * p<0.10, ** p<0.05, *** p<0.01.

As Table 1.2 suggests, the coefficient for partner's years of education (Panel I) is significant and positive (Column 1). The size and statistical significance slightly increases when I include state-specific time trends (Column 2). The coefficient even becomes larger relative to the previous column and is statistically significant (Column 3) when I control for time-varying state covariates. In the preferred specification (Column 4), the post-reform preferred level of partner's education increases by 0.26 years.

A similar pattern is also observed across various specifications for age difference between the partners (Panel II); the difference varies from -0.14 (Column 2) to -0.24 (Column 1) years. The statistical significance of the reform's impact on a couple's age gap increases in the last two Columns (Columns 3–4) when including the state control variables (Column 3), women's demographics, and interview fixed effects (Column 4).

This result suggests that women are marrying more educated partners who are almost equal to their age. The age gap between the partners can affect the status and power within the household (Jensen and Thornton, 2003). A smaller age difference might enable a change in the couple's attitudes and power relations. In particular, women have been marrying better–educated partners closer in age to themselves since the policy change.

B. Effects on women's autonomy in the family

Previous findings from South Asia suggest that married women face social restrictions (Jayachandran, 2015, Khalil and Mookerjee, 2019). The evidence of women entering into marriage with more educated husbands and less age gap between the spouses (Table 1.2) could increase women's freedom of movement and choice after the policy. Women's health in developing countries is most at risk (Garcia-Moreno and Watts, 2011, Vlassoff and Bonilla, 1994) due to domestic violence. And getting to have a say in their freedom of movement is crucial. In this section, I examine the effect of the women inclusion policy on various measures of women's autonomy.

Table 1.3 reports the estimated findings of married women's autonomy, in response to the staggered implementation of the women inclusion policy across states, using Eq. (1.1). In Table 1.3 visiting heath clinics in Panel I takes the value 1 if a women has a say and is able to visit health clinics alone. Column (1) of Panel I in Table 1.3 shows an estimated β coefficient of 0.037 ($p < 0.05$), which amounts to a 3.7 percent relative increase in women's decisions to visit health clinics after incorporating state and year of marriage fixed effects. Column (2) then adds state–specific time trends. However, the estimate remains unchanged with statistical significance at the 5 percent level. Column (3) further adds state covariates and the estimates fall only slightly, to 3.6 percent. Otherwise, the estimate remains unaltered (3.6 percent) when including additional covariates (such as women's demographics and interview fixed effects) in Column (4). Thus, the findings suggest that the probability of a woman getting married after the reform increases autonomy in visiting health clinics (Panel I) and is significant at the 5 percent level. Moreover, there is not much fluctuation in the coefficient and the statistical significance

Table 1.3: Effects on Women's Autonomy

Outcome	(1)	(2)	(3)	(4)
Panel I: Visiting health clinics				
Married After×Policy	0.037**	0.037**	0.036**	0.036**
	(0.010)	(0.010)	(0.013)	(0.013)
	{0.009}	{0.007}	{0.030}	{0.033}
Panel II: Visiting family				
Married After×Policy	0.023**	0.021*	0.026**	0.027**
	(0.011)	(0.012)	(0.013)	(0.013)
	{0.038}	{0.068}	{0.041}	{0.032}
State FE	Yes	Yes	Yes	Yes
Year-of-marriage FE	Yes	Yes	Yes	Yes
State-specific time trends	No	Yes	Yes	Yes
State covariates	No	No	Yes	Yes
Additional covariates	No	No	No	Yes

Notes: Additional covariates are women's demographic controls and interview fixed effects. Standard errors clustered at the state level are reported in parentheses. As a robustness check, I use the wild bootstrap-t method with 1,000 repetitions, (following Cameron et al., 2008), and report the p-value in braces {.} for Married After Policy * p<0.10, ** p<0.05, *** p<0.01.

remains consistent across all specifications (Column 1–4).

The results on married women's decisions to visit their families and friends after the reform is reported in Panel II of Table 1.3. Visiting family and friends in Panel II takes the value 1 if a woman makes her own decision to visit her family members and friends. Column (1) shows a 2.3 percent increase in these decisions after including state and year of marriage fixed effects. Moreover, by incorporating state-specific time trends (in Column (2)) the estimate declines to 2.1 percent and affects the statistical significance. Interestingly, when adding state covariates in Column (3), the relative effect of women's autonomy to visit their friends and relatives increases to 2.6 percent. Furthermore, including interview fixed effects and women's demographic controls (i.e. additional covariates) in Column (4) shows an increase in women's autonomy, with significant estimates (2.7 percent) at the 5% level. Taken together, the findings from Panel I and Panel II of Table 1.3 indicate an overall positive, statistically significant increase in women's autonomy after the policy implementation across states in India.

C. Effects on intimate partner violence (IPV) within the family

Intimate partner violence (IPV) is widespread in many countries (Women, UN, 2015, Butchart and Mikton, 2014). Studies have shown the deleterious impacts of IPV on the well-being of women and their children (Rawlings and Siddique, 2014, Alesina et al., 2013, Aizer, 2011, Ellsberg et al., 2008). Evidence (in Section A.) reveals that entering into marriage with a better quality partner (Table 1.2) in a context of women's political inclusion does have a positive spillover effect on women's autonomy (Table 1.3). Next, I check whether entering into marriages with a more educated partner and a partner closer to one's own age lowers IPV. Lower age gaps between a couple eliminates power imbalances, which could help to reduce domestic violence (Jensen and Thornton, 2003).

Table 1.4: Effects on Intimate Partner Violence

Outcome	(1)	(2)	(3)	(4)
Panel I: Physical Violence				
Married After×Policy	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
	{0.759}	{0.874}	{0.665}	{0.606}
Panel II: Sexual Violence				
Married After×Policy	-0.000	-0.001	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)
	{0.963}	{0.691}	{0.451}	{0.459}
State FE	Yes	Yes	Yes	Yes
Year-of-marriage FE	Yes	Yes	Yes	Yes
State-specific time trends	No	Yes	Yes	Yes
State covariates	No	No	Yes	Yes
Additional covariates	No	No	No	Yes

Notes: Additional covariates are women's demographic controls and interview fixed effects. Standard errors clustered at the state level are reported in parentheses. As a robustness check, I use the wild bootstrap-t method with 1,000 repetitions, (following Cameron et al., 2008), and report the p-value in braces {.} for Married After Policy * p<0.10, ** p<0.05, *** p<0.01.

Physical violence in Panel I takes the value 1 if the wife reports experiencing any of the following: being slapped, pushed, threatened by gun or knife, punched, kicked, dragged, strangled, or burned by her husband; or having her arm twisted or hair pulled by her husband. Sexual violence in Panel II is an indicator variable that equals 1 if the wife ever reports having engaged in forced or unwanted sex with her husband. using Eq. (1.1), Table 1.4 presents the effect of the women inclusion policy on the incidence of IPV. The results show that there is no effect on physical violence in Column (1) of Panel I. Similarly, incorporating state–specific linear trends in Column (2), state covariates in Column (3), and additional covariates in Column (4) reveal insignificant effects on physical violence after the implementation of the policy. Similarly, the estimated coefficient shows a statistically insignificant effect on sexual violence (Panel II). Overall, these findings indicate a null effect on intimate partner violence.

If women are more empowered (in Section B.) and the age gap between the spouses drops (in Section A.), then their husbands may engage in less IPV because of more power balance in the relationship. Interestingly, the findings suggest a null effect. Therefore, the effect of the policy on intimate partner violence is not immediately clear. However, the reason for a lack of effect on IPV might be that cultural and societal norms change slowly (Alesina et al., 2013, Bertrand et al., 2015, Giuliano, 2017). Furthermore, the presence of male backlash against the increase in women's autonomy might be offsetting the effect on IPV (Gangadharan et al., 2016, Benjamin et al., 2010). Thus, more empowered women may report more IPV, even if they suffer less from it(Iyer et al., 2012), possibly driving the null effect.

A potential identification concern is related to migration. However, state migration is often restricted in India because of language and social-network barriers (Munshi and Rosenzweig, 2009). Migration out of the individual's original state is low and varies from zero up to 4 percent (Roy, 2015). Thus, the randomness of the policy assignment and the low degree of migration help to resolve the likely identification concerns.

1.4.2 Placebo Test

The validity of a DD setting is dependent on satisfying the parallel trend assumption. To further support the credibility of the DD identification, I conduct a placebo test. The placebo test evaluates the effect of the women inclusion policy on the marital outcomes of those cohorts that got married before the policy was implemented in their states. If the causal interpretation of this study in Section 1.4 is valid, then the absence of this policy should not have any statistically significant effects before the policy was passed across the states in India. To explicitly test this, I perform the following. First, I restrict the cohorts strictly to the non–treated cohorts. Then, I run a placebo test which assigns a fictitious treatment year two years before the actual implementation of the policy, as a falsification strategy.

The findings from the falsification tests are reported in Tables 1.5 to Table 1.7. Table 1.5 reports the estimates of the policy effects on choice of partner, and the results, as shown in Column (1) to Column (4) for partner's education in years (Panel I) and age gap between the spouses (Panel II), are imprecisely estimated and statistically insignificant as compared to the baseline estimates in Table 1.2.

Table 1.6 shows the impact of the policy's effect on women's autonomy outcomes such as married women's decisions to visit health clinics (Panel I) and friends/family (Panel II). The estimates from Column (1) to Column (4) in Table 1.7 are different in terms of signs and are statistically insignificant. Furthermore, the policy's impact on physical violence (Panel I) and sexual violence (Panel II) in Table 1.7 from Column (1) to Column (4) has no statistically significant effect. Overall, these findings from the placebo treatment reported in Tables 1.5, 1.6, and 1.7 are statistically insignificant. More importantly, this is strong evidence that the causal interpretation of the baseline estimates in Table 1.2, Table 1.3, and Table 1.4 are capturing the policy effects.

Table 1.5: Effects on Choice of Partner by Assigning Placebo Treatment

	(1)	(2)	(3)	(4)
Panel I: Partner's education in years				
Married After×Policy	0.260	0.137	0.007	-0.002
	(0.298)	(0.326)	(0.361)	(0.347)
	{0.491}	{0.744}	{0.988}	{0.995}
Panel II: Age difference between the partners				
Married After×Policy	0.170	0.132	0.126	0.204
	(0.141)	(0.158)	(0.156)	(0.149)
	{0.277}	{0.457}	{0.453}	{0.277}
State FE	Yes	Yes	Yes	Yes
Year-of-marriage FE	Yes	Yes	Yes	Yes
State-specific time trends	No	Yes	Yes	Yes
State covariates	No	No	Yes	Yes
Additional covariates	No	No	No	Yes

Notes: The sample consists of 5,286 observations. Additional covariates include women's demographic controls and interview fixed effects. Standard errors clustered at the state level are reported in parentheses. As a robustness check, I use the wild bootstrap-t method with 1,000 repetitions, (following Cameron et al., 2008), and report the p-value in braces {.} for Married After Policy * p<0.10, ** p<0.05, *** p<0.01.

Table 1.6: Effects on Women's Autonomy by Assigning Placebo Treatment

	(1)	(2)	(3)	(4)
Panel I: Visiting health clinics				
Married After×Policy	-0.007	-0.002	-0.009	-0.010
	(0.031)	(0.029)	(0.026)	(0.026)
	{0.841}	{0.960}	{0.799}	{0.775}
Panel II: Visiting family/friends				
Married After×Policy	-0.024	-0.025	-0.028	-0.028
	(0.019)	(0.017)	(0.020)	(0.020)
	{0.230}	{0.193}	{0.243}	{0.239}
State FE	Yes	Yes	Yes	Yes
Year-of-marriage FE	Yes	Yes	Yes	Yes
State-specific time trends	No	Yes	Yes	Yes
State covariates	No	No	Yes	Yes
Additional covariates	No	No	No	Yes

Notes: The sample consists of 5,286 observations. Additional covariates include women's demographic controls and interview fixed effects. Standard errors clustered at the state level are reported in parentheses. As a robustness check, I use the wild bootstrap-t method with 1,000 repetitions, (following Cameron et al., 2008), and report the p-value in braces {.} for Married After Policy: * p<0.10, ** p<0.05, *** p<0.01.

Table 1.7: Effects on Intimate Partner Violence by Assigning Placebo Treatment

	(1)	(2)	(3)	(4)
Panel I: Physical Violence				
Married After×Policy	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
	{0.629}	{0.818}	{0.635}	{0.628}
Panel II: Sexual Violence				
Married After×Policy	-0.003	-0.005	-0.004	-0.004
	(0.005)	(0.005)	(0.003)	(0.003)
	{0.671}	{0.465}	{0.398}	{0.397}
State FE	Yes	Yes	Yes	Yes
Year-of-marriage FE	Yes	Yes	Yes	Yes
State-specific time trends	No	Yes	Yes	Yes
State covariates	No	No	Yes	Yes
Additional covariates	No	No	No	Yes

Notes: The sample consists of 5,286 observations. Additional covariates include women's demographic controls and interview fixed effects. Standard errors clustered at the state level are reported in parentheses. As a robustness check, I use the wild bootstrap-t method with 1,000 repetitions, (following Cameron et al., 2008), and report the p-value in braces {.} for Married After Policy: * p<0.10, ** p<0.05, *** p<0.01.

1.5 Does the timing of exposure matter?

So far, it is not clear when the exposure to the treatment (timing of the marriage) matters. To fully understand the impact of the policy, the average effect is not sufficient. We need to know more precisely how exposure to the policy affects the timing of marriages. For example, one year of policy exposure before marriage might be different than more years of exposure.

The implementation of the policy in a staggered fashion across all the states makes this analysis more meaningful. Furthermore, the non-parametric set up will help show the pre-treatment effects. The fulfillment of the pre-treatment coefficients can provide us with a test of the parallel trends assumption, which is crucial for identification in a DiD setup. To test if this condition holds, I consider a 6–year window, ranging from three (or fewer) years before the enactment of the law and stretching it till three (or more) years after the policy was introduced, and estimate the following regression:[9]

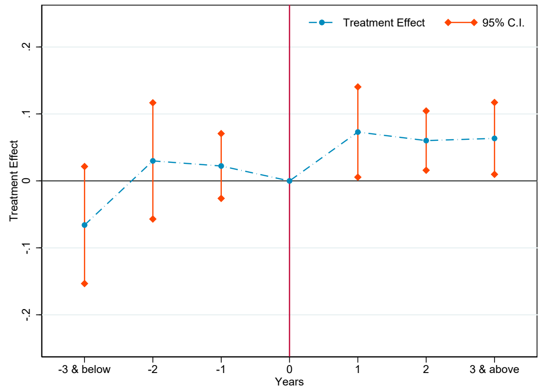
$$\begin{aligned} Y_{ist} = \alpha &+ \beta_1 \text{MAP}_{is}^{\leq -3} + \beta_2 \text{MAP}_{is}^{-2} + \beta_3 \text{MAP}_{is}^{-1} + \beta_4 \text{MAP}_{is}^{+1} \\ &+ \beta_5 \text{MAP}_{is}^{+2} + \beta_6 \text{MAP}_{is}^{\geq +3} + \gamma_1 X_i + \gamma_2 X_{st} + \lambda_q + \theta_s + \theta_t + \theta_s \times t + \varepsilon_{ist} \end{aligned} \tag{1.2}$$

Where MAP_{is}^{-k} takes the value 1 in the k^{th} year before the policy was enacted, and MAP_{is}^{+k} takes the value 1 in the k^{th} year after the women inclusion policy was implemented in the respective states. The other things remain the same as in Eq. (1.1). Furthermore, to show the effect of the women inclusion policy distinctly and dynamically, the year in which the states adopted the policy is excluded to de–trend and center estimates around year 0, defined as the year of policy implementation. Thus, I normalise the data at the vicinity of the policy enactment, which is the point of reference.

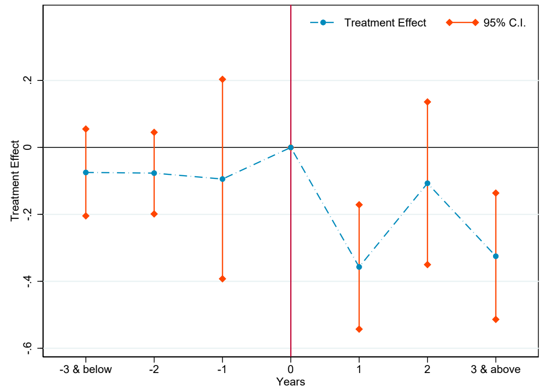
Accordingly, Figure 1.3 presents the coefficients from Eq. (1.2) in six–year bins and includes 95% confidence intervals. First, the treatment effect is estimated for partner choice in education (Figure 1.3(a)), which goes into effect soon after one year of the policy implementation. However, the treatment effect on the age difference is concentrated

[9] The estimates should be interpreted with caution as data points are not comparable.

