Alcohol Consumption and Intimate Partner Violence: Long-Term Effects of Temporary Alcohol Ban*

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Abstract

This study traces the impact of a partial liquor ban – from launch to reversal – on alcohol consumption and intimate partner violence. We decompose the long-run effects of a temporary and selective liquor ban on drinking behavior and women's experience with intimate partner violence to include a policy impact and a policy reversal effect. Leveraging neighbor states comparable and sufficiently isolated in our context as the control group, we employ difference-in-differences and event-study approaches to identify the causal impacts of a policy that shuts down hard liquor-serving local bars. We find that alcohol consumption in bars significantly reduced over the policy period and rebounded after the policy removal, with no impacts on home drinking. Consistent with a policy-induced change in the volume of alcohol consumption during and after the alcohol ban, we find that physical violence declined during the prohibition and overshot its starting level following the reversal only in high-wealth households, the primary consumers of expensive hard liquors most likely affected by the ban.

Keywords: Non-price alcohol control, Domestic violence, Alcohol drinking, Kerala, India

JEL Codes: D04, D12, J16, K42

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1 Introduction

The theoretical foundations linking alcohol consumption to negative private and social outcomes date to Becker (1968). Since then, the theoretical and empirical link between alcohol consumption and crime in general, violent behavior towards an intimate partner and strangers, mortality, and traffic accidents has been well-documented.[1] The channels triggering changes in alcohol consumption, in turn, are attributed to income effects caused by policies like taxes and bans on alcohol consumption (Aizer, 2010; Anderberg et al., 2016), and enforcement of minimum legal drinking age (MLDA) legislations. However, remarkably little is known about the persistence of *temporary* alcohol control policies on alcohol consumption and alcohol-related externalities over the long run. Exploiting a unique experiment of a *partial* and *temporary* alcohol ban in the State of Kerala in India over 2014-2017, this paper is the first to causally address the short- and long-term effects of temporary policy on alcohol consumption patterns and the incidence of intimate partner violence.

In doing so, we contribute to the literature on behavioral changes accompanying alcohol bans and specifically to the question of whether behavioral changes tend to persist after the ban has been lifted. Kerala's liquor ban was unique and partial on two fronts: *only* a particular type of liquor - hard liquors[2] - was targeted (not beer, wine, and informally produced toddy), and the prohibition of the sale of these hard liquors was confined *only* in bars without restricting their sale in liquor stores and five-star hotels. Thus, this policy offers a unique natural experiment to study the question of how this treatment affects the drinking behavior and whether it reduces intimate partner violence by directly reducing liquor consumption or increases intimate partner violence by indirectly leading to drinking outside of bars - especially at home. Furthermore, hard liquor is costly and consumed by those who are relatively rich, which allows us to analyze the heterogeneous impact across individuals belonging to different wealth groups.

While a fair number of studies have analyzed the intended and unintended consequences of alcohol-related policies for developed countries,[3] similar research in developing country contexts is sparse. India is a notable exception given its decentralized governance structure, which allows the States within the Republic to enact their own alcohol policies towards various types of liquor, and these policies, in turn, are guided by either revenue motives, health concerns, or cultural norms.[4]

[1]Alcohol consumption is associated with a variety of misbehaviors. For example, studies have examined the relationships between alcohol consumption and crime (Carpenter and Dobkin, 2015; Lindo et al., 2018; Hansen and Waddell, 2018), violent behavior (Card and Dahl, 2011; Luca et al., 2015; Ivandić et al., 2024), mortality (Carpenter and Dobkin, 2009; Carpenter et al., 2016; Yakovlev, 2018; Kueng and Yakovlev, 2021), and traffic accidents (Jackson and Owens, 2011; Lindo et al., 2016; Sloan, 2020).

[2]The hard liquors include foreign and Indian-made foreign liquors (IMFLs) such as brandy, whisky, rum, cognac, tequila, vodka, and gin, which are often expensive than other types of alcoholic drinks.

[3]As an example of such studies in the developed countries, Yakovlev (2018) finds that a kink in the policy regime of the federal excise tax on vodka and the related price increase significantly reduces the alcohol consumption and the rate of male mortality in Russia. For non-price policies, Kueng and Yakovlev (2021) show that altering the relative supply of hard and light alcohol in Russia's anti-alcohol campaign changes young consumers' alcohol taste from hard to light alcohol, which further reduces incidences of binge drinking and alcohol-related deaths.

[4]Papers analyzing alcohol policies in India can be grouped into two areas: estimating the price elasticity of demand

As of 2022, three States in India – Gujarat, Bihar, and Nagaland have complete bans on alcohol sales, while Tamil Nadu and Kerala have enacted partial bans at times prior.

To estimate the short- and long-run effects of the policy on alcohol consumption and physical violence, we use three nationally representative household surveys. First, for our analysis of alcohol consumption, we use household-level monthly panel data from the Consumer Pyramids Household Survey (CPHS), which enables us to estimate the "first stage" effect of the policy on alcohol consumption in bars and the impact on alcohol consumption at home. Results from the consumption effects also inform on the potential mechanisms through which the partial liquor ban has affected intimate partner violence in the short and long term. Second, for our analysis of intimate partner violence, we use household- and individual-year-level repeated cross-sectional data from the second, fourth, and fifth waves of India's Demographic and Health Survey (DHS). Unlike Khurana and Mahajan (2022) who use recorded crime data to analyze general violence against women throughout Kerala's partial alcohol ban, the DHS data allows us to construct the incidence of physical violence with different frequencies and conduct heterogeneity analysis at the micro (individual and household) level before, during and after the ban - such granular analysis is not commonly found in the economics of crime literature.[5] Third, we use the National Sample Survey (NSS) dataset to construct the baseline treatment intensity measure at the district level since it contains information on household's monthly consumption of different types of alcohol such as hard liquors, wine, beer, and toddy which enables us to define a treatment variable for a policy that bans hard liquors.

Our analysis shows that households' alcohol consumption in bars sharply fell right after the closure of hard liquor-serving bars. However, the decline lasted only nine months, and the consumption increased back to the pre-ban level in the 10th month and leveled off during the rest of the policy period.[6] After the policy reversal, we identify a moderate hike immediately afterward and a sharper increase after several months post-reversal. Ignoring the policy period and comparing the post-reversal and pre-ban alcohol consumption in bars yields similar but relatively more pronounced consumption-increasing effects in the periods after the policy reversal. We also find that

for various types of alcohol and examining the spillover effects of alcohol-related policies on social indicators like violence, crime, and accidents. Studies in the latter group (in which our paper is situated) show contrasting results. In response to the complete alcohol ban in Bihar introduced in 2016, Dar and Sahay (2018) finds an increase in crime while Chaudhuri et al. (2024) finds a reduction in the reported incidence of violent crimes but no impact on non-violent ones. In response to the partial alcohol ban in Kerala, Khurana and Mahajan (2022) finds no robust decline in the officially recorded complaints of violence against women.

[5] Almost all studies in the literature studying the effect of alcohol controls on violence against women are limited to violent activities outside of homes and near the drinking establishments based on criminal records, and are in the context of developed countries (see Hansen and Waddell (2018), Carpenter and Dobkin (2015), Carpenter (2007), and Conlin et al. (2005) for MLDA and Grönqvist and Niknami (2014) and Heaton (2012) for dry-day regulations).

[6] This response of alcohol consumption in bars to Kerala's liquor ban is consistent with existing findings on alcohol consumption during alcohol prohibition from the literature in developed countries (Miron and Zwiebel, 1991; Dills et al., 2005) that suggests prohibition reduces alcohol consumption only at the beginning of the prohibition period and becomes ineffective in the medium-term even though the policy is still in place. The short-term effect of the ban on alcohol consumption is also consistent with Marcus and Siedler (2015), who suggest that an alcohol sales ban reduces alcohol-related hospitalization among young people in Germany.

alcohol consumption at home was not affected by the closure of hard liquor-selling bars.

The results from our analysis estimating the effects of the policy changes on intimate partner violence highlight a statistically significant drop in physical violence within high-wealth households due to the ban during the policy period, but not in the whole sample. Zeroing in on the high-wealth households, we find that for each liter reduction in monthly consumption of hard liquor, the probability of women experiencing any physical violence in the past 12 months decreased by 0.03 percentage points within these households.[7] More importantly, our analysis suggests intimate partner violence rebounded in high-wealth households after the bar reopening. For each liter increase in monthly consumption of hard liquor, the probability of women experiencing physical violence in the past 12 months increased by 0.07 percentage points in high-wealth households, overshooting the decrease in physical violence during the policy period. Most importantly, on net, the probability of women experiencing physical violence in the last 12 months increased by 0.04 percentage points in the long run for each liter increase in monthly consumption of hard liquor in high-wealth households.