(a) Choice of Partner

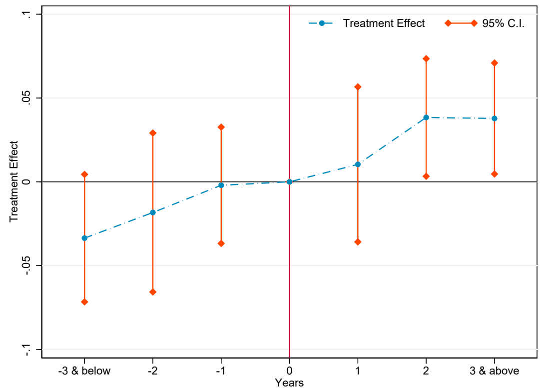


Partner's education

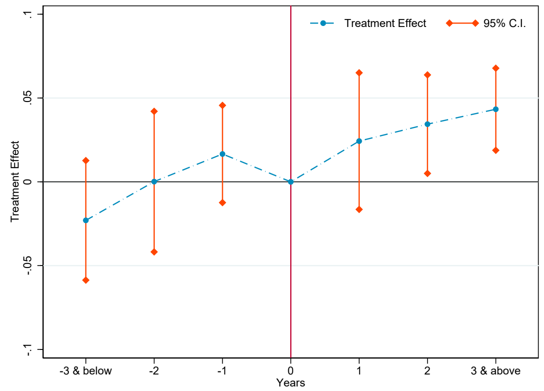


Age difference

(b) Women's Autonomy/ Intra–household bargaining power

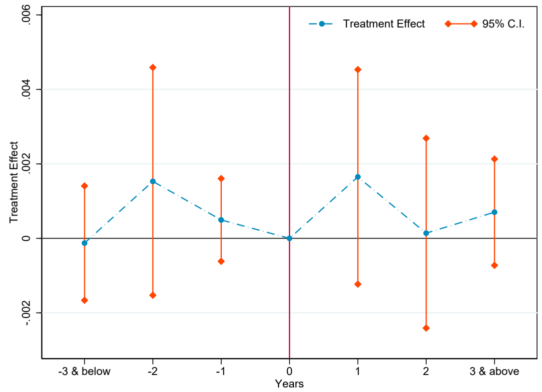


Visiting health clinic

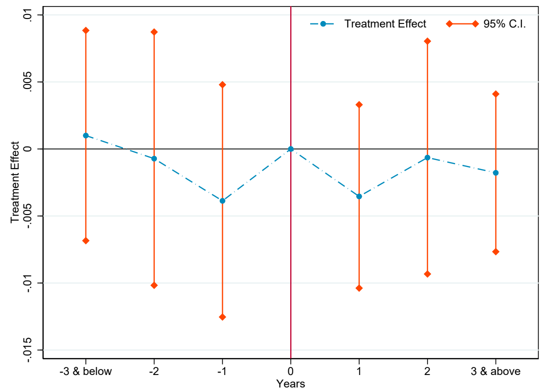


Visiting family/ friends

(c) Intimate Partner Violence



Physical Violence



Sexual Violence

Figure 1.3: Dynamic Treatment Effects of the Inclusion Policy

Notes: The vertical (red) line in each graph refers to the year of policy implementation which is the point of reference.

a year after the policy is implemented, as well as three years or more after. Second, the estimated treatment effect for women's autonomy in freedom of movement in Figure 1.3(b), such as visiting health clinics and family/friends, kicks in after the second year of the policy. Finally, there is no evidence of the policy on intimate partner violence (see in Figure 1.3(c)), either on physical violence or sexual violence. All together, it is evident from Figure 1.3 that the timing of the marriage does matter. Besides, there was no effect on the outcomes before the policy was passed. Thus, the fulfilment of the pre-treatment condition also strengthens the causal interpretation of this study.

1.6 Robustness

1.6.1 Concurrent state policies

In the baseline, I analyse the effect of the women inclusion policy on choice of partner in Table 1.2, women's autonomy in Table 1.3, and intimate partner violence in Table 1.4. However, these results might be driven by other concurrent state policies. India is a patriarchal society and women empowerment policies to reduce male dominance are scant. Interestingly, there were states such as Kerala, Karnataka, Maharashtra, Tamil Nadu, and Andhra Pradesh that improved on a 1956 federal law (the Hindu Succession Act) to pass a prominent women's inheritance law before it was declared a national law in India (known as the Hindu Succession Act) in 2006.[10] The policy did have an effect on woman's autonomy when it was passed as a national law in 2006 (see Nagarajan et al., 2010, Roy, 2008, Heath and Tan, 2014, Mookerjee, 2019).

This section examines whether the baseline increase in women's autonomy in Table 1.3 could be due to any other concurrent state policies. These baseline results could be spuriously driven by concurrent policy factors in the states. This could especially affect the results of the women's autonomy measures. To isolate the causal effect of the increase in women's autonomy is through the women-inclusion policy, and not due to other state

[10] The improvised policy implemented in the states announced that women have a right to their parental property even after marriage. Kerala declared the law in 1976, Karnataka in 1994, Maharashtra in 1994, Tamil Nadu in 1989, and Andhra Pradesh in 1986.

concurrent policies, I control for other concurrently enacted policies by modifying Eq. (1.1) to result in:

$$Y_{ist} = \alpha + \beta_1 MAP_{is} + \beta_2 CSP_{is} + \gamma_1 X_i + \gamma_2 X_{st} + \lambda_q + \theta_s + \theta_t + \theta_s \times t + \varepsilon_{ist} \tag{1.3}$$

where CSP_{is} is an indicator variable that takes the value 1 if state s adopted the improved version of the Hindu Succession Act 1956 (i.e., concurrent policy) to empower women. The other variables and fixed effects remain the same as in Eq. (1.1) in Section 1.3.

Table 1.8: Effects on Women's Autonomy through Other Women-Empowering Policies

Variables	Own health care	Visiting family
	(1)	(2)
Married After×Policy	0.036**	0.027**
	(0.014)	(0.013)
	{0.033}	{0.032}
Concurrent state policies	0.023	0.236
	(0.021)	(0.410)
	{0.383}	{0.553}

Notes: All regressions include state fixed effects, year-of-marriage fixed effects, interview fixed effects, state-specific time trends, state covariate, and women's demographic controls. Standard errors clustered at the state level are reported in parentheses. As a robustness check, I use the wild bootstrap-t method with 1,000 repetitions (following Cameron et al., 2008) and report the p-value in braces {.} for Married After Policy: * p<0.10, ** p<0.05, *** p<0.01.

Table 1.8 presents the regression estimates from Eq. (1.3). The findings show that *CSP* has no significant effect on women's visiting health clinics or family and friends. Interestingly, the relative increase in women's autonomy is statistically significant (see Columns 1 and 2) for those who got married after the implementation of the inclusion policy in their states. There is also an increase of 3.6 percent in visiting health clinics and a 2.7 percent increase in visiting family and friends, which are practically unchanged from both the autonomy outcomes in the baseline result in Section B., even after controlling for other concurrent policies. Reassuringly, the effect on autonomy is not driven by concurrent state policies (i.e. inheritance law). Altogether, this suggests that the estimated effect is not due to concurrent policy change in states, but due to the women's inclusion policy in the local councils.

1.6.2 Early adopters

It is important to understand what is driving the results. In India, some states swiftly passed the policy of including women on the local council before it was announced as a national policy in the 73rd Amendment Act (Iyer et al., 2012). I refer to these states as early adopters. Figure 1.2 represents the early and late adopters. To disentangle whether the results are driven by the early adopter states in India, I re-estimate the policy effect on choice of partner, women's autonomy, and intimate partner violence (the specification given by (1.1)), by dropping one early-adopter state at a time.

Table 1.9 reports the estimates for the choice of partner by regressing Eq. (1.1) and dropping progressive states consecutively. The coefficient (0.147) of the marriage after policy regression on partner education (Table 1.9, Panel I) is small compared to the estimates (0.262) without dropping the states in Section A.. However, the estimate is positive and statistically significant when dropping all early adopters (Column 1). The magnitude of the estimated coefficient in Column (2) increases by dropping Karnataka. When dropping Kerala (Column 3), the coefficient (0.265) remains relatively similar to the baseline estimates (in Section A.). The coefficient size and statistical significance are a little affected when excluding Maharashtra (Column 4) and Orissa (Column 5).

The estimates from the policy impact on age–difference between spouses are reported in Table 1.9, Panel II. Interestingly, even after dropping all the progressive states in Column 1, the size of the estimates considerably falls to 0.276 years from 0.191 years in the baseline (Section A.). Furthermore, the findings indicate a statistical fall in the coefficient, even after excluding Karnataka (0.200 years), Kerala (0.201 years), Maharashtra (0.246 years), and Orissa (0.180 years). Overall, the statistical significance of the age gap between the partners (Panel II) remains unaltered (Columns 1-5) at the 5 percent level.

Table 1.10 reports the estimates of the policy on women empowerment outcomes from Eq. (1.1) by excluding states that adopted the women reservation policy before the

Table 1.9: Effects on Choice of Partner by Dropping Progressive states

	State Dropped				
	All	Karnataka	Kerala	Maharashtra	Orissa
	(1)	(2)	(3)	(4)	(5)
Panel I: Partner's education in years					
Married After×Policy	0.147**	0.309**	0.265**	0.218*	0.198*
	(0.066)	(0.128)	(0.127)	(0.132)	(0.106)
	{0.015}	{0.014}	{0.031}	{0.070}	{0.082}
Panel II: Age difference between the partners					
Married After×Policy	-0.276**	-0.200**	-0.201**	-0.246**	-0.180**
	(0.071)	(0.076)	(0.078)	(0.067)	(0.073)
	{0.004}	{0.036}	{0.040}	{0.010}	{0.042}

Notes: All regressions include state fixed effects, year-of-marriage fixed effects, interview fixed effects, state-specific time trends, state covariates, and women's demographic controls. The number of observations after dropping all early-adopter states (Karnataka, Kerala, Maharashtra, Orissa) is 18,290, for Karnataka it is 23,034, and for Kerala it is 23,689. The number of observations for Maharashtra is 22,074. For Orissa, there are 23,416 observations. Standard errors clustered at the state level are reported in parentheses. As a robustness check, I use the wild bootstrap-t method with 1,000 repetitions, (following Cameron et al., 2008), and report the p-value in braces {.} for Married After Policy * p<0.10, ** p<0.05, *** p<0.01.

Table 1.10: Effects on Women's Autonomy by Dropping Progressive states

	State Dropped				
	All	Karnataka	Kerala	Maharashtra	Orissa
	(1)	(2)	(3)	(4)	(5)
Panel I: Visiting health clinics					
Married After×Policy	0.037**	0.035**	0.036**	0.038**	0.037**
	(0.014)	(0.014)	(0.013)	(0.014)	(0.013)
	{0.049}	{0.051}	{0.028}	{0.031}	{0.017}
Panel II: Visiting family					
Married After×Policy	0.034**	0.023*	0.027**	0.031**	0.032**
	(0.015)	(0.013)	(0.012)	(0.013)	(0.012)
	{0.021}	{0.092}	{0.040}	{0.025}	{0.010}

Notes: All regressions include state fixed effects, year-of-marriage fixed effects, interview fixed effects, state-specific time trends, state covariates, and women's demographic controls. The number of observations after dropping all early-adopter states is 18,290, for Karnataka it is 23,034, and for Kerala it is 23,689. The number of observations for Maharashtra is 22,074. For Orissa, there are 23,416 observations. Standard errors clustered at the state level are reported in parentheses. As a robustness check, I use the wild bootstrap-t method with 1,000 repetitions, (following Cameron et al., 2008), and report the p-value in braces {.} for Married After Policy * p<0.10, ** p<0.05, *** p<0.01.

announcement of the policy as a national law. Evidence from the estimated results suggests that the probability of married women being empowered through their partner selection after the policy change is positive compared to those women who married before the policy change. The outcome on women's decision to visit clinics alone (Panel I) varies between 3.5 and 3.8 percent and is statistically significant at the 5 percent level. The magnitude of the coefficient after the policy change remains almost unchanged (compared to the baseline results in Section B.) after dropping all the progressive states (Panel I, Column 1).

Results in Panel II show that the increase in women's sole decision to visit their family and friends due to the women inclusion policy persists after dropping the progressive states in (Column 1). Moreover, the impact of the policy is statistically significant when excluding Karnataka (2.3 percent), Kerala (2.7 percent), Maharashtra (3.1 percent), and Orissa (3.2 percent). The effect of policy implementation on the relative increase in women's decisions to visit family is positive and statistically significant. Overall, the findings suggest that the relative increase in women's autonomy is not driven by the progressive states alone.

Table 1.11: Effects on IPV by Dropping Progressive states

	State Dropped				
	All	Karnataka	Kerala	Maharashtra	Orissa
	(1)	(2)	(3)	(4)	(5)
Panel I: Physical Violence					
Married After×Policy	0.000	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	{0.767}	{0.411}	{0.959}	{0.559}	{0.680}
Panel II: Sexual Violence					
Married After×Policy	0.000	-0.001	-0.001	-0.001	-0.001
	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)
	{0.835}	{0.673}	{0.538}	{0.564}	{0.734}

Notes: All regressions include state fixed effects, year-of-marriage fixed effects, interview fixed effects, state-specific time trends, state covariates, and women's demographic controls. The number of observations after dropping all early-adopter states is 18,290, for Karnataka it is 23,034, and for Kerala it is 23,689. The number of observations for Maharashtra is 22,074. For Orissa, there are 23,416 observations. Standard errors clustered at the state level are reported in parentheses. As a robustness check, I use the wild bootstrap-t method with 1,000 repetitions, (following Cameron et al., 2008), and report the p-value in braces {.} for Married After Policy: * p<0.10, ** p<0.05, *** p<0.01.

Table 1.11 presents results from the baseline specification by eliminating the states that implemented the policy in advance of the national announcement by the government of India. It is immediately evident from the estimates in Panel I (for physical violence) and in Panel II (for sexual violence) that there is no significant effect on the occurrence of intimate partner violence. Altogether, the coefficient shows a null effect across the Columns (from (1) to (5)).

Taken together, Tables 1.9, 1.10, and 1.11 provide findings that are very similar to and in the same direction as the baseline estimates. This result is valuable as it suggests the estimated effect is not driven solely by states' progressiveness. Hence, the results indicate that regular women were empowered after the reform.

1.7 Conclusion

In this paper I presented evidence of the unintended consequences of women's political inclusion in India. The policy under discussion, which required one-third of all seats on local councils to be reserved for women, has had a role-model effect to change beliefs of marriageable-age women and their parents. I took advantage of the exogenous variation in the timing of the policy's implementation across states. In particular, I used the rollout of the unique policy implementation across states in India. Furthermore, changes in parents' beliefs in the choices of marriage partners for their daughters act as potential mechanisms for this study. I found that women have been marrying better–quality husbands since the policy change. With more–educated partners and a smaller age gap, there is a significant increase in the overall autonomy of women in cohorts married a year after the policy was implemented in their states, relative to those cohorts who married before the policy took effect. With empowered women came male backlash, however, causing an offsetting effect on IPV.

This policy of including women in a three–tiered local governance structure empowers women. Previous studies suggest that the policy aspires to bring more women into politics, shift local government spending toward female–friendly investment, increase female reporting of crime, increase girls' aspirations, reduce sex selection, and many other things. Lastly, the findings of the study have implications for understanding this policy's unintended consequences, here limited to behavior within households. In conclusion, this paper provides evidence for positive spillover effects or role model effects of forced political representation of women: the quota not only directly empowers women but also does so indirectly.

Appendix

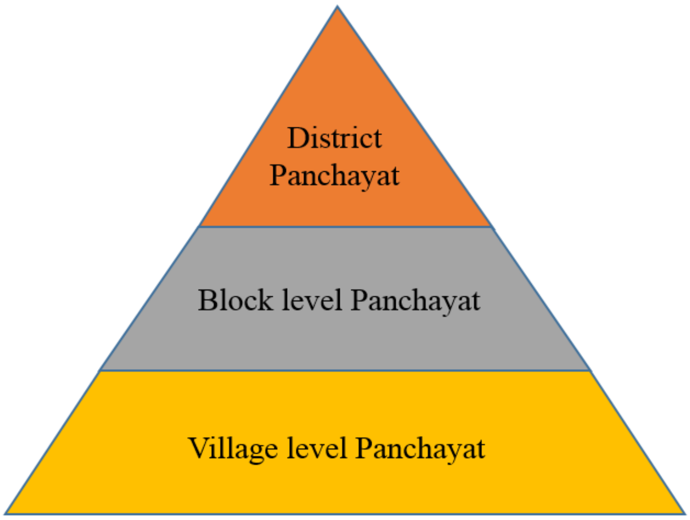


Figure A.1: Panchayati Raj Institution

Notes: The figure illustrates that states were divided into three tired set up– district, block, and village level, together known as Panchayati Raj Instituion. Source: Alok (2011), Kalsi (2017).

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Chapter 2

Gender Differences and Firm Performance: Evidence from India

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Statement of Authorship

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By signing the Statement of Authorship, each author certifies that:

i. the candidate's stated contribution to the publication is accurate (as detailed above);

ii. permission is granted for the candidate in include the publication in the thesis; and

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Abstract

Despite a growing literature investigating the effect of gender diversity of CEOs and firm performance, the answer is still unclear. This study estimates the causal effect of gender difference in ownership and in CEOs on firm performance, simultaneously. I exploit a unique data from more than 9,000 Indian firms by using propensity score matching techniques. Findings reveal a positive significant association between female CEO and firm performance but no definite association between the female owners and firm performance. The effect is more prominent in progressive and high-sex ratio states in India. Third, the evidence suggest potential payoffs to firms that adopt gender–inclusive policies intended to increase the share of female CEOs in India. Results confirm no bias from unobservable covariates.

Keywords: firm performance; ownership; CEO, glass ceiling; propensity score matching.

JEL classification: D21; J16; J71

2.1 Introduction

The issue of under–representation of women at the top hierarchies is prevalent not only in developing countries but also in developed countries (Fortin et al., 2017, Ely et al., 2011, Economist, 2015). For instance, the share of female CEOs in Fortune 500 companies is 25 percent (Fortune, 2018). Despite policies adopted by various countries to increase female representation, contemporary firms continue to be dominated by men (Matsa and Miller, 2013). There are two strands of literature on the role of gender diversity on firm performance: one on the gender of owners and the other on the gender of management.[1] As the literature treats these two aspects separately, there is almost no empirical evidence on which is responsible for the performance of firms: is it the owners or the CEOs? Moreover, much of the existing literature on CEOs concerns advanced economies, studies on developing economies is scant.

This study investigates the impact on how gender of both owners and managers affects firm performance in Indian firms. It exploits the World Bank Enterprise Survey which contains data on more than 9000 Indian firms. The paper studies how gender affects various measures of firm performance such as labour productivity, sales, and exports corresponding to the two strands of literature: (i) female ownership and (ii) female leadership. The criteria defining "female participation in ownership" used in prior studies do not entirely capture the participation intensity of female-owned firms (Presbitero et al., 2014). A cross-country empirical analysis by Aterido and Hallward-Driemeier (2011) reports that for labour productivity, female decision-making power matters rather than partial ownership. Furthermore, Marques (2015) defines decision-making power as the condition in which ownership or top management is all female. To address these measurement issues, this study defines a firm to have female ownership when it is owned by women solely or in a partnership in which the female holds a majority share.

One major concern is that women might self-select into their preferred industry. To address this identification concern, I employ one-to-one nearest-neighbor propensity score matching (PSM). The findings suggest that female leadership has a positive and significant

[1] "Leadership", "management," and "CEO" are used interchangeably in this paper.

impact on firm performance. In presence of female leadership, sales goes up by 13.5 percent, labour productivity by 7.5 percent and exports by 1.5 percent. In contrast, the study finds no definite relationship between female ownership and firm performance. Moreover, the results are driven by progressive states and states with high sex ratios. In other words, states where the average number of females per 1,000 males is above the national average (as measured in the 2011 census) tends to have a positive and statistically significant impact on firm's performance in the presence of female CEOs.

In addition, gender differences stems from many factors which are unobservable for the econometrician. To deal with this issue, I perform Oster (2017) approach to check the regressions' sensitivity to unobserved covariates and to ensure that the bias from the unobserved covariates is in the same direction as the estimate. The results reveal that the unobserved variables do not have the statistical power to influence the direction of the estimates. An explanation for the existence of gender gaps is Becker's theory of statistical discrimination (Becker, 2010). Additionally, the findings suggest potential economic payoffs to firms from adopting gender-inclusive policies that intend to increase the low share of female CEOs in India.

Bloom and Van Reenen (2007) were among the first to explore gender differences in management practices. A women's tenure in the CEO position is short (Cook and Glass, 2014) because women are highly scrutinized (e.g., they are quickly fired if firm performance is low). Some studies reveal that female leadership reduces the gender gaps on boards (Lucifora and Vigani, 2016) and outweighs the cost associated with firm and industry characteristics (Hillman et al., 2007). Moreover, the appointment of a female CEO is endogenous to the gender of the CEO's children (Green and Homroy, 2018).

Existing research has detected that discrimination increases the profitability of male-owned firms (Hardy and Kagy, 2018). It also reveals that female-owned firms underperform in comparison to their male counterparts (Klapper and Parker, 2010). These findings contrast with those of Kepler et al. (2007), who find no such effect of gender difference on ownership. However, Sabarwal and Terrell (2008) find that in Eastern Europe and Central Asia, sales performance of female-owned firms is much lower than that of their male-owned counterparts. Most importantly, country specific studies are scant.

This study makes several contributions to the literature. First, to the best of my knowledge, this is the first study to investigate the association between firm performance and the gender of firm owner and CEO simultaneously. Second, this paper is the first in employing matching techniques in the context of gender differences and firm performances, where women may self select into their profession. Third, studies on gender and firm performance in developing countries are scant. This paper performs micro level data analysis in India to answer the impact of gender differences and firm performance. Lastly, the paper may be interesting for policy makers who aim to promote gender equality and development. The findings from this paper highlight the importance of having gender-inclusive policies which are essential for economic development. The remainder of the paper unfolds as follows: Section 2.2 describes the data, Section 2.3 illustrates the empirical strategy, Section 2.4 presents the results, and Section 2.5 concludes the paper.

2.2 Data

This study uses the World Bank Enterprise Survey 2014 survey, conducted from June 2013 to June 2014 by the World Bank in collaboration with Nielson India. The data were collected using a stratified random sampling of $9,281$ formal enterprises in twenty-seven states in India. The survey covered firms across retail, non-retail, and manufacturing sectors, excluding agricultural and extracting industries. In addition to gender and firm characteristics (such as firm age and size), the survey provides detailed information on the business environment (such as whether firms have engaged in bribery).

This study controls for a new variable in the literature: firms' female hiring constraint. "The firms were asked if any of the following were constraints to hiring women?"
1) Challenges of working with women due to their family commitments,
2) Challenges of hiring women given government regulations such as working hours and maternity leave.

Female hiring constraint is an indicator variable that takes the value 1 if any of the mentioned challenges was answered as and yes, and 0 otherwise. On average over sectors, twenty-two percent of the firms in the dataset face constraints in hiring women. Figure 2.1 shows that the average sales of firms with a female hiring constraint are lower than those for firms without a constraint.

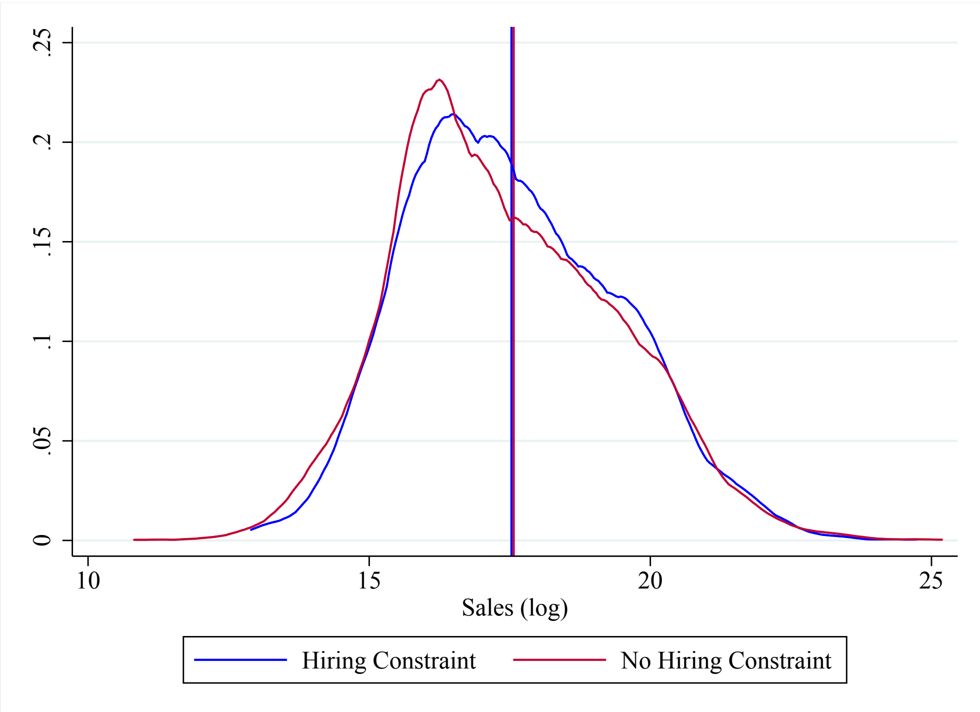


Figure 2.1: Average sales of firms with and without a female hiring constraint
Notes: Vertical bars is the average of log sales for the two types of firms.

Table 2.1 provides summary statistics of key variables used in the study. Female Owner is a binary variable that takes the value 1 if a female is the sole proprietor or has a majority share in a partnership (more than 50 percent) and Female CEO signifies whether the CEO is a woman in the top management of an enterprise.Firm age is the number of years the firm has existed. Firm size indicates the number of workers. Firms in the data-set are quite old, with the youngest firm being only five years old. A firm is considered small when it employs twenty or fewer employees but medium and large when it employs more than twenty but fewer than ninety-nine and above ninety-nine, respectively. Foreign ownership indicates whether more than 10 percent of the firm's capital are held by a foreign company. Access to credit is measured by the ease of accessing credit, either internally or by a loan.

Internet access is an indicator that a firm has an email address and website. Multi-plant firms are part of a large firm and CEO's experience is a measure of a CEO's tenure in years. I also control for the education, such as, if the workers have completed secondary and bachelor's studies. Moreover, formal training indicates if training was provided to the workers. Table 2.1 shows that only 4 percent of firms are owned by women, and only 8 percent of sampled firms have a female CEO. Firms both managed and owned by women account for less than 0.01 percent.

Table 2.1: Summary Statistics

Variables	Mean	SD	Min	Max
Female Owner = 1	0.04	0.20	0.00	1.00
Female CEO = 1	0.08	0.27	0.00	1.00
Firm Age (in years)	23.29	13.90	5.00	155.0
Access to Credit = 1	0.27	0.45	0.00	1.00
Large City = 1	0.40	0.49	0.00	1.00
Multi-plant Firm = 1	0.22	0.41	0.00	1.00
Foreign Ownership = 1	0.01	0.09	0.00	1.00
Internet Access = 1	0.86	0.34	0.00	1.00
Small = 1	0.34	0.47	0.00	1.00
Medium = 1	0.44	0.50	0.00	1.00
Large = 1	0.23	0.42	0.00	1.00
CEO Experience (in years)	13.43	9.25	1.00	65.00
Female Hiring Constraint = 1	0.22	0.42	0.00	1.00
Formal Training for Workers = 1	0.43	0.49	0.00	1.00
Secondary Education of Workers = 1	0.51	0.32	0.00	1.00
Bachelor's Education of Workers = 1	0.28	0.28	0.00	1.00

Notes: Multi-plant firms are a part of large firms. *Source*: Author's calculation.

Figure 2.2 shows kernel density plots of the distributions of a set of firm-performance measures and their respective averages corresponding to firms with male (blue line) and female (red line) owner (and CEOs) in 2013–14. The vertical lines indicate the average of the firm-performance measures by gender. The kernel densities are estimated using kernel bandwidths.[2] The top panel in Figure 2.2 represents the gender of the firm owner. We observe that the means for log sales (Figure 2.2a) and log labour productivity (Figure 2.2b) are substantial for men. However, for exports (Figure 2.2c), the mean of female ownership

[2] Kernel = Epanechnikov, Bandwidth = 0.2409.

is larger than that of male ownership. Furthermore, the lower panel in Figure 2.2 presents the gender of firm CEOs and indicates that firms with female CEOs have larger sales, labour productivity, and exports than their male counterparts (Figure 2.2d, 2.2e, and 2.2f).

2.3 Identification Strategy

This study aims to identify the effect of gender differences on firm performance simultaneously on two levels: (i) female ownership, and (ii) female leadership. To investigate the causal effect of gender differences on firm performance, the paper utilizes different model specifications. First, ordinary least squares (OLS) with fixed effects is performed. Second, I employ propensity score matching, followed by sensitivity to unobservables using Oster (2017). In addition, several robustness checks have been carried out along with heterogeneity analysis.

2.3.1 OLS Framework (with Fixed Effects)

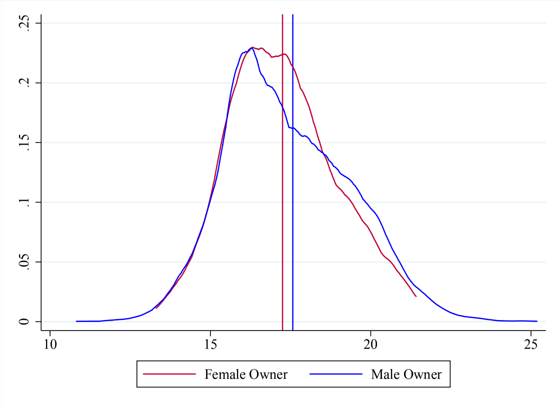
The OLS specification with fixed effects is modeled as follows:

$$Y_{ijs} = \beta_0 + \beta_1 FemaleOwner_i + \beta_2 FemaleCEO_i + \gamma_i X_i + \alpha_s + \alpha_j + \alpha_s \times \alpha_j + u_{ijs} \quad (2.1)$$

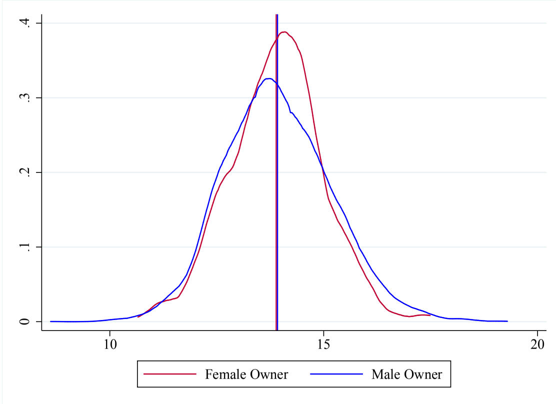
The dependent variable Y_{ijs} is the performance-outcome indicator for firm i, in industry j, located in state s. I estimate this separately for three measures of firm– performance outcomes– sales, labour productivity, and exports. The sales variable is measured as the log of the total sales in rupees in 2014. Labour productivity is measured as the log of the ratio of total sales to total number of full-time paid workers. Lastly, export is an indicator variable for firms involved in direct and indirect exports of goods.

The binary variable $FemaleOwner_i$ and $FemaleCEO_i$ takes on the value 1 signifies whether the CEO/owner is a woman in the enterprise and 0 otherwise. The coefficients of interest are β_1 and β_2, which capture the differences in responsibility of female owners and leaders compared to male owners and leaders respectively. X_i includes a set of control

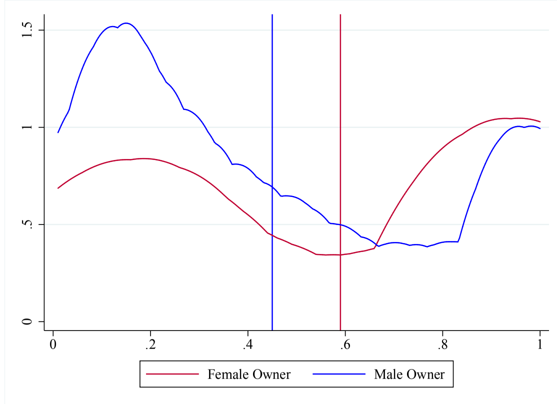
Top panel: Gender of owner



(a) Sales (log)

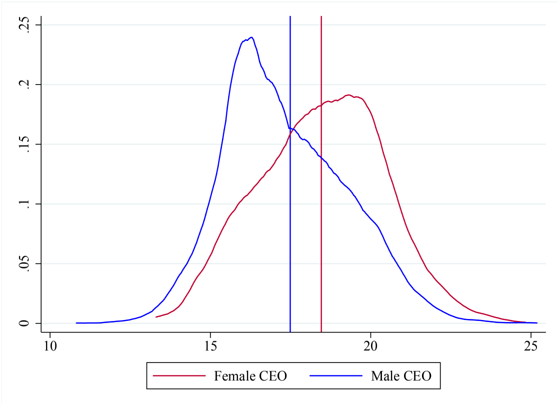


(b) Labour Productivity (log)

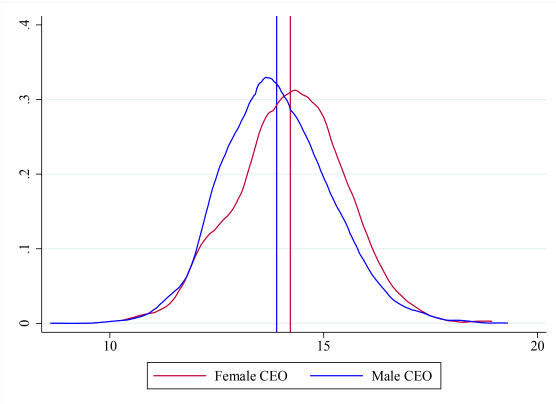


(c) Exports

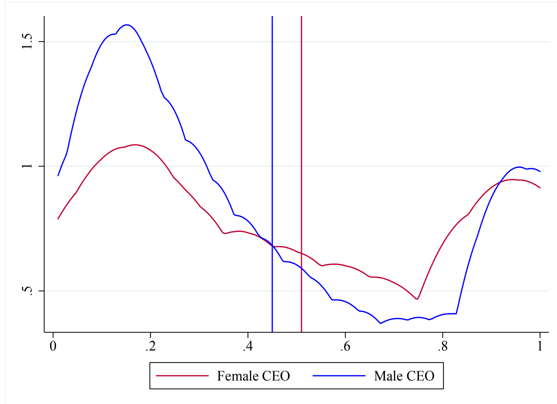
Bottom panel: Gender of CEOs



(d) Sales (log)



(e) Labour Productivity (log)



(f) Exports

Figure 2.2: Kernel density estimation of firm performance measures by Gender.