Our results from the consumption regressions suggest that there is no strong transition of alcohol consumption from bars to home, which precludes the possibility of an increase in home drinking via alcohol purchases from liquor shops in response to the closure of bars. Thus, the changes in intimate partner violence over the policy episodes are primarily explained by the changes in alcohol consumption in bars and the neuro-behavioral response to alcohol consumption. Findings from the literature studying the alcohol tolerance at different levels of alcohol intake also contribute to explaining the overshooting positive impact of removing the ban that prohibits hard liquor on intimate partner violence that we identified in the long term after the policy reversal.

The rest of the paper is organized as follows. Section 2 introduces our policy context. Section 3 describes the data and descriptive statistics. Section 4 presents the effects of policy changes on alcohol consumption. Section 5 discusses the effects of the alcohol policy episodes on intimate partner violence. Finally, Section 6 concludes.

2 Policy Context, Alcohol Consumption and Intimate Partner Violence in Kerala

Kerala's partial liquor ban is unique due to its partial nature: in terms of both the alcohol types and the specific outlets from which the prohibited types could be purchased, *and* its temporary implementation. The State government, at the time of the ban, intended to limit alcohol consumption and the related externalities in Kerala by proposing a 10-year plan to make Kerala a dry state; however,

[7] Our results on the domestic violence-reducing effect of the policy in the short-run are consistent with other studies such as Luca et al. (2015), who find a reduced incidence of violence against women (both at home and in crime data) over the short run associated with complete alcohol bans in India.

it was only effective from April 2014 till July 2017.[8]

2.1 Policy Context

In 2014, the Congress-led United Democratic Front (UDF) government announced a liquor ban that would have been progressively implemented through multiple stages, with complete prohibition by 2024. In the first stage, all bars in the state except the ones with five-star status are banned from selling hard liquor. Hence, only five-star hotels were able to grant an FL3[9] liquor license, and there were only 14 five-star hotels in the state by the start of the program. According to the official information from the Excise Department of Kerala, which grants alcohol licenses in the state, a total of 719 hard liquor serving bars run by private hoteliers with annual licenses of FL3 shut their doors in two phases on March 31, 2014 and March 31, 2015.[10] Bars renew their annual licenses specific to their services every year, and some bars, e.g., those renewed their license on February 28, 2014, were still operating until March 31, 2015, when all hard liquor-selling bars were closed. Approximately 400 bars were closed on March 31, 2014, and another 300 bars closed on March 31, 2015.[11]

Bars affected by the partial ban, however, were allowed to operate beer or wine parlors. Liquor stores were expected to be phased out gradually, i.e., retail liquor shops remained open during the first stage of this policy.[12] In terms of varieties, toddy or palm wine and drinks other than hard liquor, such as country liquor, beer, and wine, continued to be legally sold. This initial step of the policy was effective until July 2017, when the policy was eased by the Left Democratic Front (LDF) government in Kerala, allowing hotels with three or more stars to serve hard liquor after the new government won the State elections in May 2016. The newly elected State government argued that there were heavy losses in the State's tax revenue and a sharp deterioration in the tourism industry, which happens to be the main revenue contributor to the State.

2.2 Alcohol Consumption

Kerala has long been considered an Indian State with high alcohol consumption. Specifically, Kerala was the fourth-highest alcohol consumption State with 4.6 liters of monthly per-capita alcohol consumption, on average, over the period 2001-2012 according to the National Sample Survey (NSS) data (Column (1) of Table 1). Notable here is that the average monthly per-capita alcohol

[8] `https://www.bbc.com/news/world-asia-india-40213562`

[9] The Excise Commissioner grants several licenses to serve different types of alcohol in various outlets, and FL3 is a license that bars obtain for serving liquors.

[10] Information on the total number of bars closed down and the dates of two phases of the closure were obtained from the Excise Department of Kerala on request placed by the authors.

[11] `https://www.theguardian.com/world/2014/aug/29/kerala-alcohol-ban-hits-holidaymakers`

[12] With only 14 five-star hotels and 338 liquor shops (`https://www.bbc.com/news/world-asia-india-28892073`) owned by a state-run organization, Kerala State Beverages Corporation (KSBC) or BEVCO, from which people can freely purchase hard liquors, the closure of 719 hard liquor-selling bars is a significant intervention. The BEVCO administers all retail liquor stores in Kerala and organizes liquor distribution and wholesale of liquors across the State.

Table 1: Top 10 States/UTs with Highest Monthly Per Capita Alcohol Consumption (in liters)

	Top 10 states	Alcohol Mean (1)		Top 10 states	Toddy Mean (2)		Top 10 states	Country Liq. Mean (3)
1	Andhra Pradesh	5.331	1	Maharashtra	3.651	1	Arunachal Pradesh	1.989
2	Maharashtra	5.266	2	Mizoram	3.014	2	Gujarat	1.631
3	Arunachal Pradesh	4.921	3	Andhra Pradesh	2.936	3	Dadra & Nagar Haveli	1.595
4	Kerala	4.602	4	Kerala	2.436	4	Nagaland	1.346
5	Chandigarh	4.429	5	Puducherry	2.153	5	Andhra Pradesh	1.303
6	Daman & Diu	4.236	6	Daman & Diu	1.782	6	Assam	1.074
7	Dadra & Nagar Haveli	4.135	7	Nagaland	1.747	7	Sikkim	1.051
8	Puducherry	4.125	8	Jharkhand	1.744	8	West Bengal	0.942
9	Nagaland	3.963	9	Assam	1.707	9	Jharkhand	0.869
10	Gujarat	3.801	10	Arunachal Pradesh	1.595	10	Kerala	0.837
	All India	3.878		All India	2.204		All India	0.609

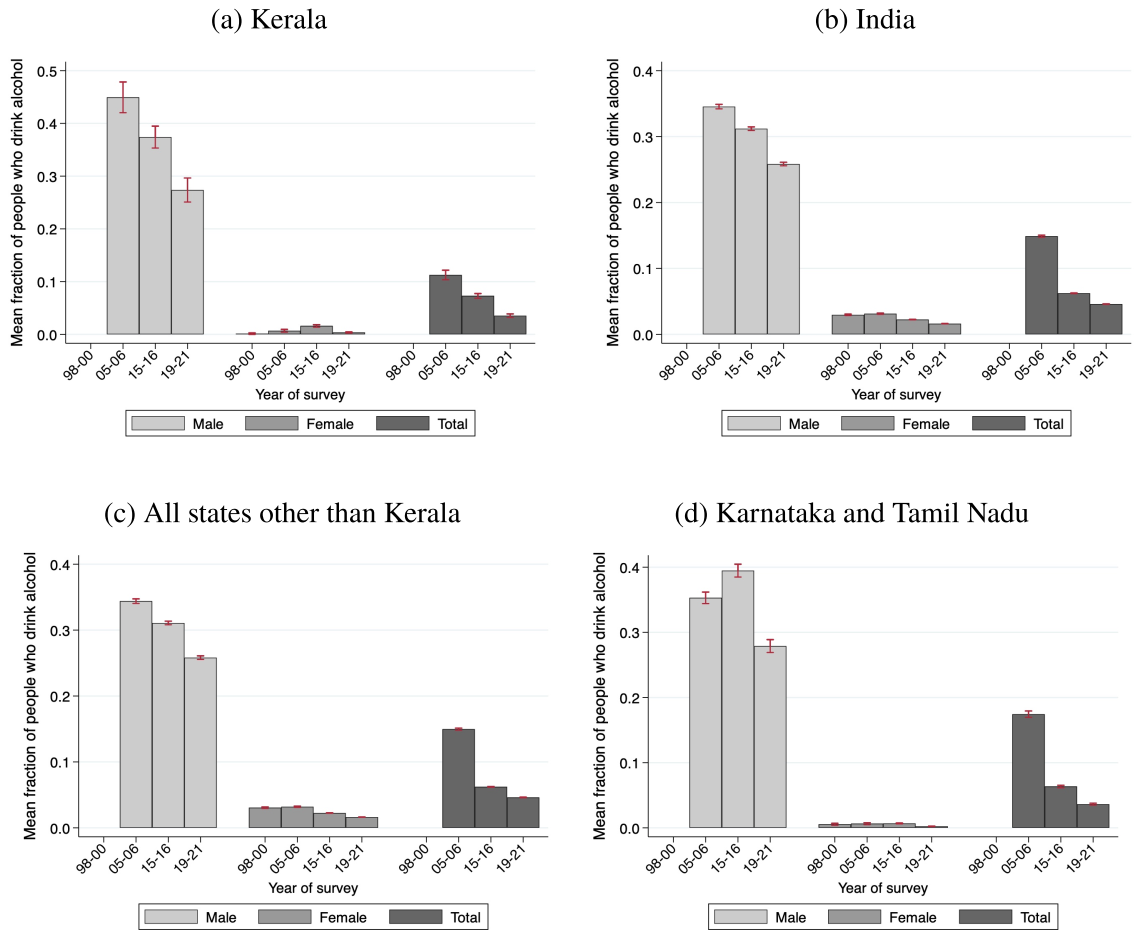
	Top 10 states	Beer Mean (4)		Top 10 states	Foreign Liq. Mean (5)
1	Chandigarh	2.994	1	Dadra & Nagar Haveli	0.633
2	Daman & Diu	1.634	2	Gujarat	0.434
3	Tripura	1.133	3	Jammu & Kashmir	0.405
4	Karnataka	1.106	4	Kerala	0.404
5	Arunachal Pradesh	1.004	5	Himachal Pradesh	0.395
6	Sikkim	0.996	6	Chandigarh	0.381
7	Madhya Pradesh	0.990	7	Delhi	0.366
8	Kerala	0.925	8	Mizoram	0.360
9	Delhi	0.858	9	Punjab	0.357
10	Dadra & Nagar Haveli	0.854	10	Haryana	0.352
	All India	0.762		All India	0.302