Notes: Vertical bars are the averages for the respective type of firms outcome.

variables firm age, size, foreign ownership, access to credit, multiplant firm, internet access, CEO experience, female hiring constraint, workers formal training, worker's education–secondary and bachelor's as this could affect the firms' performance. α_s and α_j are state and industry dummies to allow for varying state and industry effects while u_{ijs} captures the error term. Further, geography plays a crucial role in determining growth (Acemoglu et al., 2001, Nordhaus, 2006). Per the geographical classification of Topalova and Khandelwal (2011), Indian states are quite heterogeneous. The coastal states experience more sales, increased labour productivity, and more exports because of easy access to nearby ports and the ease and lower cost of transporting their products in bulk (Gallup et al., 1999). $\alpha_s \times \alpha_j$, is the state-specific industry fixed effects accounting for any changes in the industries over states. All the standard errors are clustered at the industry level to account for the possible correlation of shocks within industries. Results are reported in Section 2.4.1.

2.3.2 Selection Effect: Propensity Score Matching

A potential source of bias in the estimates of Eq. (2.1) can arise as women work with the firms of their choice. They might prefer a firm with certain characteristics—such as industry, size, and whether it has branches in other places—that might affect the outcome variable. Not accounting for this might result in either underestimation or overestimation of the true effect. I address this issue by using the propensity score matching (Henceforth, PSM) approach introduced by Rosenbaum and Rubin (1983).

The idea of matching is to compare the means of the treated group and the control group sharing similar features to uncover the true effect of gender on firm performance. The crucial assumption is that both groups differ only in their observable features. This can be expressed as $(Y_{1i}^F, Y_{0i}^M) \perp D_i | X_i$, where $\perp$ represents independence, Y_{1i}^F is the outcome of the treated group (females), and Y_{0i}^M is the outcome of the nontreated group (males). D_i is the indicator of the treatment group, and X_i is a set of covariates. The PSM is the probability assignment of treatment conditional on pretreatment observables, estimated as follows:

$$P(X_i) = Pr(D_i = 1 | X_i) \tag{2.2}$$

The treatment–effect estimation is based on satisfying two conditions: the conditional independence assumption (CIA) and the common support condition. First, under the CIA, the treatment variable should be independent of the outcome variable after conditioning on a set of observable covariates. Second, the observation of the treatment should lie within the common support region to satisfy the common support condition (Leuven and Sianesi, 2003).[3] Therefore, as Rosenbaum and Rubin (1983) argue, all biases arising from observed covariates can be eliminated by satisfying both conditions.

After computing the propensity score, the average treatment effect on the treated (ATT) can be estimated as follows:

$$ATT = \mathrm{E}\left[\mathrm{E}\left[Y_{1i}^{F} \mid D_i = 1, P(X_i)\right] - \mathrm{E}\left[Y_{01}^{M} \mid D_i = 0, P(X_i) \mid D_i = 1\right]\right] \tag{2.3}$$

This study uses the nearest–neighbor matching technique (Caliendo and Kopeinig, 2008). To avoid bad matches, I use without–replacement matching. Furthermore, balancing the observable covariates between the treated group and the control group is of prior importance (Heinrich et al., 2010). In addition, I conduct a post-estimation test to check the quality of PSM matches before and after the treatment. The findings from this section is discussed in Section 2.4.2.

2.3.3 Sensitivity to Unobservable/Omitted Covariates

I test the sensitivity of gender differences to the unobserved variables (such as the characteristics or personality traits of the female owner and CEO) by employing methods developed by Oster (2017). Mathematically, this method is explained as follows:

$$Y_{ijs} = \beta Female_i + Z_{1i} + Z_{2i} + \varepsilon_{ijs}, \tag{2.4}$$

For the linear combination shown in Eq. (2.4), the observed control variables are captured by Z_{1i} and that the unobserved confounding elements are captured in Z_{2i}, which is correlated with the outcome variable Y_{ijs}. The indicator variable $Female_i$ denotes female ownership,

[3] The propensity score should lie between 0 and 1.

and ε_{ijs} captures the error term.

The approach relies on the assumption of equal proportionality between the observed (Z_{1i}) and unobserved (Z_{2i}) covariates as shown in Eq. (2.5),

$$\frac{\text{Cov}(Female_i, Z_{2i})}{\text{Var}(Z_{2i})} = \delta \frac{\text{Cov}(Female_i, Z_{1i})}{\text{Var}(Z_{1i})} \tag{2.5}$$

where δ captures the degree of proportionality. $\delta = 1$ signifies that observed and unobserved factors are of equal importance while $\delta > 1$ signifies a greater impact of unobservables than observables on the outcome.

Let $\tilde{\beta}$, $\tilde{R}$ be the coefficient and R^2 of the regression using the observed covariates (Z_{1i}), and let $(\dot{\beta}, \dot{R})$ be hypothetical values for the regression including the unobserved variables (Z_{2i}). R_{max}, which is the R^2 of the model presented in Eq. (2.4), cannot be determined as it depends on the unobservable covariates (Z_{2i}) and the idiosyncratic variation in the firm performance captured by the error term (ε_{ijs}). Further, Oster (2017) suggests that $R_{max} = \pi\tilde{R}$, where the bounding value of $\pi = 1.3$. The intuition behind this assumption is that the observables are selected with care and explain a significant portion of the variation in the dependent variable. Hence these observables are the key covariates in explaining the variation in firm performance.

Oster (2017) proposes a bias-adjustment coefficient β^* in Eq. (2.6) that helps us to provide an insight on the sensitivity of the unobserved covariates.

$$\beta^* = \tilde{\beta} - \tilde{\delta} \frac{(\dot{\beta} - \tilde{\beta})(R_{max} - \tilde{R})}{\tilde{R} - \dot{R}} \tag{2.6}$$

I will describe the results of this method in the following Section 2.4.3.

2.4 Results

2.4.1 OLS Estimates

Table 2.2 reports the results from different versions (Columns 1–3) of Eq. (2.1). Column 1 has no covariates. Column 2 adds covariates. Column 3, includes covariates, state fixed effects and industry–level fixed effects. Column 4 incorporates state–specific industries, which estimates Eq. (2.1).

Panel A of Table 2.2 presents the gender gap estimates for the the log of sales. The striking feature is that in every specification, other things equal, female ownership is associated with lower sales (Columns 1-4). The magnitude of the coefficient varies from -0.47 to -0.12. A coefficient magnitude of -0.16 (Column 4) implies that a female-owned firm experiences 15.45 percent lower sales in a year than a male-owned firm.[4] In contrast, female leadership has a significant positive association with the log of sales, and a female-led firm experiences an increase in sales of 20.68 (Column 4). Thus, a firm's sales performance is significantly and negatively impacted when a female owns the firm, while it has a significant positive association with the presence of a female CEO.

The primary results are similar when I examine labour productivity and gender. Panel B of Table 2.2 shows the the effect of a firm having a female owner or female CEO on labour productivity. Female ownership has an insignificant negative association with the productivity of labour across all specifications except in column 4, in which state and industry fixed effect is controlled. By contrast, if a firm has a female CEO, labour productivity goes up in all specifications, which vary between 9.30 and 38.40 percent higher. Thus, a 1 percent increase in the female–CEO variable increases labour productivity by 14.45 percent while holding other predictors constant.

Panel C of Table 2.2 reports the estimates on the effect of gender differences on export share and shows no significant association between female ownership and exports (Column 4). However, an increase in the female–CEO variable has a significant positive impact ranging from 2 to 8 percent (Columns 1–4).

Hence, female ownership has a significant negative association with sales (Panel A, Column 4) but no significant association with labour productivity (Panel B, Column 4) and exports (Panel C, Column 4), but having women in the CEO position significantly increases sales, labour productivity, and exports respectively (Column 4).

[4] Calculated effects as (exp(beta)-1)*100 [percent].

Table 2.2: Firm Performance and Gender Inequality

	(1)	(2)	(3)	(4)
		Panel A: Sales (log)		
Female Owner	-0.475***	-0.124*	-0.191***	-0.168**
	(0.101)	(0.075)	(0.059)	(0.062)
Female CEO	1.010***	0.255***	0.218***	0.188***
	(0.077)	(0.057)	(0.060)	(0.058)
Control Variables	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
State FE	No	No	Yes	Yes
State FE × Industry FE	No	No	No	Yes
Observations	8,946	8,928	8,928	8,928
		Panel B: Labour Productivity (log)		
Female Owner	-0.078	-0.072	-0.127*	-0.078
	(0.067)	(0.065)	(0.064)	(0.066)
Female CEO	0.325***	0.145***	0.135***	0.089**
	(0.051)	(0.049)	(0.048)	(0.049)
Control Variables	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
State FE	No	No	Yes	Yes
State FE × Industry FE	No	No	No	Yes
Observations	8,941	8,923	8,923	8,923
		Panel C: Exports		
Female Owner	0.021*	0.033***	0.017	0.015
	(0.011)	(0.011)	(0.012)	(0.012)
Female CEO	0.085***	0.058***	0.034**	0.023**
	(0.008)	(0.008)	(0.013)	(0.013)
Control Variables	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
State FE	No	No	Yes	Yes
State FE × Industry FE	No	No	No	Yes
Observations	9,261	9,240	9,240	9,240

Notes: Clustered robust standard errors (at the industry level) are enclosed in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

2.4.2 Propensity Score Matching

Propensity scores[5] are matched using one to one nearest-neighbor matching technique. Matching is done without replacement with a common support and a radius caliper of 1 percent. After removing the bias, ATT reports the difference in mean outcomes in the gender group and the matched control group in Table 2.3.

Table 2.3: Accounting for Selection Bias Using Nearest-Neighbor Matching

Outcome	ATT	Number of Treated	Number of Controls
	(1)	(2)	(3)
Panel A: Female Ownership			
Sales (log)	-0.093	377	8,559
	(0.132)		
Labour Productivity (log)	-0.001	377	8,554
	(0.086)		
Exports	0.030*	394	8,858
	(0.018)		
Panel B: Female CEO			
Sales (log)	0.135***	681	8,228
	(0.107)		
Labour Productivity (log)	0.075***	681	8,223
	(0.070)		
Exports	0.019***	726	8,498
	(0.016)		

Notes: Clustered robust standard errors (at the industry level) are enclosed in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Panel A of Table 2.3 shows the estimates for female ownership (Column 1) while panel B reports estimates for female leadership. Columns 2 and 3 report the treated and controlled observations. The impact of female ownership is in the same direction as that in the baseline results. Further, the presence of female owners significantly increases the exports of firms (3 percent); however, their presence does not have a significant effect on sales or labour productivity (Column 1).

Unlike the effect of female ownership, female leadership has a positive influence on

[5] Propensity scores are estimated using logit regressions.

firms' performance and is in line with the baseline direction. After applying the nearest-neighbor matching technique, I find that having a female leader is significantly associated with the various measures of firm performance. The presence of a female CEO increases sales, labour productivity, and exports by 13.5, 7.5, and 1.5 percent respectively (Table 2.3, Panel B, Column 1).

The bias-reduction graphs in Figure 2.3 present female ownership in the top panel and female leadership in the bottom panel. The vertical line indicates no standardized bias. Before PSM (indicated by dots), the control variables lie far from the vertical line, indicating bias. After PSM (shown by the cross mark), bias drops substantially and we can see the covariates lie closer to the vertical line. This validates the propensity score matching graphically.

The purpose of using PSM is to obtain balanced covariates between the treatment group and the control group. Table 2.4 presents the postcredibility results of using PSM. Panel A presents the results from balanced covariates before and after matching for the female ownership, and panel B reports the case of female leadership.

The pseudo-R^2 significantly drops from the range 12 to 6 percent before matching to 0.1 percent after matching. Before matching, the standardized mean difference of the observables used in propensity score was 8 to 13 percent, while the figure was less than 1 percent after matching.[6] This considerably reduces the total bias through matching by 68 to 92 percent (Table 2.4). The joint significance of the observed covariates was always significant before matching (see the *p*-value of the likelihood ratio tests) but was rejected every time after matching. Interestingly, after matching low pseudo-R^2, high total bias reduction, and low mean standardized bias, the insignificant *p*-values of the likelihood ratio test confirm the credibility of the suggested specification of PSM in terms of balancing the covariates between the two groups.

[6] The covariate balancing tests are obtained using pstest and psmatch2 commands in STATA14 (Leuven and Sianesi, 2003).

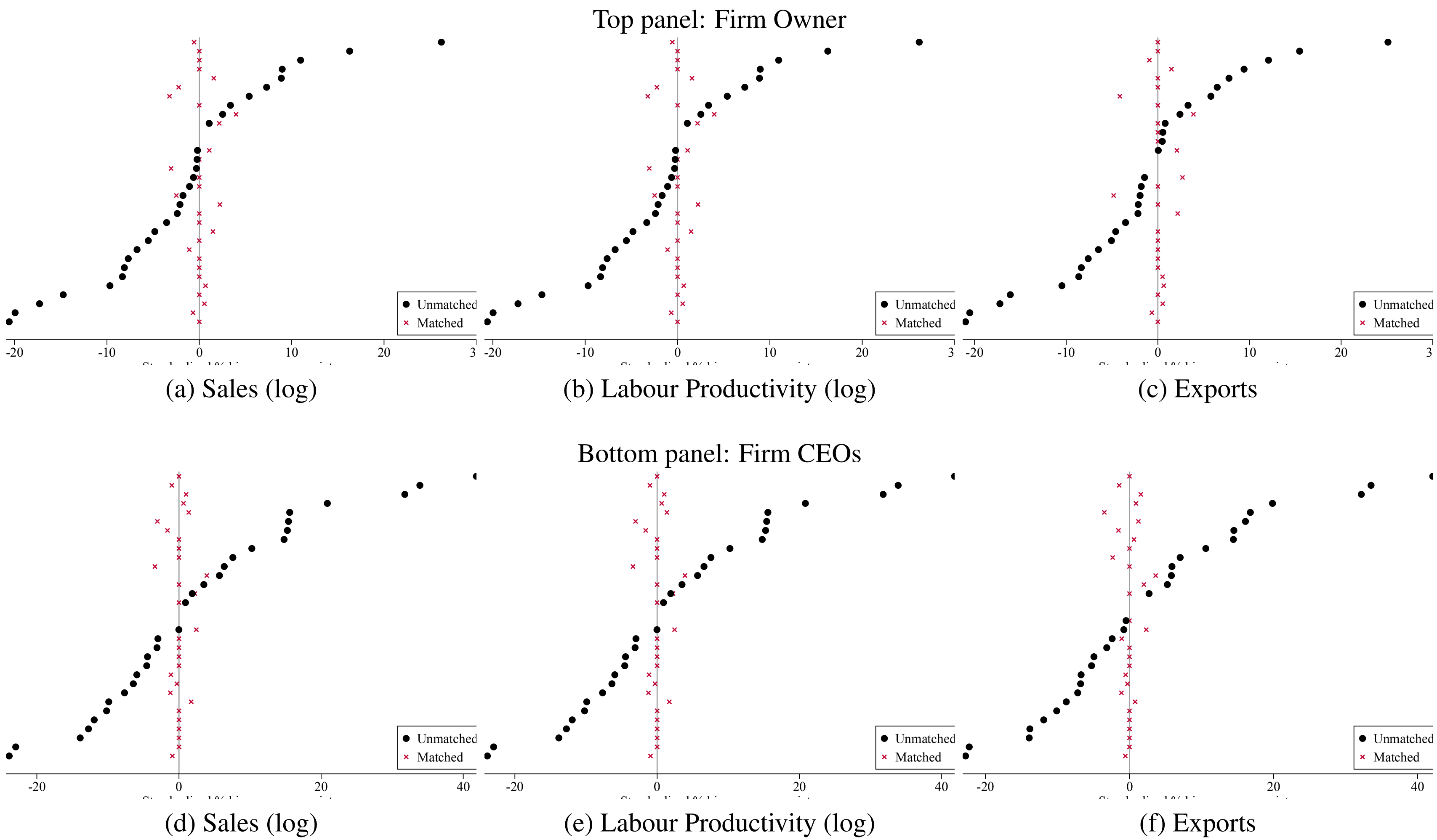


Figure 2.3: Bias Reduction Graph

Table 2.4: PSM Credibility Indicators before and after Matching

Outcome Variable	Pseudo R^2 before matching	Pseudo R^2 after matching	LR χ^2 (p-value) before matching	LR χ^2 (p-value) after matching	Mean standardized bias before matching	Mean standardized bias after matching	(Total) % | bias | reduction
Panel A: Treatment effect - Female Ownership							
Sales (log)	0.066	0.001	207.18 (p= 0.00) ***	1.34 (p= 1.00)	8.7	0.7	68.2
Labour Productivity (log)	0.066	0.001	206.94 (p= 0.00) ***	1.35 (p= 1.00)	8.7	0.7	68.1
Exports	0.066	0.001	213.71 (p= 0.00) ***	1.03 (p= 1.00)	8.6	0.5	69.1
Panel B: Treatment effect - Female CEO							
Sales (log)	0.119	0.001	573.68 (p= 0.00) ***	1.84 (p= 1.00)	12.5	0.8	91.5
Labour Productivity (log)	0.119	0.001	573.33 (p= 0.00) ***	1.69 (p= 1.00)	12.5	0.8	91.8
Exports	0.121	0.001	617.29 (p= 0.00) ***	1.88 (p= 1.00)	12.4	0.8	92.4

Notes: Nearest-neighbor matching technique was used without replacement, with common support, and with radius of 0.01 caliper.