Notes: The table shows the top 10 states with the highest per capita alcohol consumption in India over the period 2001-2012, on average, based on nine NSS rounds from 2001-2002 (57^{th} round) through 2011-2012 (68^{th} round). Alcohol consumption is also disaggregated into four different types (toddy, country liquor, beer, and foreign/refined liquor or wine), and the top 10 states that lead in terms of consumption of each type are presented in comparison to the country average. The mean amount of total alcohol consumption is calculated by adding the mean consumption of each alcohol type, as shown in Column (1). All states and union territories (UTs) of India are considered, and we report ten states/UTs with the highest amount (liter) of monthly per capita alcohol consumption over the years, on average.

consumption in the neighboring States was 2.8 liters in Karnataka and 2.2. liters in Tamil Nadu, while the national average of monthly per-capita alcohol consumption was 3.9 liters over the 2001-2012 period. Disentangling the varieties of alcohol consumed reveals that Kerala was also the fourth State with the highest consumption of toddy and hard liquor, with per-capita consumption of 2.4 liters of toddy and 0.4 liters of hard liquor per month (Columns (2) and (5) of Table 1, respectively). Additionally, monthly per-capita consumption of country liquor and beer in Kerala was also high over the 2001-2012 period (Columns (3) and (4) of Table 1, respectively).

A gender-based comparison of alcohol consumption trends, measured by the share of individuals who drink alcoholic beverages to some extent in India and in selective States, is presented in Figure 1. The share of individuals (male and female) consuming alcohol within Kerala after the 2014 partial alcohol ban decreased to 7.3 percent in 2015-2016 from 11.3 percent in 2005-2006

Figure 1: Trends and Gender Differences in Alcohol Use in India



Notes: Based on DHS datasets 1998-2000, 2005-2006, 2015-2016, and 2019-2021. The figure presents the mean fraction of people who drink alcohol to some extent in the state of Kerala (panel (a)), India (panel (b)), the rest of the states other than Kerala (panel (c)), and two states that neighbor with Kerala—Karnataka and Tamil Nadu (panel (d)) with corresponding 95% confidence intervals.

(Figure 1a). This decline in the share of Kerala alcohol consumers during the policy period was driven by a decrease in the share of male consumers from 44.9 to 37.4 percent, while the fraction of female consumers has been constantly increasing from 0.1% in 1998-2000 to 0.7% as in 2005-2006 to 1.6% as in 2015-2016. The share of female consumers is almost negligible, especially in Kerala, compared to its country-level equivalent of 2.8%, which, by contrast, has been slightly decreasing over time (Figure 1b). Figure 1c presents the mean fraction of individuals who consume alcohol to some extent in the rest of India, which shows an very similar pattern compared to that of the entire country. The total mean fraction of individuals who consume alcohol in Kerala and its neighboring States, i.e., Karnataka and Tamil Nadu, evolved quite differently, as evidenced by the share of male and female alcohol consumers in these States. While the fraction of male consumers increased in Karnataka and Tamil Nadu during the policy period, the fraction of female consumers was relatively stable in these two States over the 1998-2016 period (Figure 1d). Figure 1 also shows that alcohol consumption by males drives the overall drinking pattern in India, with more than 30% of males but less than 3% of females consuming alcohol. This tendency is more pronounced in Kerala and its neighboring states.

2.3 Intimate Partner Violence

Two rounds of the DHS survey before the implementation of the policy, DHS-2 and DHS-3, show that Kerala had mid-to-low levels of domestic violence against women between 1998-2000 and 2005-2006, with 7.5% and 16.1% of women ever-experiencing any physical violence (Table 2), compared to other States and Union Territories (UTs) of the country. The mean fraction of women who experienced physical violence ever in life declined to 13.3% in 2015-2016 and to 9.6% in 2019-2021, as shown in Table 2. However, over the 1998 to 2021 time period, the incidence of physical violence - ever experienced in life - towards women in Kerala was below the national average.

Table 2: Mean Fraction of Women Ever Experienced Physical Violence

	State	1998-2000		State	2005-06		State	2015-16		State	2019-21
1	Tamil Nadu	0.360	1	Bihar	0.574	1	Manipur	0.511	1	Karnataka	0.431
2	Telangana	0.277	2	Madhya P.	0.454	2	Andhra P.	0.438	2	Bihar	0.394
3	Bihar	0.264	3	Uttar P.	0.436	3	Bihar	0.430	3	Manipur	0.380
4	Odisha	0.229	4	Tamil Nadu	0.421	4	Telangana	0.421	4	Tamil Nadu	0.374
5	Uttar P.	0.218	5	Manipur	0.418	5	Tamil Nadu	0.393	5	Telangana	0.367
6	Madhya P.	0.209	6	Rajasthan	0.416	6	Uttar P.	0.360	6	Uttar P.	0.352
7	Jharkhand	0.200	7	Tripura	0.412	7	Chhattisgarh	0.360	7	Jharkhand	0.310
8	Karnataka	0.196	8	Assam	0.385	8	Odisha	0.333	8	Assam	0.308
9	Arunachal P.	0.188	9	Arunachal P.	0.384	9	Jharkhand	0.325	9	Andhra P.	0.301
10	Andhra P.	0.173	10	Jharkhand	0.368	10	Madhya P.	0.319	10	Odisha	0.298
27	Kerala	0.075	24	Kerala	0.161	30	Kerala	0.133	30	Kerala	0.096
	Total	0.168		Total	0.319		Total	0.280		Total	0.264

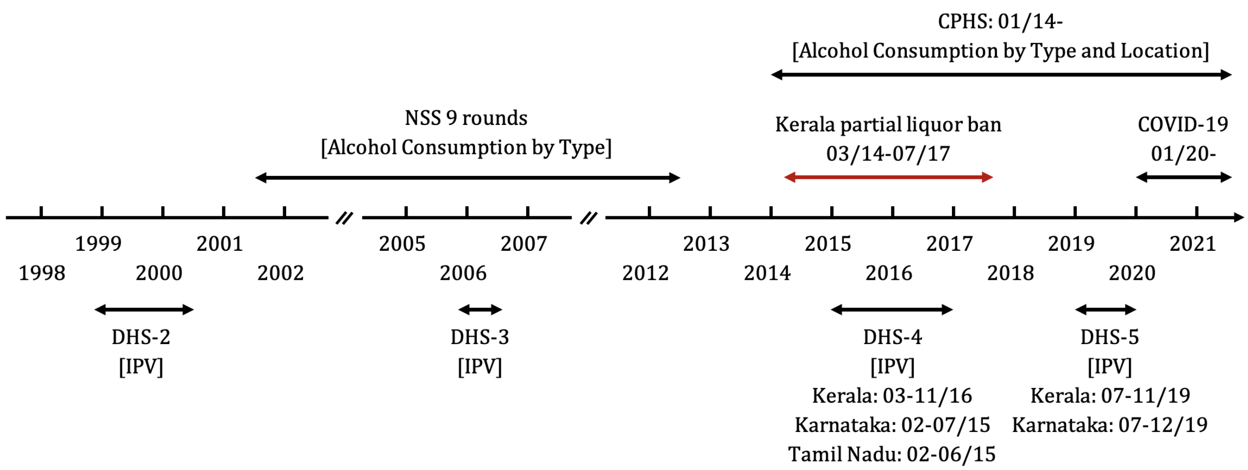
Notes: The table summarizes the domestic violence against women by state recorded in four rounds of India's DHS (1998-2000, 2005-2006, 2015-2016, and 2019-2021). All regions of India (states and union territories) are considered, and we highlight the top 10 states with the highest rate of physical violence, along with Kerala state. P. stands for Pradesh. The mean fraction of women who experienced physical violence by their husbands or partners substantially varies across regions throughout the country. The physical domestic violence against women considered is (i) having ever been pushed, shaken, or had something thrown, (ii) ever been slapped, (iii) ever been punched with a fist or hit by something harmful, (iv) ever been kicked or dragged, (v) ever been strangled or burnt, (vi) ever been threatened with knife/gun or other weapon, and (vii) ever had arm twisted or hair pulled.

3 Data

We use three nationally representative surveys. First, we use a household-level monthly panel dataset from the Consumer Pyramids Household Survey (CPHS) starting in January 2014. Using this dataset, we estimate the "first stage" effects of the closure of hard liquor-selling bars on household alcohol consumption in bars over the short (during the policy period) and long (after the policy reversal) run and evaluate potential mechanisms through which the policy could have affected the intimate partner violence by analyzing alcohol consumption at home over different episodes of the policy. Second, we use an individual-by-interview year-level pooled cross-sectional dataset from

India's Demographic and Health Survey (DHS) for the frequency of intimate partner violence reported by women. Our analysis relies on three rounds of this qualitatively rich survey for the years 1998-2000, 2015-2016, and 2019-2021.[13] We estimate the short-run impact of the treatment via the use of DHS-2 and DHS-4 and add the most recent DHS-5 to evaluate the long-run impact of the treatment and the effect of policy removal. Third, we use household-level cross-sectional data from the National Sample Survey (NSS) to (i) test parallel pre-trend for alcohol consumption and (ii) construct the treatment intensity variable at the district level for 2012, which is the NSS round closest to the policy change just before the treatment. We merge the district-level treatment intensity variable with the CPHS and DHS data to evaluate the intensive margin impact of the 2014 Kerala liquor ban on consumption and intimate partner violence patterns. Figure 2 illustrates the timing of key events and time coverage of the primary datasets.

Figure 2: Event and Data Timelines



Notes: The figure presents a diagram summarizing the timeline of events and datasets that illustrate the setting and justifies the empirical strategy used in our analysis.

3.1 Alcohol Consumption

Alcohol consumption data for pre- and post-ban periods generally come from two different sources. For the pre-policy period, alcohol consumption data is mainly accessed from the NSS, which reports monthly expenditures on various types of alcohol for cross-sectional households. Specifically, we use nine rounds of NSS from 2001-2002 through 2011-2012 to check the parallel pre-trends in alcohol consumption.

For the post-ban period, we utilize data on alcohol consumption from the Consumer Pyramids Household Survey (CPHS), starting in January 2014, which provides only three months of pre-ban periods. We use the CPHS dataset for our event-study analysis, estimating the short- and long-

[13] Data on domestic violence against women was not recorded in the first wave (1992-1993), and district identifier is not reported in the third round (2005-2006).

run effects of the partial liquor ban on household alcohol consumption. The CPHS is a monthly panel of household expenditure surveys and enables us to control for fixed effects at the granular level, such as household, month of the year, and year fixed effects. The advantage of the CPHS dataset is that it reports the location of a household's expenditure on alcohol. In particular, the data provides two variables on household alcohol consumption. The first variable is the expenditure by a household on the purchase of alcohol for consumption *at home* during the month. It includes country liquors, beer, wines, whiskey, rum, gin vodka, brandy, other similar liquor products, and local alcoholic drinks such as bhang and toddy. The second variable is the household's expenditure on the purchase of alcohol for consumption *in restaurants*, *bars*, *lounges*, or *parties*. This feature of alcohol consumption information in the CPHS is particularly useful as this paper assesses an alcohol control policy that shuts down hard liquor-serving bars. Using information on household expenses on alcohol for consumption at home, we analyze if there is any switch from drinking in bars to home drinking. Unfortunately, the data does not provide information on the alcohol types, restricting us from examining the potential substitution pattern across alcoholic drinks.

3.2 Intimate Partner Violence

Physical violence is another outcome of interest, and existing literature suggests that alcohol consumption increases the likelihood of domestic violence.[14] For the post-treatment period, the DHS-4 and DHS-5 surveys identify intimate partner violence both at the extensive margin - whether a woman experienced different types of intimate partner violence and at the intensive margin - the frequency of domestic violence over the past 12 months. Physical acts of intimate partner violence include whether a woman has ever been (i)-pushed, shaken, or had something thrown, (ii)-slapped, (iii)-punched with a fist or hit by something harmful, (iv)-had her arm twisted or hair pulled, (v)-kicked or dragged, (vi)-strangled or burnt, and (vii)-threatened with knife/gun or another weapon. For the pre-treatment period, the DHS-2 reports an aggregate variable on the incidence of physical violence and its frequency without details on the types of physical violence.[15]

The DHS reports intimate partner violence at the extensive and intensive margins. At the extensive margin, DHS indicates domestic violence being ever-experienced by a woman until the survey date. For domestic violence at the intensive margin, DHS includes the frequency of intimate partner violence experienced in the last 12 months, including (i) never, (ii) ever-experienced before the past 12 months, (iii) sometimes in the last 12 months, and (iv) often in the last 12 months. Noteworthy here is the overlap between the timing of the partial alcohol ban and women's experience of intimate partner violence captured in the DHS surveys. Recall that the policy started on April

[14]For example, Luca et al. (2015) show that the state-level alcohol prohibitions in India and the associated decline in husband's alcohol consumption is significantly associated with a decrease in the likelihood of the husband beating his wife using India's DHS-2 (1998-1999) and DHS-3 (2005-2006) datasets.

[15]Even though DHS-4 and DHS-5 include data on sexual violence, DHS-2 does not. As a result, we limit our analysis to physical violence only. Moreover, the surveys do not provide any information about domestic violence experienced by male respondents.

1, 2014. The DHS-4 wave was conducted in Kerala, Karnataka, and Tamil Nadu between March and November 2016, February and July 2015, and February and June 2015, respectively. Thus, the DHS-4 survey coding a woman's experience in the last 12 months might have captured her experience in, for example, February 2014, which was before the policy implementation. But this timing inconsistency should be negligible for intimate partner violence experiences in the last 12 months as there is only a month of mismatch at most.[16] Hence, we construct a dummy for physical violence experienced in the last 12 months, regardless of the frequency, by combining (iii) and (iv) to estimate the impact on the overall experience in the last year. The DHS-5 sample used in our analysis covers periods in 2019. Thus, domestic violence experience over the past 12 months captures incidences that occurred in 2018, after the policy reversal in 2017. As a result, there are no timing-related issues for domestic violence experiences during the last 12 months in DHS-5. Table 3 shows the correlation between men's and women's alcohol consumption and physical violence against women at the extensive and intensive margins. First, the likelihood of intimate partner violence at the extensive margin is positively associated with alcohol consumption by the male partner. Second, and interestingly, a woman's self-drinking is positively associated with her risk of experiencing intimate partner violence at the extensive margin. However, the magnitude is smaller when compared to the male's drinking status. Third, alcohol consumption by the male partner is also positively associated with the frequency of violence at the intensive margin. All these associations are statistically significant at the 1% level.

Table 3: Correlation between Alcohol Use and Physical Violence

	Wife herself drinks (1)	Husband drinks (2)
Physical violence ever experienced	0.030***	0.285***
	22772	22767
Physical violence ever experienced before the past 12 months	0.020***	0.151***
	22771	22766
Physical violence ever experienced in the last 12 months	0.025***	0.231***
	22770	22765
Physical violence experienced sometimes in the last 12 months	0.017**	0.211***
	22770	22765
Physical violence experienced often in the last 12 months	0.037***	0.136***
	22769	22764
Frequency of physical violence	0.033***	0.234***
	22772	22767

Notes: Based on India's DHS datasets 2005-2006, 2015-2016, and 2019-2021. The table presents the pair-wise correlation of a woman's and her husband's drinking status with a woman's incidence of physical intimate partner violence with different timings and frequencies in Kerala, Karnataka, and Tamil Nadu. The number of observations is also provided. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

[16]Domestic violence incidences captured by the "ever" experiences in the DHS-4 survey could include experiences well before the policy implementation. The status and frequency of domestic violence experiences in the last 12 months will thus be our main focus. However, we also examine ever-experienced physical violence and those experienced before the past 12 months.