2.4.3 Checks on Sensitivity to Unobservables

A first-order concern about unobservables led me to conduct a preliminary test using methods developed by Oster (2017). Table 2.5 reports the sensitivity analysis of the unobserved covariates. δ captures the degree of proportion between observed and unobserved covariates (Column 3). The corrected estimates are predicted by taking observed covariates, unobserved covariates, the degree of proportion, and R_{max} into account (Column 4).

Table 2.5: Accounting for Omitted Variable Bias

Variables	Baseline Effect $\dot{\beta},(S.E),[\dot{R}]$	Controlled Effect $\tilde{\beta},(S.E),[\tilde{R}]$	Delta (δ)	Corrected Effect β^*
	(1)	(2)	(3)	(4)
Panel A: Sales (log)				
Female Owner	-0.474***	-0.167**	1.315	-0.129
	(0.101)[0.020]	(0.062)[0.604]		
Female CEO	1.010***	0.188***	0.292	0.145
	(0.077)[0.020]	(0.058)[0.604]		
Panel B: Labour Productivity (log)				
Female Owner	-0.078	-0.078	0.808	-0.081
	(0.067)[0.005]	(0.059)[0.325]		
Female CEO	0.325***	0.088*	0.145	0.079
	(0.051)[0.005]	(0.047)[0.325]		
Panel C: Exports				
Female Owner	0.021*	0.015	0.412	0.015
	(0 .011) [0.012]	(0.012)[0.364]		
Female CEO	0.084***	0.023*	0.166	0.022
	(0 .009) [0.012]	(0.013)[0.364]		

Notes: Results for the baseline effect in the uncontrolled model using OLS regression are reported in Column 1. Results of the controlled model are obtained by including all the controlled variables from the OLS regression using the state and industry interaction terms (Column 2). Results in Column 3 are estimated using Eq. (4), and column 4 reports the results from Eq. (5). A STATA command "PSACALC" is used following Oster (2017) to report the estimations in columns 3 and 4. Clustered robust standard errors (at the industry level) are enclosed in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Panel A of Table 2.5 reports the impact of female ownership (Column 2), and the corrected estimate is in the same direction (Column 4). The impact of female leadership on log sales is positive (Column 2), and the true estimate is in the same direction (Column 4). More specifically, the difference between the controlled effect and the corrected effect is less. Further, the bias direction of female ownership and leadership is the same for both log labour productivity (Panel B, Column 4) and exports (Panel C, Column 4). The difference in bias estimate after the controlled effect (Column 2) and corrected effect (Column 4) is almost negligible. The sensitivity check rules out bias running from unobservable covariates to the estimates.

2.4.4 Other Drivers

The important thing is that bribery and innovation may affect firm performance and also influence the choice of firms by firms. In the following, two drivers of firm performance that are not included in the baseline estimates are controlled for.

A. Innovation

A primary driver of firm performance is innovation. Innovative firms typically have higher sales and exports (Gunday et al., 2011). Innovation is an indicator variable taking the value 1 for innovation.[7] Innovation has a significant positive association with firm performance: a 1 percent increase in the innovation indicator leads to an increase in firm outcome varying between three and twenty-seven percentage points (Table 2.6, Columns 1-9). Moreover, the key results concerning the effect of female ownership and leadership on firm performance are robust.

B. Bribery

The impact of corruption on firm performance is unclear. Firms paying bribes incur an additional cost (Persson et al., 2003) but at the same time may gain favorable treatment (e.g., quicker processing of administrative procedures, more lenient application of customs

[7] Firms were asked to indicate "formal engagement in research and development activities, either in-house or contracted with the companies" and whether they have "introduced any new and significant products or services" in the last three years.

Table 2.6: Firm Performance and Gender Inequality

	Sales (log)			Labour Productivity (log)			Exports		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Female Owner	-0.171***	-0.167**	-0.170**	-0.080	-0.078	-0.079	0.015	0.015	0.015
	(0.060)	(0.063)	(0.061)	(0.066)	(0.067)	(0.066)	(0.012)	(0.012)	(0.012)
Female CEO	0.167***	0.190***	0.169***	0.078	0.090*	0.079	0.021*	0.023*	0.021*
	(0.054)	(0.058)	(0.053)	(0.047)	(0.049)	(0.047)	(0.012)	(0.013)	(0.012)
Innovation	0.274***		0.278***	0.141***		0.143***	0.030***		0.031***
	(0.050)		(0.050)	(0.047)		(0.047)	(0.009)		(0.009)
Bribe		-0.124**	-0.140**		-0.058	-0.067		-0.000	-0.002
		(0.055)	(0.058)		(0.056)	(0.058)		(0.007)	(0.007)
Observations	8928	8928	8928	8923	8923	8923	9240	9240	9240

Notes: All the regressions include control variables and state-industry fixed effects. Clustered robust standard errors (at the industry level) are enclosed in parentheses.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

regulations or import tariffs). Women are more moral and are less corrupt (Breen et al., 2017). As there is a large shadow economy in India (Chaudhuri et al., 2006), it is important to know which effect holds true for Indian firms. Bribery in this study is defined as a binary variable when firms give bribes.[8]

Table 2.6 confirms that bribery has a negative impact on the performance of the firms. It is seen that as bribery increases, the performance of the firm decreases. Female ownership shows a significant dampening effect on sales (Table 2.6, Columns 1-3) while the presence of a female CEO shows a positive association with a firm's performance (Table 2.6, Columns 1-9).

The results and various robustness checks lead me to a three-part interpretation. First, owners do not participate in the day-to-day operation of the business, but their approval is required for major decisions, such as whether the firm should export. Second, I observe that female leadership is more positively associated with labour productivity than is female ownership, which is in line with the literature (Aterido and Hallward-Driemeier, 2011). Third, the negative association of log sales and female ownership might be due to gender discrimination in access to finance (Aterido et al., 2013, Bardasi et al., 2011, Asiedu et al., 2013), which directly affects sales. An increase in access to finance would help firms to invest more in technology to innovate their products and increase sales (Bruhn et al., 2010).

2.4.5 Heterogeneity Analysis

India being a patriarchal society (Derné, 1995), it is not suprising that 96 percent of Indian firms are owned by males, as is evident in the dataset. India is a large country with a very diverse culture. As culture influence gender norms and behavior, the results may differ across different regions. I therefore investigate the heterogeneity along these lines. This will help us to get a clearer picture of the prevailing social norms in a male-dominated society.

[8] Firms were asked "if the firms are expected to give gifts or informal payments to the government official to get things done smoothly."

A. Geographical Variation

Geographical variation across Indian states is present in several dimensions of human development (Dyson and Moore, 1983, Sen, 1992). One such crucial dimension is the population sex ratio, where a son is preferred over a daughter (Arnold et al., 1998) in India.

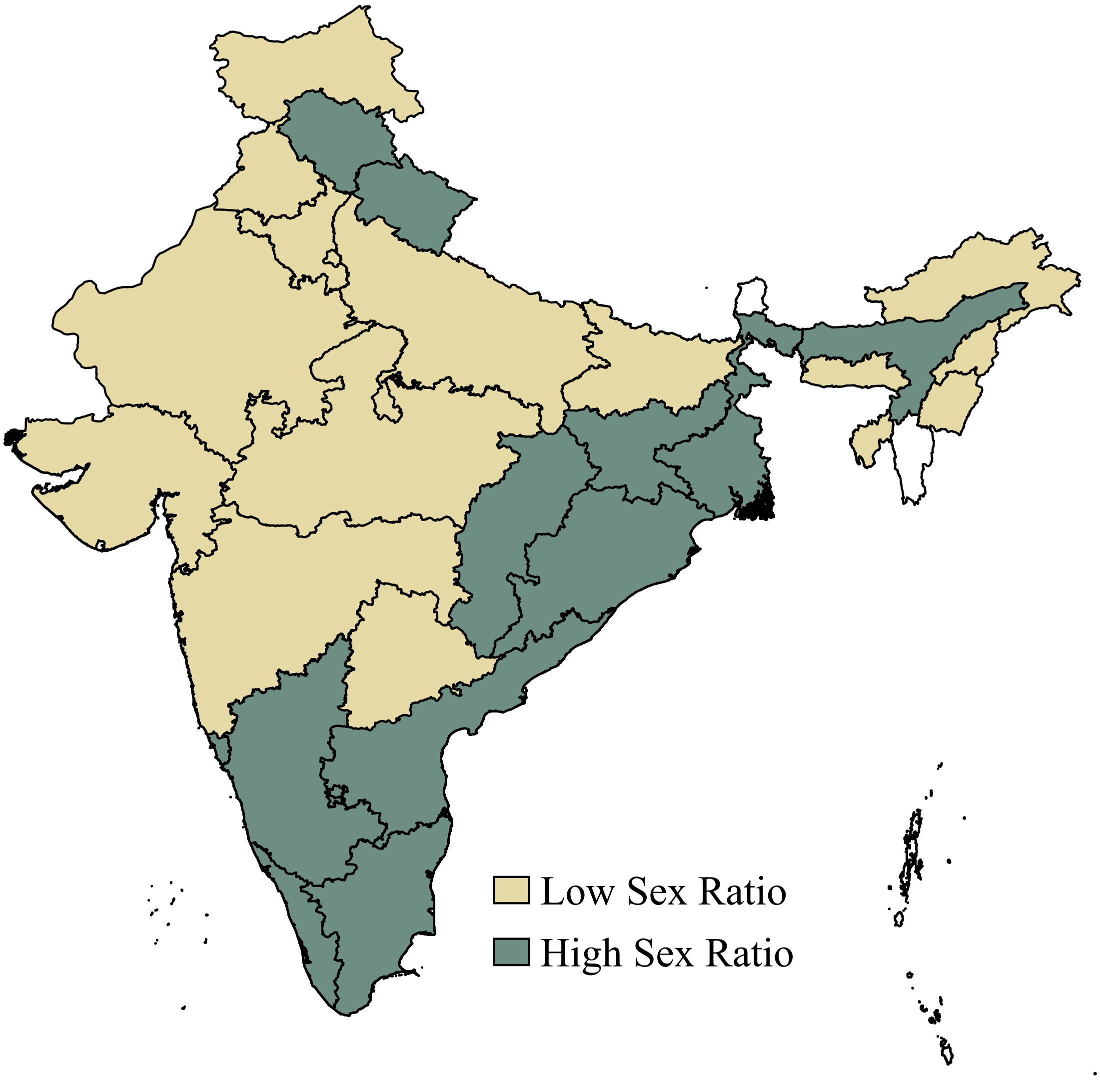


Figure 2.4: Variation in States by Sex Ratio

Notes: A state is said to have a low sex ratio (number of females per 1,000 males) if the average is less than the national average (in the 2011 census). A high sex ratio indicates the opposite. Telangana was a part of Andhra Pradesh till June 2014.

Sex ratio is measured as the number of females per one thousand males (Census 2011); it varies across the states. Population sex ratio is used to divide states into two parts. States are categorized as having a low sex ratio when the ratio is less than the national mean—in other words, where discrimination is higher—and as having a high sex ratio when discrimination is lower. Figure 2.4 presents the geographical variation (by state) in proportion of women. The corresponding regression estimates are presented in Table 2.7.

Table 2.7: Effect on Proportion of Women on Firm Performance

Outcome	Baseline	Low Sex Ratio States	High Sex Ratio States
	(1)	(2)	(3)
	Panel A: Sales (log)		
Female Owner	-0.168**	-0.178**	-0.142
	(0.062)	(0.067)	(0.089)
Female CEO	0.188***	0.108	0.262***
	(0.058)	(0.082)	(0.091)
Observations	8928	4513	4415
	Panel B: Labour Productivity (log)		
Female Owner	-0.078	-0.063	-0.092
	(0.066)	(0.058)	(0.095)
Female CEO	0.088**	0.002	0.167**
	(0.049)	(0.063)	(0.086)
Observations	8923	4512	4411
	Panel C: Exports		
Female Owner	0.015	0.022	0.007
	(0.011)	(0.019)	(0.012)
Female CEO	0.023**	0.016	0.028**
	(0.012)	(0.017)	(0.017)
Observations	9240	4726	4514

Notes: All the regressions include control variables and state-industry fixed effects. Clustered robust standard errors (at the industry level) are enclosed in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

The results show that the impact of female ownership on sales (Table 2.7, Panel A) in more biased states (Column 2) is significantly negative and insignificant in less biased states. Further, no significant evidence is found in states with a low sex ratio (Column 2).

However, as the sex ratio across the states moves above the median (i.e., high sex ratio), the magnitude of the female–CEO variable increases significantly. This means the average effect of female leadership (Column 1) is driven by less biased states (Column 3).

B. Progressive and Conservative States

Indian states can also be categorized as progressive or conservative, depending on the presence or absence of progressive state laws. One law is the Hindu Succession Act (1956), which specified that the daughters of a Hindu male[9] had rights to ancestral property till they were married. Once married, they had no rights to the property. Certain states swiftly amended the law to improve women's rights before a national law was passed in 2005.[10] I call these states progressive and other states conservative, as shown in Figure 2.5.

[9] This governed not only Hindus but also Buddhists, Sikhs, and Jains.
[10] The national law gave inheritance rights to the daughter irrespective of her marital status.

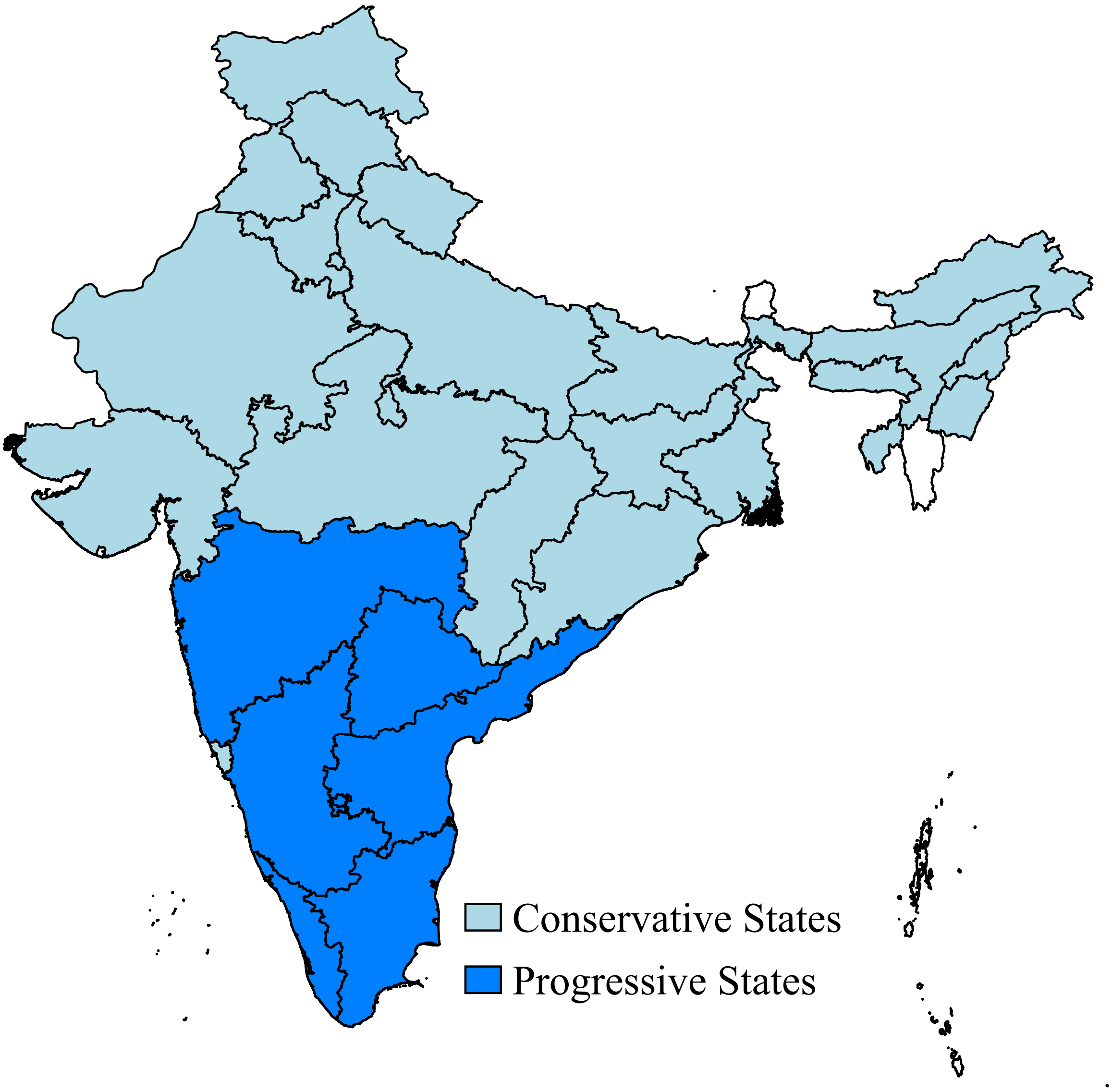


Figure 2.5: Progressive and Conservative States

Notes: Progressive State indicates a state implemented progressive policies (with respect to the Hindu Succession Act); Conservative State indicates other states. Telangana was a part of Andhra Pradesh till June 2014.

A pattern similar to that in Table 2.7 can be found in Table 2.8. The coefficients do not show any clear-cut pattern for female ownership. Contrary, having a female CEO does have a positive impact on firm performance in progressive states (Column 3). In other words, a significant and positive association between female leadership and firm performance is driven by the progressive-states (Heath and Tan, 2014).

Table 2.8: Conservative vs. Progressive States

Outcome	Baseline	Conservative States	Progressive States
	(1)	(2)	(3)
Panel A: Sales (log)			
Female Owner	-0.167**	-0.131**	-0.247
	(0.062)	(0.057)	(0.162)
Female CEO	0.188***	0.098	0.347 ***
	(0.058)	(0.066)	(0.092)
Observations	8928	6223	2705
Panel B: Labour Productivity (log)			
Female Owner	-0.078	-0.071	-0.077
	(0.066)	(0.058)	(0.178)
Female CEO	0.088**	0.023	0.196**
	(0.049)	(0.055)	(0.088)
Observations	8923	6219	2704
Panel C: Exports			
Female Owner	0.015	0.009	0.040
	(0.011)	(0.012)	(0.033)
Female CEO	0.023**	0.016	0.033**
	(0.012)	(0.014)	(0.022)
Observations	9240	6467	2773

Notes: All the regressions include control variables and state-industry fixed effects. Clustered robust standard errors (at the industry level) are enclosed in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

2.5 Conclusion

This paper contributes to the literature on the impact of gender differences and firm performance in India. Specifically, the study seek to understand the gender diversity in ownership and leadership (CEO) on firm performance by using large firm–level data–set from World Bank Enterprise Survey.

Overall, the evidence above indicates no definite pattern for female ownership and firm performance. Focusing on female CEO, this paper demonstrates a positive and significant association with firm-performance outcomes (sales, labour productivity, and exports). Under–representation of women in the CEO position is associated with a high cost (Flabbi et al., 2019). Because of discrimination, women typically lack the interest in attaining positions of power, which might also explain low female representation in the leadership position (Schuh et al., 2014). Investigating the mechanism behind these findings, showed that the result for female CEOs is driven by progressive and less-biased state in terms low–sex ratio in India.

Equality in a civil society is a fundamental right and draws key attention to the requirement of gender policies (Duflo, 2012). Evidence from this study supports calls for relevant inclusive policies in the workplace to provide women with opportunities to break the glass ceiling in firm leadership. Moreover, policies should incentivize more women to start an enterprise to break free of the prejudiced mindset as it is hindering the growth and development of the nation.

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Chapter 3

The Impact of Alcohol Bans on Crime in India

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By signing the Statement of Authorship, each author certifies that:

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Abstract

This paper examines the impact of alcohol bans on crime in India. The analysis exploits the variation in alcohol prohibition imposed across districts between 1972 to 2016. The quasi–experimental structure of the policy facilitates identification, using the causal impact of the policy on crime outcomes. I use the largest-to-date annually archived administrative dataset on district crime statistics. Results uncover that bans on liquor in districts led to a significant reduction in criminal activities, particularly for violent crimes, crimes against women, and property crimes. A plausible mechanism is that crimes are primarily perpetrated by individuals who suffer from alcoholism. The effect is most pronounced in high–poverty states in India.

Key Words: alcohol bans, violent crime, property crime, crime against women

JEL Codes: K42, K23, O20

3.1 Introduction

Criminal activities impose a huge cost on society, both tangible and invisible (Miller, 1996, Cohen et al., 2004, McCollister et al., 2010). For instance, the cost associated with violent and property crime accounts for $426 billion and $24 billion annually in the United States (Miller, 1996). One potential determinant that has received substantial attention from psychologists suggest that alcohol consumption could spur criminal activities.[1] The association between drinking and crime is well–documented (Carpenter, 2005, 2007, Carpenter and Dobkin, 2010, 2015). Lindo et al. (2018) highlight that drinking at college parties increases crimes against women. Alcohol regulation has often been used as a crime policy. The United States was one of the first countries to embark on an anti–alcohol movement, from 1920 to 1933 during its Prohibition Era. The historical literature suggests that Prohibition led to a short–term drop in crime (Warburton et al., 1932, Miron and Zwiebel, 1991, Dills et al., 2005) as people viewed alcohol prohibition as a price increase (Cook, 2007).

Globally, alcohol is consumed in various forms across countries. According to the WHO (2019), spirits are the most commonly consumed alcohol globally (44.8 percent), followed by beer (34 percent), wine (11.7 percent), and others (9.3 percent), respectively. Spirits make up the overwhelming share of alcohol consumption in South-East Asia (87.9 percent), where India accounts for the highest share. Alcohol per capita consumption in India has increased to 5.7 liters, consisting of 92 percent spirits, 8 percent beer, and less than 1 percent wine (WHO, 2019). More importantly, alcohol laws in India have greatly varied over time, more so than in Canada or the United States, so the data provides adequate identifying variation. Also, the impact of alcohol bans on crime in developing countries is relatively under–explored.

This paper investigates this question by exploiting a quasi–experimental setting to analyze the impact of alcohol bans on criminal activities in India. The geographical variation in alcohol prohibition across India's districts provides an ideal environment for

[1] For evidence, see Steele and Josephs (1990), Parsons and Farr (1981), Evert and Oscar-Berman (1995), McCrady and Smith (1986), Tarter (1973), Ratti et al. (1999), Fein et al. (1990), Wechsler (1941), Hill and Mikhael (1979), Fillmore et al. (1998), Mitchell (1985) and Wilson et al. (1984).

this study. This policy was never introduced in reaction to crime rates but for several other reasons, including westernization and religious belief (for more see Section 3.2), and it was repealed due to a shortage in state revenues. Accordingly, I study the potential heterogeneous effects across states to trace the mechanism. Previous paper by Luca et al. (2015) has analyzed the impact of alcohol bans on crime against women but suffered from more refined data. I revisit the problem and improve on design, by using the timing of district–level alcohol bans and employing more micro–level data. Furthermore, these studies focused on state–level alcohol bans and not at the district–level alcohol bans.

To estimate the effect of alcohol bans on crime in India, a difference–in–differences (DD) approach is adopted. In this framework, the treated districts are those where alcohol bans were imposed and all other districts act as control units. To uncover the link between alcohol prohibition and crime, I use district–level crime data across an annual panel of 324 districts for 45 years during the period 1972 to 2016, an improvement over existing studies. Along with district fixed effects, year fixed effects and district-specific time trends are employed to account for any potential changes over time.

The estimates show that the enactment of the alcohol ban policy led to a substantial reduction in criminal activities. Specifically, the ban reduced the rate of murders and kidnappings. Results also show a fall in crimes against women: rape (7 percent of the mean), assault (13 percent of the mean), and cruelty (22 percent of the mean). In addition, it led to a drop in property crimes, such as theft (13 percent of the mean), robbery (18 percent of the mean), armed robbery (19 percent of the mean), burglary (8 percent of the mean), and rioting (14 percent of the mean). The results pass the placebo check, which signals that alcohol bans helped to reduce crime in India.

In terms of mechanisms, the results suggest that these effects are driven mainly by high–poverty states in India. Alcoholism is more prevalent among societies with low income in India (Schilbach, 2019). Heavy alcohol consumption leads to intoxication, which in turn causes impairment to many cognitive abilities and may increase a person's propensity to engage in crime (Steele and Josephs, 1990, Evert and Oscar-Berman, 1995, Fillmore et al., 1998). Thus the prohibition of alcohol reduced violent crime across districts in India.

Interestingly, the reason behind the fall in property crimes due to alcohol bans is different. Property crimes may be committed to acquire money to purchase alcohol, so eliminating this particular motive may reduce property–related crime.

Prior studies on India looked at alcohol prohibition using data at the state level (Luca et al., 2015). Evidence suggests that alcohol bans led to declines in violent and non-violent crime in Bihar (Chaudhuri et al., 2018). Luca et al. (2019) provides evidence that a higher minimum legal drinking age reduces vehicle crimes. However, Dar and Sahay (2018) reveals that the alcohol ban in Bihar led to an increase in crime. The past evidence on alcohol prohibition and crime is inconclusive. Furthermore, the existing literature looks at alcohol bans across states and did not consider a more disintegrated level.

Against this backdrop, this paper contributes to the literature by investigating the impact of alcohol bans across districts on crime. Second, this paper employs the largest–to–date available crime dataset in India to study the causal associations. This setting allows me to estimate the long–term average effects of alcohol bans on crime. Third, I extend the work of Kumar and Somanathan (2009) in mapping districts' boundaries to their parent states from 2001 onward. Importantly, studies by Luca et al. (2015), Khurana and Mahajan (2019) suggested no effect of alcohol bans on rape in India. However, my findings show otherwise: they establish that alcohol bans can reduce rapes. Lastly, this paper helps to shed more light on alcohol prohibition policies in societies like India, where alcoholism is more prevalent among the poor, making it relevant to policymakers.

The rest of the paper proceeds as follows. Section 3.2 provides the policy background in India and the variation in alcohol bans across districts. Section 3.3 describes the administrative data. Section 3.4 presents the econometric model for this study. Section 3.5 lays out the empirical estimates, followed by robustness checks. Section 3.6 digs into the mechanisms and then Section 3.7 concludes the paper.

3.2 Background and Setting

The colonization period significantly introduced India to alcohol drinking, fermentation, and distillation. This made alcohol a commercial product and a central part of Indian culture (Saxena, 1999). Prior to 1949, the government of India had complete control over alcohol policies. In 1949 the preamble to the constitution of India authorised Indian states to have control over alcohol regulation policies (such as alcohol tax imposition, prohibition of production and consumption, and regulation of alcohol industries). The transfer of authority from the central government to individual states made regulation of alcohol particularly significant because of its harmful effects on health.[2]

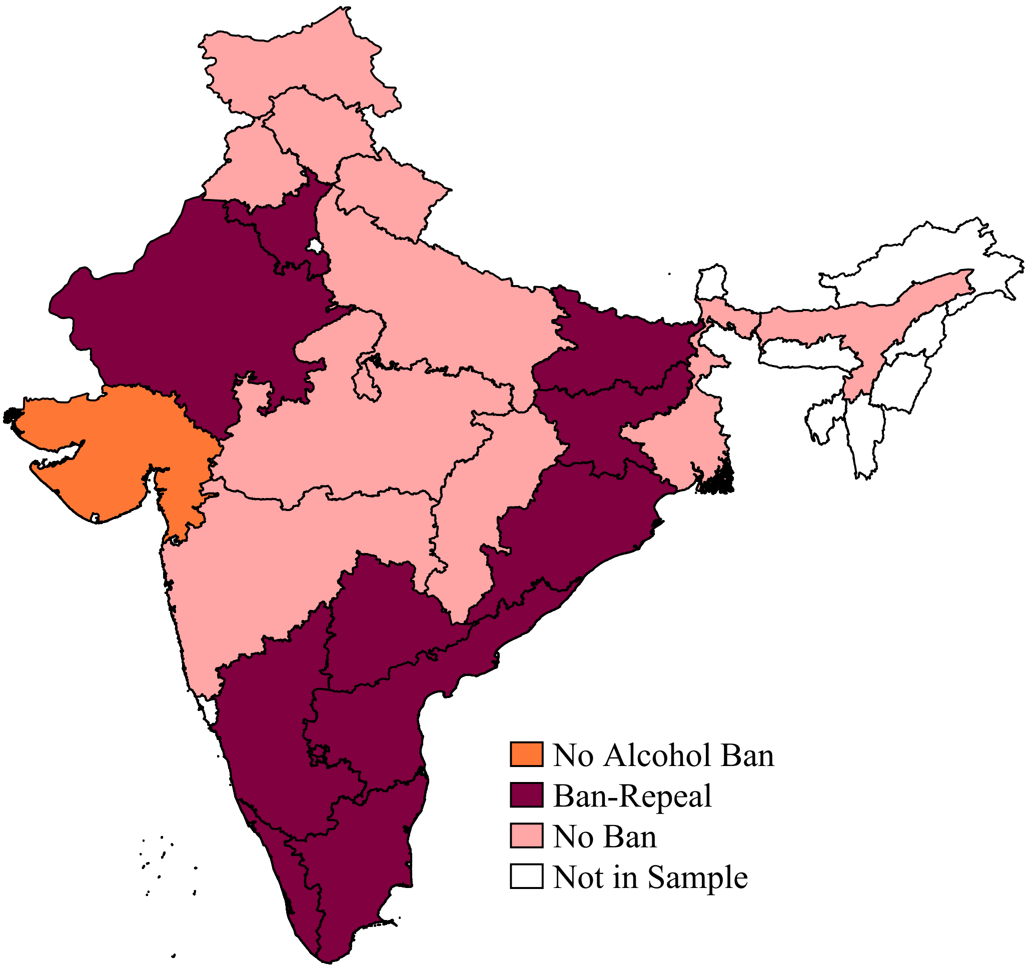


Figure 3.1: State Variations in Alcohol Ban

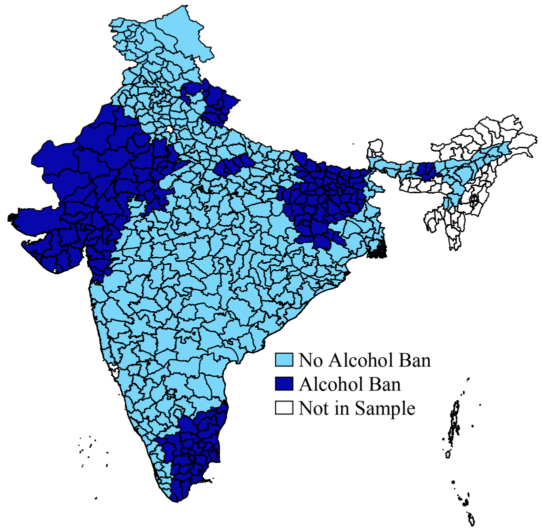
[2] The Directive Principles in Article 47 suggests: "The state shall regard the raising of the level of nutrition and standard of living of its people as among its primary duties and, in particular, the state shall endeavour to bring about prohibition of the use except for medicinal purposes of intoxicating drinks and of drugs which are injurious to health."