3.3 Treatment Intensity

The key regressor that we focus on is the interaction of the post-treatment dummy and the intensity of the hard liquor ban that varies across districts in Kerala. The district-level treatment intensity measure is constructed based on district d's share of hard liquor consumption in Kerala's total consumption of hard liquor in the pre-treatment period.[17] We use 2012 as a pre-treatment period for the construction of the treatment intensity measure since the 68th round of NSS conducted over the period 2011-2012 is the latest wave before the implementation of the policy in 2014. Using the NSS data, we aggregate the amount of households' hard liquor consumption (in liters) at the district level and then calculate each district's share of hard liquor consumption in Kerala's total hard liquor consumption. This variable thus approximates the policy intensity or the degree to which each district was affected by the policy. The rationale behind this intensity measure is guided by the intuition that districts within the treated State with a higher share of hard liquor consumption before the treatment are expected to be affected more by the hard liquor ban. The novelty of this approach is that it introduces variation across districts within the treated state. We are the first to use this approach of capturing the intensity of alcohol ban based on pre-treatment alcohol consumption, while previous works, e.g., Khurana and Mahajan (2022), construct the intensity of alcohol ban based on number of bars affected by the policy.

In addition to our baseline measure of treatment intensity based on consumption share, we also construct two alternative measures of treatment intensity for robustness checks discussed in Section 4.4. The first alternative measure is based on district d's per-capita consumption of hard liquor. The second alternative measure is the number of bars closed down, which provides a direct measure of treatment intensity.[18] We merge these three district-specific treatment intensity measures with the DHS and CPHS datasets at the district level.

3.4 Control Variables

Consistent with the literature on the determinants of intimate partner violence (see Appendix A), we include as controls a set of individual's characteristics (age, education level, employment status, marital status, religion, and place of residence (urban or rural)) and household's characteristics

[17] Since the policy is at the state level and we have only two control states and few pre-treatment periods, the problem of a small number of clusters persists even if we employ wild cluster bootstrapping or cluster at the district level when we use state-level treatment measures. Thus, we define our treatment at the district level.

[18] We obtained data on the total number of bars closed down from the Excise Department of Kerala and matched it to the district-specific number of bars closed down in Khurana and Mahajan (2022)'s dataset - the latter provided to us by Kanika Mahajan and Saloni Khurana, for which we are grateful. However, the number of hard liquor serving bars closed down in Thiruvananthapuram district was not available in either of the two datasets. We thus calculate the number of bars closed down in the Thiruvananthapuram district by subtracting the number of bars closed down in other districts from the total number of bars closed down. Interestingly, most hard liquor serving bars shut down in the Thiruvananthapuram district, introducing the most significant variation in treatment intensity.

(wealth and size) - all of which are gleaned from the DHS survey.[19]

Although we control for time-varying household characteristics such as household wealth, the changes in alcohol consumption and the associated change in intimate partner violence could be driven by changes in local economic activities on top of the policy changes. As a result, we also include district-level real gross domestic product (net of depreciation) per capita to partial out the impact of any local economic activity changes at the district level to isolate the effect of the partial liquor ban. Controlling for this direct measure of local economic activities also partially addresses the impacts of any other changes, such as the effects of elections or changes in a district's political party representatives, that might be associated with economic activities at the district level. In India, the State government is formed by a single party or an alliance of parties post-election. Thus, different districts within a State might have legislative representation from different political parties due to alliances formed after elections. Put differently, state-by-time fixed effects capture variation across time at the state level but fail to account for the time-varying changes in local political representation at the district level. Our additional control of district-level time-varying economic activities measure thus partials out the impacts of any such changes at the district level. The data on district-specific net domestic product per capita, in turn, is collected from the statistical yearbook (annual economic surveys) and deflated by the 2011-2012 constant price for each of the States (Kerala, Karnataka, and Tamil Nadu) under consideration in our alcohol consumption and domestic violence regressions.

4 Effect on Alcohol Consumption

This section examines the impacts of the policy episodes on household expenditure on alcohol, including the effects on alcohol consumption in bars and at home. In doing so, we separately estimate the short-run effects of the intervention, the impacts of policy removal, and the long-run effects of the policy using different time windows.

4.1 Empirical Strategy

We employ difference-in-differences event-study specifications to quantify the effects on alcohol consumption due to our high-frequency panel data at the granular level from the CPHS on household's monthly alcohol consumption. The estimating equations essentially compares households

[19] In our analysis using the DHS data, i.e., estimating the program effects on intimate partner violence, we employ household wealth as the DHS data does not directly report household income. However, some of the principal components of the household wealth index include assets such as a refrigerator, television, washing machine, electric fan, air conditioner or cooler, and computer that incur operational expenses like electricity and maintenance costs. Thus, for example, as shown in Basu et al. (2024), household wealth and income are likely positively correlated and comparable in the context of India. Additionally, in our analysis leveraging the CMIE's Consumer Pyramids matched with the Income Pyramids data, e.g., first-stage effects of the program on alcohol consumption, we use household income directly collected in the data. Finally, our analysis based on the NSS datasets uses household total expenditure as an indirect measure of household income.

in the treatment and control groups around the policy changes or the events. We use the State of Kerala as the treatment group and the neighboring state(s) as the control group. This strategy is in line with the existing studies that quantify the impact of alcohol control policies in India (for example, Dar and Sahay, 2018; Khurana and Mahajan, 2022; Chaudhuri et al., 2024).

Short-Run Effect of Partial Liquor Ban. We first estimate the following regression to examine the short-term impact of the policy:

$$\log(\text{Consumption}_{ht}) = \alpha + \sum_{\tau \neq -1; \tau=-3}^{\tau=38} \beta_\tau \times I_\tau \times \text{Treat}_d + \boldsymbol{X}'_{ht}\boldsymbol{\delta} + \boldsymbol{M}'_{d\times\text{Year}}\boldsymbol{\lambda} + \\ + \mu_h + \text{Year}_t + \text{Month}_t + \pi_s \times \text{Year}_t + \pi_s \times \text{Month}_t + \xi_{ht}, \tag{1}$$

where Consumption_{ht} is the value (in rupees) of household h's alcohol consumption in a month t between January 2014 and June 2017. Outcomes are in logs and include alcohol consumption in bars, alcohol consumption at home, and total alcohol consumption. The parameter of interest is β_τ, which captures the impact of the liquor ban in Kerala that was effective from April 2014 to June 2017. The terms, I_τ, are leads and lags in event time, with $\tau = -1$ as the reference category, and the policy event date (I_0) is April 2014. A continuous treatment variable, Treat_d, captures the treatment intensity for district d in the treated State of Kerala and takes a value of zero for districts in the neighboring States of Karnataka and Tamil Nadu, included in the control group (Figure B.1).

We use district-level treatment intensity for identification purposes. If we construct the treatment at the state level as the policy is a State regulation, we would have clustered the standard errors at the state level. It would have led us to a small number of clusters problem (Cameron et al., 2008) because our primary specification essentially relies on only three states (a treatment state, Kerala, and two comparison states, Karnataka and Tamil Nadu, neighboring the treatment state). Small-number-of-clusters problem is usually solved by adjusting standard errors using wild cluster bootstrapping (Cameron and Miller, 2015); however, the existing methods are applicable for small but more than 10-12 clusters, especially when one has a single-treated cluster and without many pre-treatment periods. For example, one could adjust standard errors using wild bootstrap cluster following Roodman et al. (2019) for LPMs and score wild cluster bootstrap following Kline and Santos (2012) for nonlinear models such as logit and probit regressions. However, based on simulation analysis, MacKinnon and Webb (2020) show that wild cluster bootstrap over-rejects when there is only one treated cluster. Methods for adjusting standard errors and making an inference with a single treated cluster are proposed; however, these methods work when there are multiple control states, e.g., 24 or more (Ferman and Pinto, 2019) and 15 or more (Hagemann, forthcoming), or there are one treated group and one control group but multiple pre-treatment periods (Ferman and Pinto, 2019). However, we have only one data point for the pre-treatment period and two control states. Therefore, the most promising way of overcoming the small-number-of-cluster issue is to construct district-level treatment variables and cluster standard errors by districts.