Surprisingly, these states did not implement the policy because of health reasons and nutrition levels, in line with the constitution of India, but for various other reasons, as described below. Over time, alcohol regulation laws in India varied significantly at the state and district level. Figure 3.1 highlights the variation in the state-level alcohol bans.

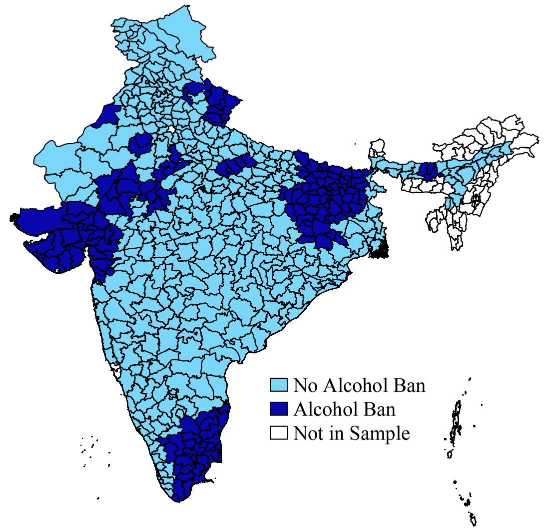
Gujarat is the only state to continuously prohibit alcohol (i.e., be a dry state) since 1960. The ban was imposed in the birth state of Mahatma Gandhi, who is known as the "Father of Nation" in India (Brown, 1991). Prohibition was adopted in Gujrat to pay homage to Gandhi's belief that alcohol is not good for one's health. States like Rajasthan, Haryana, Bihar, Jharkhand (part of Bihar until 2001), Orrisa, Andhra Pradesh, Telengana (part of Andhra Pradesh until 2014), and Tamil Nadu enacted the ban but repealed it a couple of years later, following changes in state leadership. The no alcohol policy was primarily imposed as a reaction against too much westernization, differences in religion or politics, and per capita income (Luca et al., 2015). However, alcoholic beverages are heavily taxed and represent the second–largest source of state funding. As alcohol taxation covers up to one–fifth of each state's total revenue, several states had to repeal the ban because of deficits.[3] The district variation can be seen in Figure 3.2, which shows the decade–to–decade variation in alcohol barriers.

The states implemented two types of alcohol prohibition: Complete Prohibition and Partial Prohibition. Complete prohibition occurs when both the production and consumption of alcohol was banned; Partial Prohibition occurs when only a subset of liquor is banned. I refer to both policies as an "alcohol ban" in this paper.

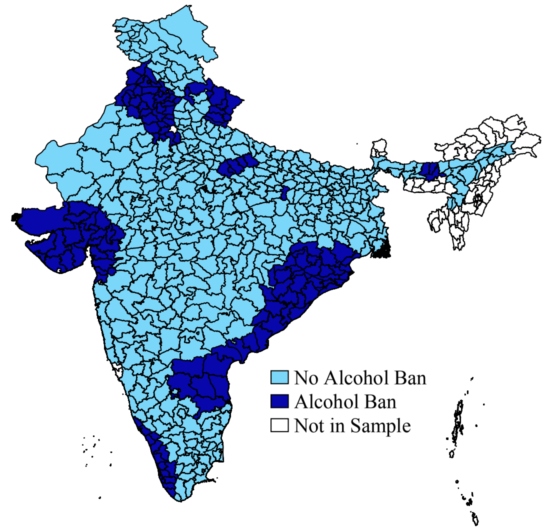
[3] Repealing a policy based on state/central revenue is not new. A ban on cigarette smoking was repealed after the American Civil War (Tate, 2000, Gottsegen, 1940). Also, Prohibition was called off due to the shortage of revenue following World War I and the Great Depression in 1933 (Okrent, 2010).



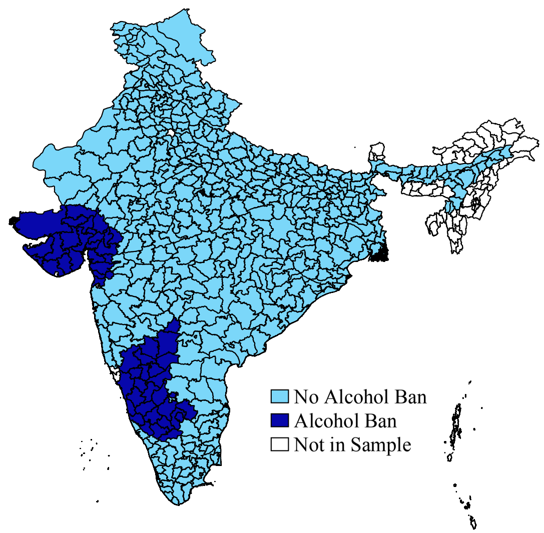
(a) 1972-1979



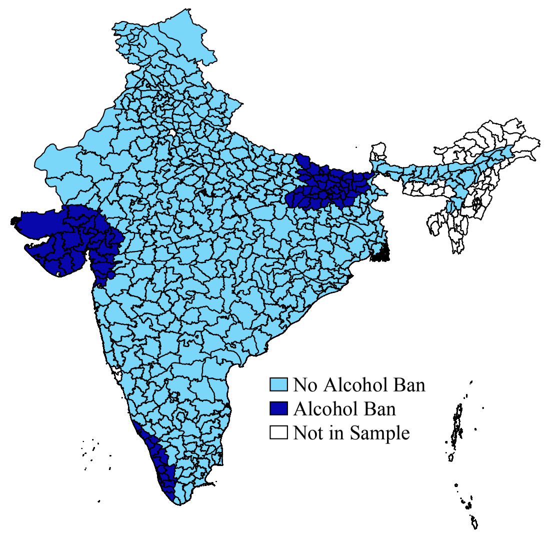
(b) 1980-1989



(c) 1990-1999



(d) 2000-2009



(e) 2010-2016

Figure 3.2: Geographical Variation in Alcohol Bans by Decade Across Districts

3.3 Data

3.3.1 Data Source

For crime statistics, I pulled information from India's National Crime Records Bureau (NCRB), a government publication on crime across the country. This is the only source that provides official crime data in India. NCRB compiles and publishes annual data[4] in the "First Investigation Report (FIR)" from the district police departments. For the analysis, district–level crime data for 45 years from 1972 to 2016 was used, since these are the only years in which district crime statistics are available. The publications cover many different crime categories. However, for comparability with previous studies, I selected the most–studied crime categories: murder, kidnapping, rape, assaults, cruelty, theft, robbery, armed robbery, burglary, and rioting. Further, I employed five waves (1971, 1981, 1991, 2001, and 2011) of district–level population records from the Indian Census to obtain crime rates per 100,000 inhabitants.

3.3.2 District Boundaries

India has witnessed a dramatic decentralization of districts over the last five decades (e.g., splitting a district into two new districts over time). In 1971, there were 356 districts, which became 366 districts in 1981, 450 in 1991, 593 in 2001, and 640 districts in 2011. Currently, there are 725 districts in India. In order to compare the same geographical units over time, I needed to map the districts to their parent districts in 1971. To construct the district mapping changes in boundaries, I follow Kumar and Somanathan (2009) to map the districts until 2001. Beyond that point, I extend the work of Kumar and Somanathan (2009) in mapping districts' boundaries until 2011. In this procedure, several districts were either aggregated to form a single parent district or one large district was formed by combining several districts.

[4] Under the Indian Penal Code (IPC) and Special & Local Laws (SLL).

3.3.3 Estimation Sample

98% of the Indian population lives in these 21 major states.[5] Every district located in the corresponding state after bifurcation (Jharkhand was separated from its parent state Bihar, Uttrakhand from Uttar Pradesh, Chhattisgarh from Madhya Pradesh, and Telangana from Andhra Pradesh) is covered under that particular state from the beginning (1972). Therefore the final sample consists of 324 districts across 21 states between the years 1972 and 2016.[6]

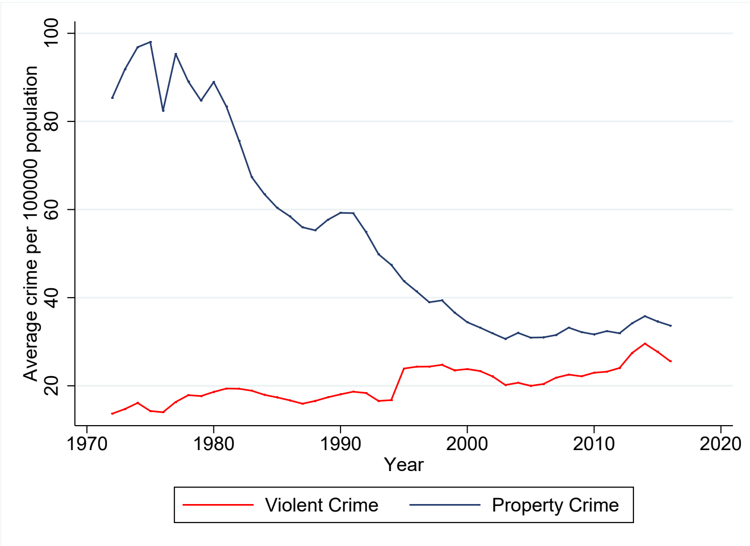


Figure 3.3: Nationwide Raw Trends in Crime Rates

Crime rates are computed as annual incidents per 100,000 population from 1972–2016. Statistics for crime against women, specifically assaults (which includes molestation and sexual harassment) and cruelty, are available from 1995 onward. Figure 3.3 shows the raw trend in nationwide crime rates. It shows that the incidence of property crime (such as theft, robbery, armed robbery, burglary, and rioting) gradually declined from 1980 onward. By contrast, violent crime (murder, kidnapping, rape, assault, and cruelty) has increased over time. Table 3.1 shows summary statistics from 1972–2016 in my final sample.

[5] These are: Andhra Pradesh, Assam, Bihar, Chattisgarh, Gujrat, Haryana, Himachal Pradesh, Jammu & Kashmir (which was considered a state until 2019), Jharkhand, Karnataka, Kerala, Madhya Pradesh, Maharashtra, Odisha, Punjab, Rajasthan, Tamil Nadu, Telengana, Uttar Pradesh, Uttrakhand, and West Bengal.

[6] Karump district (in the state of Assam) was dropped as there was an alcohol prohibition in Barepta district, which was carved out from its parent district, Karump. To follow districts over time (since 1972), Karump was dropped from the analysis.

Table 3.1: Summary Statistics of Crime Rates

	Sample Size	Mean	SD
Violent Crimes			
Murder	14500	3.39	2.35
Kidnapping	14500	2.53	2.53
Rape	14500	1.60	1.59
Assaults	7128	5.41	4.76
Cruelty	7128	6.14	5.99
Property Crimes			
Theft	14500	34.68	37.42
Robbery	14500	2.53	2.98
Armed Robbery	14500	0.94	1.63
Burglary	14500	16.34	14.41
Riot	14500	9.49	10.07

Notes: Author's calculation from crime statistics.

3.4 Econometric Model

The identification strategy relies on the variation in alcohol regulation across districts over time. I estimate the following difference-in-differences specification:

$$C_{dt} = \alpha_0 + \beta_1 DistrictPostBan_{dt} + \delta_d + \phi_t + \delta_d \times t + \xi_{dt} \quad (3.1)$$

Here C_{dt} is the crime rate in district d in year t. $DistrictPostBan_{dt}$ is a dichotomous variable which takes 1 for the period alcohol is prohibited in the treated districts and 0 otherwise. Treated districts take the value 1 if alcohol is banned in those districts and 0 for the control districts if an alcohol ban was not imposed. There are reasons to believe that the effect of alcohol bans took place immediately as it was associated with a penalty. The parameter of interest is β_1, the causal effect of exposure to alcohol bans in the treated districts, which is identified from the variation within districts across years. In the preferred specification, I allow for unrestricted time–fixed effects (ϕ_t) and district effects (δ_d) to capture time–invariant district and year heterogeneity. Standard errors are clustered at the district level.

Given the long panel, I include district-specific linear time trends ($\delta_d \times t$) to capture trends in the unobserved district characteristics over criminal activities. District–specific linear trends will absorb any changes across districts in criminal activities over time due to unobserved factors (such as the Naxalite movement, health). Due to the presence of large and small districts, the regression is weighted using population weights. ξ_{dt} captures other unobserved determinants of crime.

One of the potential identification problems that arises in this study arises from migration. In India more than 29 languages are spoken across the districts. Language barriers act as a hindrance to social networking (Munshi and Rosenzweig, 2009). Therefore, migration in India is very low and affects around 4 percent of the total population (Roy, 2015).

3.5 Results

I begin the causal analysis by presenting evidence on the average effect of alcohol bans on criminal activities. Figure 3.4 reports estimates associated with different crime measures (violent crimes and property crimes). Each coefficient, when read from left to right in the graph, is the outcome of different regression specifications from Eq. (3.1). The first coefficient outcome plotted in the graphs does not include any controls. The second outcome plotted includes district fixed effects. The third adds time fixed effects and the last one adds district-specific time trends. The vertical lines denote the confidence interval in Figure 3.4. As can be seen in Figure 3.4, the estimates for murder, assaults, cruelty, and robbery are all negative and are statistically significant, even before including the district-specific linear trends. However, the magnitude of the coefficients is smaller for murder, assault, cruelty, and robbery after including those trends. Findings reveal that assault drops after the alcohol bans and is statistically significant across all the specifications.

Figure 3.4 also shows that the coefficients for rape and rioting increased after controlling for district and year fixed effects. By contrast, inclusion of the linear trend shows that bans reduced rape significantly at the 5% level. Furthermore, the estimates for kidnapping, theft, armed robbery, and burglary are insignificant when controlling only for state and year fixed effects. Taken together, these estimates suggest that alcohol bans across districts in India successfully reduced criminal activities. Interestingly, the analysis shows that these estimates drop after the inclusion of linear trends.

The figure also shows that the coefficients for rape and rioting have increased after controlling for district and year fixed effects. By contrast, inclusion of the linear trends shows that bans reduced rape significantly at the 5% level. However, the estimates for kidnapping, theft, armed robbery, and burglary are insignificant when controlling for the fixed effects. More importantly, the finding shows that the estimates significantly drop after the inclusion of the linear trend. Taken together, these estimates suggest that alcohol bans across the districts in India successfully reduced criminal activities.

Table 3.2: Effects of Alcohol Ban on Violent Crime

	Violent Crime				
			Crime Against Women		
	Murder	Kidnapping	Rape	Assault	Cruelty
District×Post Ban	-0.222**	-0.492***	-0.105**	-0.727**	-1.351***
	(0.083)	(0.104)	(0.048)	(0.254)	(0.326)
Mean of dependent variable	3.39	2.53	1.60	5.41	6.14
Percentage change (in %)	-0.07	-0.19	-0.07	-0.13	-0.22
District fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
District–specific time trends	Yes	Yes	Yes	Yes	Yes
R–square	0.71	0.75	0.83	0.84	0.88
Sample Size	14500	14500	14500	7128	7128

Notes: Each parameter is from a separate regression of the crime outcome variable on exposure to an alcohol ban. Crime rates are expressed in per 100,000 population. Estimates are weighted using population weights and clustered in districts. ***, **, and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 3.2 and 3.3 reports the causal estimates of the policy on crime rates. The table shows the comprehensive and preferred point estimates from Eq. (3.1). As seen in Table 3.2, the alcohol ban led to a significant drop in the rate of murder, by 0.222 crimes per 100,000 population (7% of the mean). Likewise, prohibition appears to have decreased