The vector $\boldsymbol{X}_{ht}$ contains time-varying household characteristics that determine the alcohol consumption, including household size, urban/rural dummy, age group, occupation group, education group, gender group, and (log) income per household member.[20] The vector $\boldsymbol{M}_{d\times\text{Year}}$ contains district d's characteristics varying over time, and we control for the district's (log) per capita gross domestic product (GDP, net of depreciation), which is expected to proxy many local changes such as political and environmental changes correlated with the economy. Taking advantage of the household-level panel structure of the data we use in this analysis, we control for household fixed effects, μ_h. The year and month fixed effects, Year_t and Month_t, nonparametrically capture all unobserved contributing factors of consumption varying across years and months, respectively. The interaction between the State dummy (π_s) with yearly (Year_t) and monthly (Month_t) dummies nonparametrically capture all changes at the State level over time, even within a year, such as liquor tax policies.[21] The error term, ξ_{ht}, captures the remaining unobserved, time-varying, and household-specific determinants of alcohol consumption. The standard errors are clustered by districts to allow for heteroskedasticity and serial correlation within clusters, assuming that treatment is varied by districts (Bertrand et al., 2004; Angrist and Pischke, 2009).[22]

Effect of Policy Removal. The key contribution of this paper is to understand the long-term consequences (if any) of a policy affecting alcohol purchases. Toward this end, we examine whether the partial alcohol ban had any persistent effect. Our analysis proceeds in two steps: first, the impact of the policy removal, and second, the net impact of the partial alcohol ban by comparing pre-ban and post-removal levels of outcome.

To estimate the impact of policy reversal, we run the following event-study specification:

$$\begin{aligned}\log(\text{Consumption}_{ht}) = \alpha + \sum_{\tau\neq-1;\tau=-39}^{\tau=29} \beta_\tau \times I_\tau \times \text{Treat}_d + \boldsymbol{X}'_{ht}\boldsymbol{\delta} + \boldsymbol{M}'_{d\times\text{Year}}\boldsymbol{\lambda} + \\ + \mu_h + \text{Year}_t + \text{Month}_t + \pi_s \times \text{Year}_t + \pi_s \times \text{Month}_t + \xi_{ht},\end{aligned} \tag{2}$$

where $t \in \{\text{April 2014}, \text{December 2019}\}$, and the event date of policy removal (I_0) is July 2017.

[20] Household income was obtained from the Income Pyramids and merged with Consumer Pyramids by household and year-month.

[21] The government-owned companies with the exclusive privilege of wholesale and retail supply of hard liquors, such as IMFL and FMFL, including BEVCO for Kerala, TASMAC for Tamil Nadu, and KSBCL for Karnataka, play a substantial role in the distribution and sales of liquors throughout the three states under investigation. For example, Tamil Nadu State Marketing Corporation Limited (TASMAC) runs about 5000 outlets. The Kerala State Beverages (M&M) Corporation Limited (BEVCO) has 23 FL-9 licensed warehouses and 265 retail outlets with FL-1 licenses. Also, the Karnataka State Beverages Corporation Limited (KSBCL) distributes liquor with 71 Indian-made liquor warehouses across the state, with a total turnover of 28,066 crore rupees (equivalent to approximately $3.5 billion) for 2019-2020 (KSBCL Annual Report). These indicate that a major portion of liquor-related decisions are made at the state level by State-owned monopolies in these three States.

[22] Our sample contains 14, 28, and 30 districts in Kerala, Karnataka, and Tamil Nadu, respectively. The number of clusters in our baseline specification is thus 72, and it is large enough to make a credible inference since 42 clusters are considered large enough (Angrist and Pischke, 2009).

Other variables are the same as those in equation (1). The control group consists of households in Karnataka only since Tamil Nadu had a parallel alcohol ban in early 2017.[23,24]

Long-Run Effect of Partial Liquor Ban. We evaluate the net impact of the partial alcohol ban and the effect of the policy reversal by comparing the pre-ban period with a post-reversal period in the treatment and control groups and ignoring the period over which the policy was in place. Fictitiously considering that the policy was still in effect even after its reversal allows us to examine the leftover impact of policy reversal after netting out the policy impact. This exercise enables us to evaluate whether the potential rebound in outcomes following the policy reversal exceeds, is less than, or equal to the pre-ban level.

We estimate the long-term impact of Kerala's partial liquor ban using the following regression:

$$\begin{aligned}\log(\text{Consumption}_{ht}) = \alpha + \sum_{\tau\neq-1;\tau=-3}^{\tau=29} \beta_\tau \times I_\tau \times \text{Treat}_d + \boldsymbol{X}'_{ht}\boldsymbol{\delta} + \boldsymbol{M}'_{d\times\text{Year}}\boldsymbol{\lambda} + \\ + \mu_h + \text{Year}_t + \text{Month}_t + \pi_s \times \text{Year}_t + \pi_s \times \text{Month}_t + \xi_{ht},\end{aligned} \tag{3}$$

where the time frame t covers periods of pre-ban (January-March 2014) and post-policy removal (July 2017-December 2019), and the event date (I_0) is July 2017, i.e., the date of policy removal. Similarly, other variables are the same as those in the previous two regressions. The control group consists of households in Karnataka only, similar to equation (2).

4.2 Identification and Assumptions

Several assumptions are needed for a difference-in-differences (DID) event-study estimate to capture a causal effect in our setting. First, in the absence of the policy event, the outcome variables in the treatment and control groups should change over time in a parallel fashion, conditional on covariates. This common trend assumption is the key to the DID event-study estimator. Thus, conditional on covariates, treatment and control groups should be comparable without any other changes occurring in the treated and control groups over the pre- and post-treatment periods that could have affected the outcomes. Second, given that we have a continuous treatment variable, districts in the treated state with different levels of treatment intensities (or "doses") should not have underlying differences that lead to different doses before the treatment, conditional on covariates. Third, treatment should not have any spillover impact on the control group.

[23] In 2016, around 500 out of 6800 liquor shops were closed, and the business hours of state-run liquor stores were reduced. Another 500 liquor shops shut their doors in February 2017. For details, see `https://www.firstpost.com/india/tamil-nadu-govt-to-shut-down-500-liquor-shops-from-19-june-2843154.html`.

[24] Excluding Tamil Nadu from the control group reduces the number of districts from 72 to 44, and even though 44 clusters are considered large enough according to Angrist and Pischke (2009), we also employ Roodman et al.'s (2019) approach of wild cluster bootstrap to examine the extent to which the standard errors are affected due to the change. We find that the statistical inferences remain unchanged.

Since information on household alcohol consumption from the CPHS spans since January 2014, we have only three months of pre-ban data. While we show that the parallel pre-trends assumptions are plausible for those two pre-ban months (the remaining pre-ban month serves as a reference period) using this dataset, the CPHS is thus not sufficient to check the parallel pre-trends in alcohol consumption. Therefore, we conduct event study analysis to test an assumption of parallel pre-trends in household consumption of alcohol and hard or foreign/refined liquor using the National Sample Survey (NSS) data over a decade before the liquor ban (2001-2002 through 2011-2012). The NSS dataset does not report the location of liquor consumption, unlike the CPHS, and as a result, we consider total consumption when analyzing the parallel pre-trends.

In the following, we discuss and test each of these assumptions in detail in our context. Given that the NSS data covers only pre-ban periods, the focus here is on checking the identification assumption for the specification for the short-run impact of the ban.