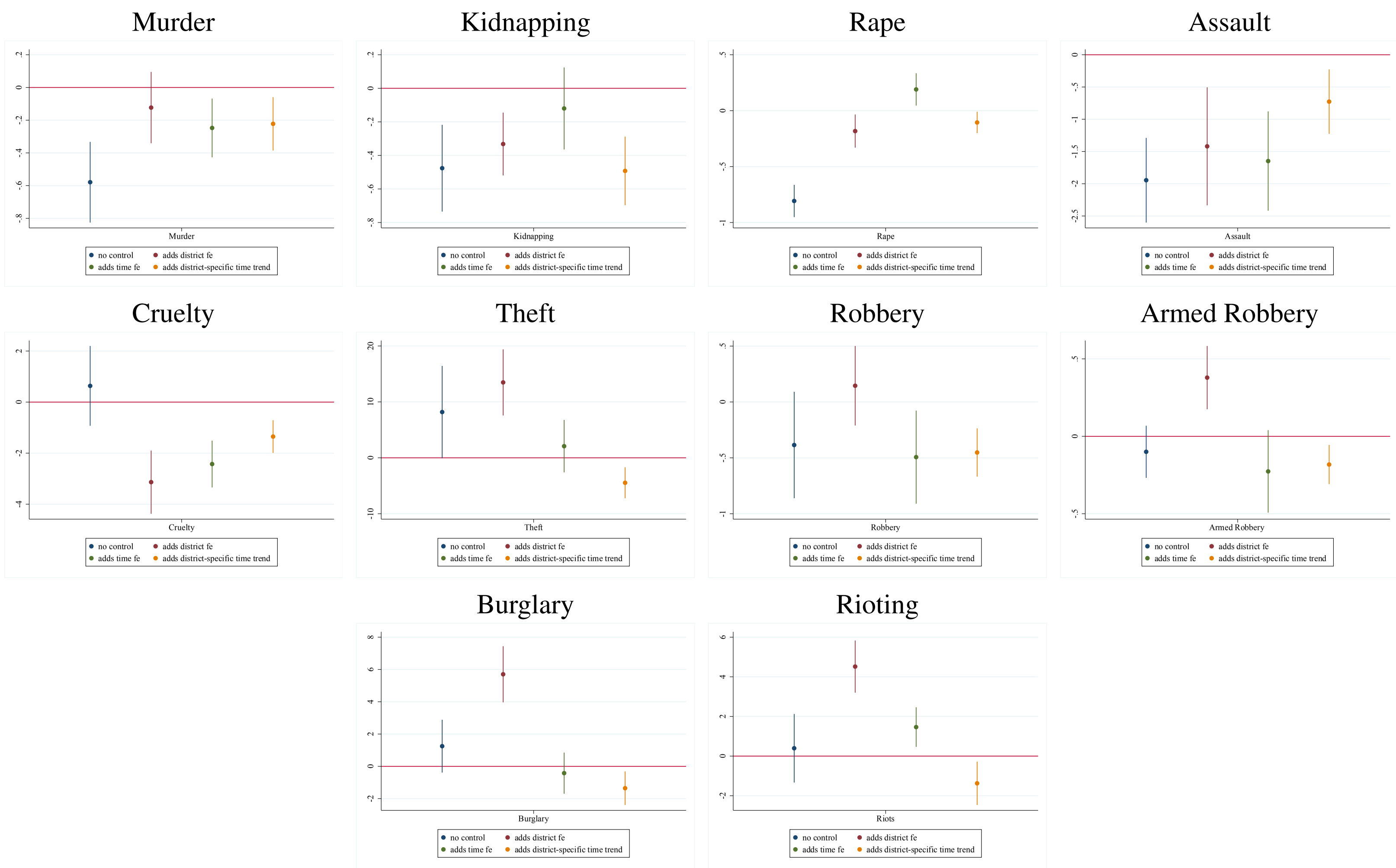


Figure 3.4: Estimates from different specifications for each crime

Notes: The vertical line associated with the coefficient represents the confidence interval. Graphs report coefficient estimates from separate regressions of Eq. (3.1).

kidnapping by 19% (-0.492/2.53) at the 1-percent level. The estimates for crime against women are divided into three categories (rape, assault, and cruelty); they clearly drop and are statistically significant. Alcohol bans prevented rapes by 0.105 crimes per 100,000 population (7 percent of the mean) and reduced assault by 0.727 crimes per 100,000 population (13% of the mean) in India. Noticeably, the results show a significant drop in cruelty by 1.35 crimes per 100,000 population (approximately 22 percent of the mean). Altogether, the findings suggest that violent crimes could be prevented by the presence of alcohol prohibition laws.

Recent work by Luca et al. (2015) did not detect any significant effects on rape by the state–level alcohol bans. However, I find a significant fall in rapes after imposing district–level alcohol prohibition laws. This might be because there is more variation in the district–level alcohol bans. Also, there is heterogeneity in time effects, so district–level study is better able to capture the correct effect.

Table 3.3: Effects of Alcohol Ban on Property Crime

	Property Crime				
	Theft	Robbery	Armed Robbery	Burglary	Riot
District×Post Ban	-4.464**	-0.452***	-0.182**	-1.353**	-1.373**
	(1.405)	(0.110)	(0.064)	(0.527)	(0.555)
Mean of dependent variable	34.68	2.53	0.94	16.34	9.32
Percentage change (in %)	-0.13	-0.18	-0.19	-0.08	-0.14
District fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
District–specific time trends	Yes	Yes	Yes	Yes	Yes
R-square	0.86	0.68	0.75	0.81	0.69
Sample Size	14500	14500	14500	14500	14500

Notes: Each parameter is from a separate regression of the crime outcome variable on exposure to an alcohol ban. Crime rates are expressed in per 100,000 population. The model controls for district fixed effect, year fixed effects, and district-specific time trends. Estimates are weighted using population weights and clustered by districts. ***, **, and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 3.3 reports the separate estimates of alcohol bans on crime, for five different property crimes (theft, robbery, armed robbery, burglary, and rioting), using Eq. (3.1). On average, theft is a frequent crime (34.68). Imposition of bans significantly helped to prevent theft by 13 percent of the mean, with a coefficient of 4.464 crimes per 100,000 population. Robbery declined by 0.452 crimes per 100,000 population (18 percent of the mean). Importantly, the impacts of the bans remain negative and statistically significant for armed robbery at 0.182 crimes per 100,000 population. Similarly, burglary dropped by 1.353 crimes per 100,000 population, which is approximately 8% of the mean. Finally, prohibition significantly helped to prevent riots by 1.373 crimes per 100,000 population. These results indicate that alcohol bans help to reduce property crimes.

3.5.1 Placebo Test

This section provides a further check on my identification strategy. To support the causal interpretation of my results, I conduct a falsification test.

The alcohol bans were implemented across the districts at different points in time. This suggests a natural approach to a placebo test. First, the sample is restricted to the non-treated districts. Second, I randomly assign a fake treatment to the stipulated number of districts within and across the years, as in the actual treatment, and re–estimate Eq. (3.1) on this sample. Finally, this procedure is repeated 1000 times. Figure 3.5 represents the graphs from the above falsification procedure for each crime. It is evident from the graphs that the pseudo–estimates from assigning random fake treatments 1000 times are centered around zero. The actual estimates (see the vertical line in the sub–figures of Figure 3.5) lie to the left of the pseudo–distribution.

Table 3.4 reports the average of the coefficients obtained from this simulation after controlling for district fixed effects, year fixed effects, and district–specific linear trends. The average estimates indicate a null effect across various violent and property crimes. The results of the placebo test boosts the credibility of my estimates.

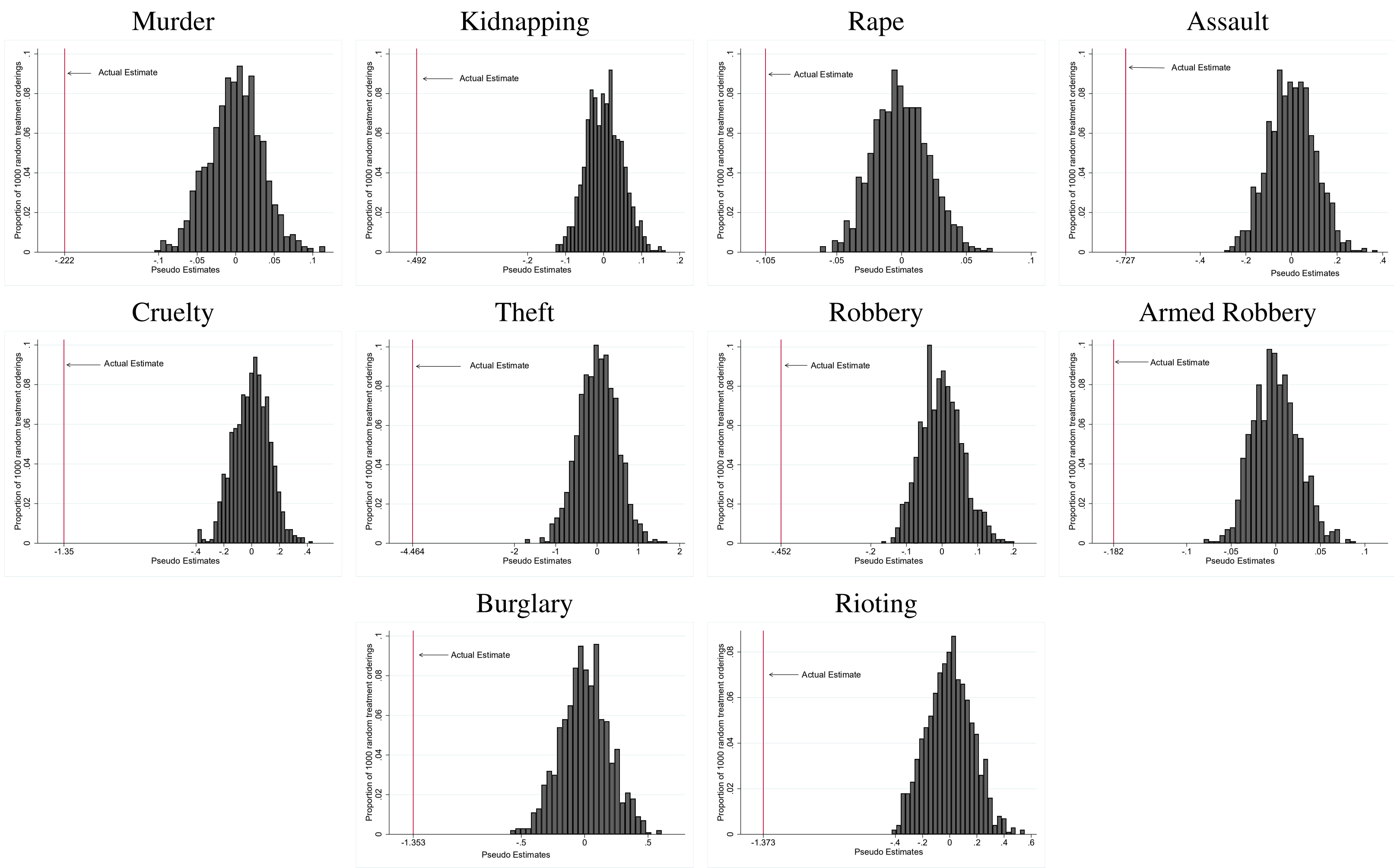


Figure 3.5: Random Treatment Ordering of Criminal Activities

Notes: Each sub-figure represents average estimates of each crime from the placebo test. The horizontal axis measures pseudo estimates, while the vertical axis shows the associated proportion of 1000 random treatment orderings. The vertical line (red) represents the actual (baseline) estimate.

Table 3.4: Falsification Treatment Effect on Crime Outcomes

	Average Effect on Violent Crime				
			Crime Against Women		
	Murder	Kidnapping	Rape	Assault	Cruelty
District×Post Ban	-0.0007	0.0015	-0.0005	0.0006	-0.0015
	(0.0344)	(0.0469)	(0.0213)	(0.1059)	(0.1271)
District fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
District–specific time trends	Yes	Yes	Yes	Yes	Yes
R-square	0.713	0.749	0.826	0.845	0.898
	Average Effect on Property Crime				
	Theft	Robbery	Armed Robbery	Burglary	Riot
District×Post Ban	0.0129	-0.0016	-0.0000	0.0027	-0.0006
	(0.4833)	(0.0586)	(0.0253)	(0.1838)	(0.1722)
District fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
District–specific time trends	Yes	Yes	Yes	Yes	Yes
R-square	0.875	0.681	0.757	0.827	0.716

Notes: Baseline estimates are shown in Table 3.2 and 3.3. Estimated coefficients are averages of 1,000 placebo coefficients. Each parameter is from a separate regression of the crime outcome variable on exposure to an alcohol ban. Crime rates are expressed in per 100,000 population. All regressions are weighted using population weights and standard errors are clustered by districts.

3.6 Mechanism

Finally, I investigate the underlying mechanism of the impact of alcohol bans and crime on India. The results from this section can help guide policy discussions and future studies on alcohol bans.

One of the channels for the reduction in criminal activities may be related to cognitive impairment due to the intoxication that occurs under the influence of alcohol. As alcoholism is associated with poverty and unemployment (Fleisher, 1966, Fajnzylber et al., 2002), poor people will be greatly affected. To unpack the mechanism at play in this study, I therefore test for heterogeneous treatment effects by levels of poverty and unemployment across Indian states.

Two-thirds of the Indian population lives on less than $2 a day.[7] Heavy alcohol drinking is common among low-income populations in India (Schilbach, 2019). Experimental evidence by Schilbach (2019) suggests that self-control among heavy drinkers is limited. If alcoholism is associated with crime, then having alcohol bans in place should have a particularly large effect in states with high levels of poverty. In India, high–poverty states are called the BIMARU (meaning "sick") states.[8] The current BIMARU states are Bihar, Jharkhand, Rajasthan, Madhya Pradesh, Chattisgarh, Uttar Pradesh, and Uttarakhand. These states are categorized on the basis of low literacy rates, low economic growth, and high population.

More specifically, I construct binary variables. High–poverty state takes the value 1 if the state is a part of the BIMARU group. Likewise, low–poverty states is an indicator variable (=1) if the state is not a part of the BIMARU group.

Table 3.5 Column (1) first reproduces the estimate from the baseline specification using Eq. (3.1). Column (2) reports the estimates from low–poverty states. Specifically, the estimates in Column (3) are from the high-poverty states in India. The result in Table 3.5 shows that alcohol bans in low poverty states lead to sizable and significant reductions in kidnapping, assaults, cruelty, and theft. Conversely, the no alcohol policy in high–poverty states significantly impacts almost all types of violent and property crime activities. In sum, Table 3.5 suggests that the impact of alcohol bans on crime is substantially driven by high–poverty states in India.

[7] https://www.soschildrensvillages.ca/news/poverty-in-india-602.

[8] The term BIMARU was coined by Indian demographer Ashish Bose in 1980 by isolating the first letter from each parent state (Bihar, Madhya Pradesh, Rajasthan, and Uttar Pradesh) in India. Later, the states bifurcated from these states were also included in the BIMARU group. These states were classified as high–poverty states according to the Human Development Index (HDI) and Multidimensional Poverty Index (MPI).

Table 3.5: Effect of Alcohol ban on crime by high (low) poverty states

Outcome	Baseline	Low poverty states	High poverty states
	(1)	(2)	(3)
Murder	-0.222**	0.016	-0.406**
	(0.083)	(0.078)	(0.147)
Kidnapping	-0.492***	-0.426***	-0.790***
	(0.104)	(0.121)	(0.168)
Rape	-0.105**	0.075	-0.263**
	(0.048)	(0.055)	(0.094)
Assaults	-0.727**	-1.087***	-1.519***
	(0.254)	(0.324)	(0.234)
Cruelty	-1.351***	-2.141***	-0.373
	(0.326)	(0.365)	(0.320)
Theft	-4.464**	-2.610**	-13.22***
	(1.405)	(1.204)	(3.158)
Robbery	-0.452***	0.031	-1.577***
	(0.11)	(0.087)	(0.203)
Armed–Robbery	-0.182**	0.001	-1.120***
	(0.064)	(0.038)	(0.141)
Burglary	-1.353**	-0.738	-5.076***
	(0.527)	(0.584)	(0.943)
Riots	-1.373**	-0.778	-1.614*
	(0.555)	(0.693)	(0.895)

Notes: Each outcome is from a separate regression of the crime outcome variable on exposure to an alcohol ban. Crime rates are expressed in per 100,000 population. The total number of observations for the baseline outcome is 14500 (for assault and cruelty it is 7128), the number of observations in low-poverty states is 8204 (for assault and cruelty it is 4048), and the number of observations in high-poverty states is 6296 (for assault and cruelty it is 3080). All the regression controls for district fixed effects, year fixed effects, and district-specific time trends. Estimates are weighted using population weights and clustered by districts. ***, **, and * indicate significance at the 1%, 5% and 10% levels, respectively.

3.7 Concluding Remarks

The idea that alcohol bans may promote crime occupies a prominent position in the public debate among crime watchers for developed countries. However, this analysis offers causal evidence, using the largest–to date dataset on the impact of district alcohol bans on crime, suggesting that such a concern is not justified for developing countries.

Results suggest that alcohol bans are associated with substantial reductions in crime rates. The estimates reflect that exposure to alcohol prohibition led to a decrease in various crime outcomes: violent crimes, crime against women, and property crimes. Interestingly, I find that rape significantly dropped by 7 percent after alcohol prohibition across districts, whereas the study by Luca et al. (2015) suggested no effect on rapes after alcohol prohibition at the state level. Results are robust to randomly assigned pseudo treatment to districts across years. In addition, the results are dominated by high–poverty states in India, which suggests, in light of the literature, that alcoholism's connection to cognitive behaviour is the underlying mechanism..

This paper's limitations include three factors. First, the estimates (like those of crime studies in India generally) speak directly to changes in reported crimes only. Second, the potential cost involving alcohol bans is a loss of state revenue. Third, the effect of illicitly produced alcohol is beyond the scope of this paper.

This study takes a step towards the larger goal of understanding the relationship between crime rates and alcohol ban policies across districts. More research is needed on this topic, as the findings, importantly, draw attention to potential policy implications for lowering crime rates in a developing country set-up.

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