Standard Parallel Trend Assumption. Using data on household consumption for nine pre-policy periods, $\tau = -12, \ldots, -7, -6, -4, -3$, we run the following regression:

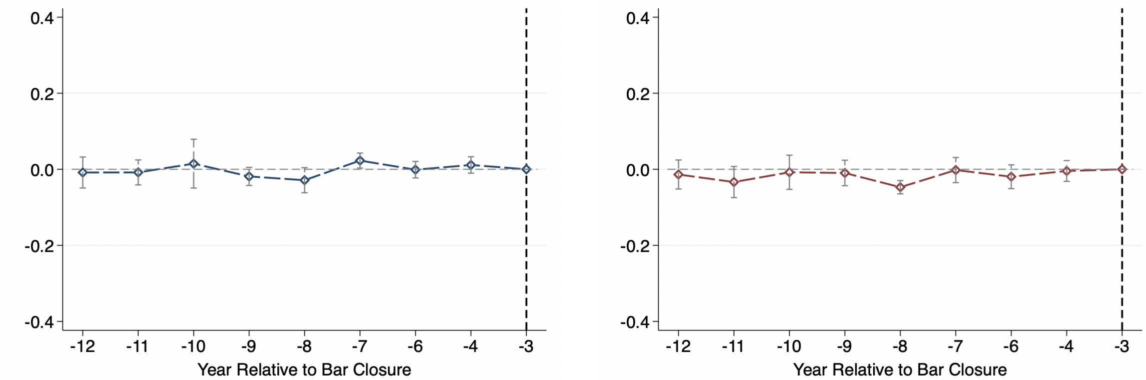
$$\begin{aligned}\log(\text{Consumption}_{hdst}) = \alpha + \sum_{\tau \neq -3; \tau = -12} \delta_\tau \times I_\tau \times \text{Treat}_d + \boldsymbol{X}'_{hdst}\boldsymbol{\gamma} + \boldsymbol{M}'_{dt}\boldsymbol{\lambda} + \\ + \theta_d + \pi_{st} + \mu_t + \varepsilon_{ht},\end{aligned} \tag{4}$$

where $\text{Consumption}_{hdst}$ is the amount (in liters) of monthly alcohol consumption for household h living in district d of state s in year t, I_τ are lags in event time, with $\tau = -3$ as the reference year. The consumption outcomes include total alcohol consumption and foreign/refined liquor consumption. The vector $\boldsymbol{X}_{hdst}$ contains a set of household characteristics, including age, gender, education, and marital status of household head, household size, scheduled caste or tribe, urban/rural dummy, religion of the household, and monthly per capita expenditure quintile. The district, θ_d fixed effect nonparametrically controls for all unobserved contributing factors to alcohol consumption varying across districts but common over time. The remaining variables are similar to those in equation (1). Figure 3 shows that parallel pre-trends in total alcohol consumption (panel (a)) and hard liquor consumption (panel (b)) are strongly plausible: compared to the base year (three years before the implementation of the partial alcohol ban), the subsequent changes in household-level alcohol consumption between the treatment and control groups are not significantly different for most of the pre-closure years. It is worth noting that there are some differences between the treatment and control states, for example, in education level, institutional quality, and political view. But we argue that the parallel pre-trends assumption is plausible in our setting conditional on covariates. For example, the district and state-by-time fixed effects would control for the institutional quality differences between control and treatment states.[25]

[25] Tariffs on imported liquor also remain high over the pre-treatment periods under consideration, especially from 2003 to 2010 (Dhanuraj and Kumar, 2014), supporting the parallel pre-trends in consumption of hard liquor.

Figure 3: Event Study—Test of Parallel Pre-Trend in Household Alcohol Consumption

(a) Total alcohol consumption (b) Hard liquor consumption

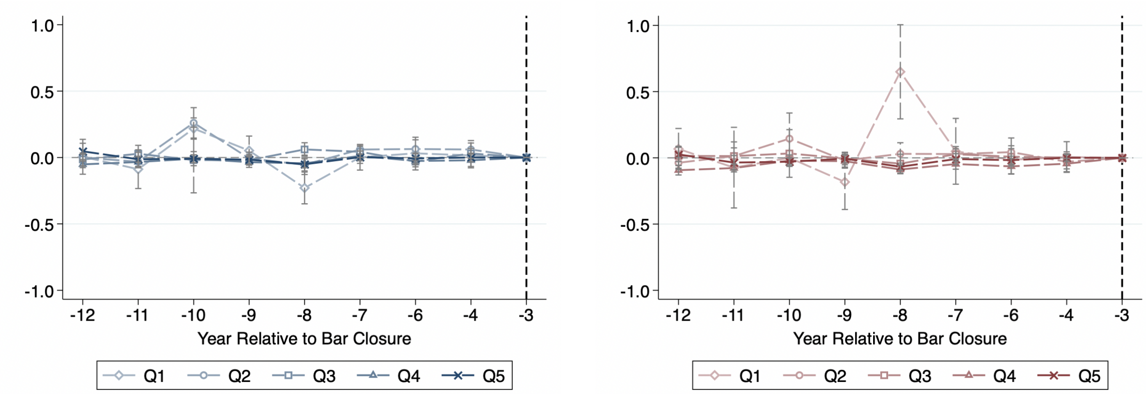


Notes: The figure shows results from event study analysis testing parallel pre-trends in household's total alcohol consumption and consumption of hard liquors in Kerala and its neighboring states (Karnataka and Tamil Nadu). The analysis uses the NSS for nine years before Kerala's liquor ban (2001-2002 through 2011-2012 with gaps of 2008-2009 and 2010-2011), with a base year of 2011-2012. The data includes all 14 districts in Kerala and the 26 and 28 districts in Karnataka and Tamil Nadu, respectively. All specifications control for unreported household covariates, district-specific (log) real GDP per capita (net of depreciation), district and year fixed effects, state-by-year FEs, and a constant term. Household covariates include age, gender, education, and marital status of household head, household size, scheduled caste or tribe, urban/rural dummy, religion of the household, and monthly per capita expenditure quintile. Standard errors are clustered by districts, and 95% confidence intervals are shown.

As suggested by Olden and Møen (2022), parallel pre-trend assumption has to be satisfied for heterogeneous groups of households for our specifications estimating heterogeneous treatment effects. Figure 4 shows that the parallel pre-trend assumption is plausible for families with different incomes for alcohol consumption (panel (a)) and consumption of hard liquor (panel (b)).

Figure 4: Event Study—Test of Parallel Pre-Trend in Household Alcohol Consumption among Heterogeneous Households

(a) Total alcohol consumption (b) Hard liquor consumption



Notes: The figure illustrates results from event study regressions heterogeneous by household income quintiles testing parallel pre-trends in total alcohol consumption and consumption of hard liquor. Household income is measured by the household's monthly per capita expenditure. Standard errors are clustered by districts, and 95% confidence intervals are shown.

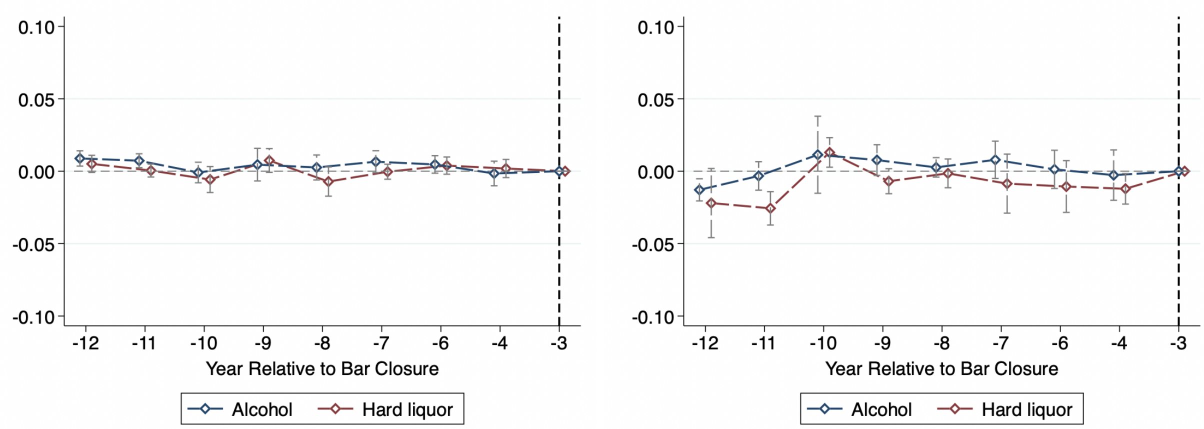
Strong Parallel Trend Assumption. Since our treatment variable is continuous, we also need to check the "strong" parallel trends assumption as suggested by Callaway et al. (2024).[26] We check the

[26] Callaway et al. (2024) show that treatment groups with different "doses" of treatment could be inherently different

strong parallel pre-trends assumption by splitting the treated districts into two categories: above and below the median dosages of treatment intensity and separately comparing them with the control group.[27] Figure 5 shows that households' total alcohol consumption and hard liquor consumption in treated districts with high (above median) and low (below median) doses are reasonably comparable, conditional on covariates relative to the control group before the treatment.

Figure 5: Event Study—Test of "Strong" Parallel Pre-Trend in Household Alcohol Consumption

(a) Districts with high treatment intensity (b) Districts with low treatment intensity



Notes: Panels (a) and (b) show results from event study analysis testing strong parallel pre-trends in household consumption of hard liquor and total alcohol consumption for treated districts with "dose" of treatment above (high) and below (low) the median treatment intensity, respectively. We used district d's per capita consumption of hard liquor in 2012 as a treatment intensity variable in these event study regressions, and the qualitative results remain the same when we use the other two measures of treatment intensity. Standard errors are clustered by districts, and 95% confidence intervals are shown.

Stable Unit Treatment Value Assumption (SUTVA). For our DID event-study framework, stable assignment between the pre- and post-alcohol ban periods should also be satisfied. Thus, there should not be any migration between the treatment and control groups and any spillover treatment effects from the treatment to the control group. A negligible level of internal migration, especially intra-state migration in India (Munshi and Rosenzweig, 2016; Kone et al., 2018; Nayyar and Kim, 2018) supports ruling out the possibility of violating the assumption of stable assignment via migration from treatment to control group. Moreover, there are at least three reasons why spillover effects via cross-border sales/consumption and smuggling can also be ruled out. First, the population in Kerala is clustered along the coastline of the Arabian Sea (Figure 6a), and the regions closer to the border of the neighboring states are highly elevated or mountainous (Figure 6b).[28]

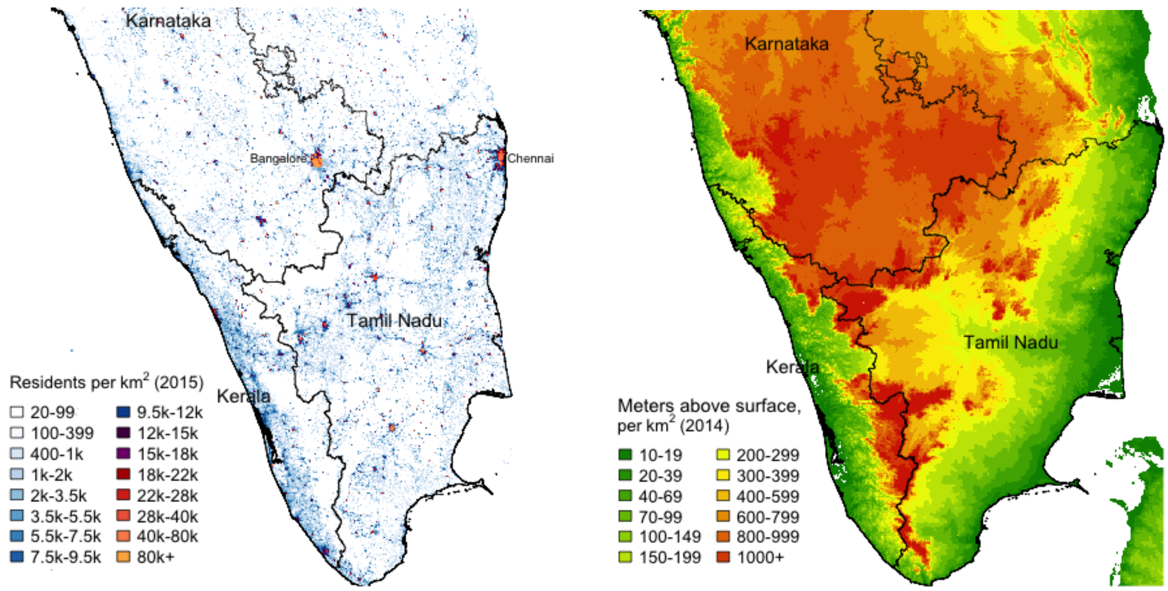
from each other, leading to "selection bias" in the estimates, and "standard" parallel trends assumptions are not strong enough to disentangle whether the change in outcomes is due to receiving higher treatment or due to other characteristics that make the unit to have higher treatment or dose.

[27] Treated districts with treatment doses above the median include Thiruvananthapuram, Wayanad, Pathanamthitta, Kollam, Ernakulam, Idukki, and Kottayam. Below median districts contain Alappuzha, Palakkad, Thrissur, Kasaragod, Kozhikode, Kannur, and Malappuram.

[28] The state of Kerala occupies the south-west corner of the Indian subcontinent with beaches spread along the 550

Figure 6: Population Density and Land Elevation in Treatment and Control Groups

(a) Population Density (b) Land Elevation



Notes: The left panel plots the distribution of population in treatment (Kerala) and control (Karnataka and Tamil Nadu) states using data from the Global Human Settlement Layer (GHSL). The right panel depicts the elevation of land in treatment (Kerala) and control (Karnataka and Tamil Nadu) states using data from the Pacific Islands Ocean Observing System (PacIOOS).

Second, people are less likely to spend long hours traveling to neighboring states to consume hard liquor, given that these varieties can be purchased in liquor shops and five-star hotels in Kerala without any restriction (just not sold in bars). Third, Kerala has a good police capacity to implement the policy, supporting the compliance of the policy and making the regulation less likely to be circumvented (Dar and Sahay, 2018). However, given the lower institutional quality and police capacity in Karnataka and Tamil Nadu, one might argue that smuggling hard liquor into Kerala is possible. However, after internalizing the transaction and transportation costs due to the weight and volume of hard liquors, potential legal penalties, and given that these hard liquors are legally available in liquor stores and five-star hotels (just not in bars) we can safely preclude the smuggling of hard liquors into Kerala as a profitable activity.

Nonetheless, we formally check whether there is any contagious effect of the prohibition on neighboring states by constructing the control group with (i) the interior districts of the neighboring States and (ii) the border districts of the neighboring States.[29] A positive treatment impact when border districts of neighboring States are used as a control group would imply that those border districts are crowded out. We then use the interior districts in the neighboring States to check whether our results change with the choice of remote districts as a control group. Our baseline

kilometers Arabian Sea coastline and runs about 580 kilometers in total length and between 35-120 kilometers in width. Thus, individuals living along the coastline are more likely to comply with the liquor ban.

[29]The border districts include Karnataka (Dakshina Kannada, Kodagu, Mysore, and Chamarajanagar) and Tamil Nadu (Nilgiris, Coimbatore, Tiruppur, Dindigul, Theni, Virudhunagar, Tirunelveli, and Kanniyakumari).

results are unaffected by the choice of these alternative control groups, implying that our setting is stable, i.e., there is no spillover from the treatment to the comparison group.[30]

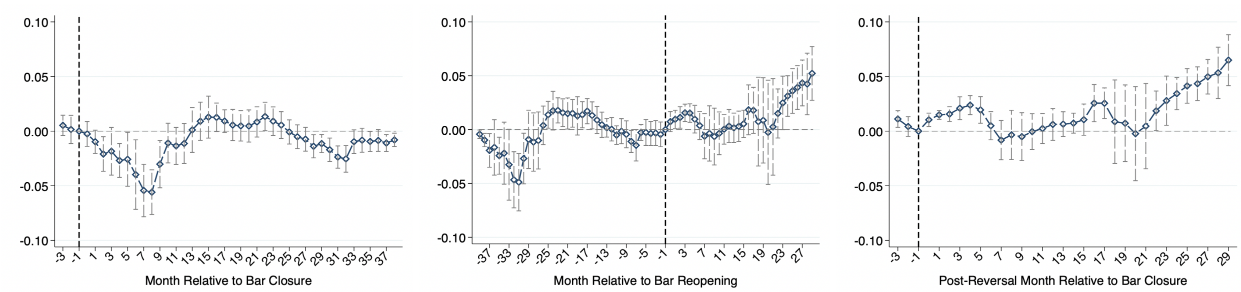
4.3 Results

Our analysis provides three sets of results, including the effects on alcohol consumption in bars (first-stage effect) and alcohol consumption at home.

First-Stage Effect on Alcohol Consumption in Bars. Figure 7 presents the short- and long-run impacts of the ban and the effects of policy removal on household alcohol consumption in bars. We see that liquor consumption in bars responds to the policy changes in the baseline.

Figure 7: Effects on Household Alcohol Consumption in Bars

(a) Short-Run Effects (b) Effects of Policy Reversal (c) Long-Run Effects



Notes: Panel (a) shows the effects of shutting down hard liquor-serving bars during the policy period. Panel (b) presents the impacts of policy removal. Panel (c) reports the policy effects over the period when the policy was no longer effective, i.e., long-run effects. The dependent variable in all regressions is (log) expenditure by a household on the purchase of alcohol for consumption in bars. The treatment intensity in these event study regressions is our baseline measure based on district d's share of hard liquor consumption in the state's total consumption in 2012. The control group includes households in neighboring states of Karnataka and Tamil Nadu in panel (a) and in Karnataka only in panels (b) and (c). The analysis uses household-level monthly panel data from the Consumer Pyramids. All specifications include baseline controls and fixed effects. Standard errors are clustered by districts, and 95% confidence intervals are shown.

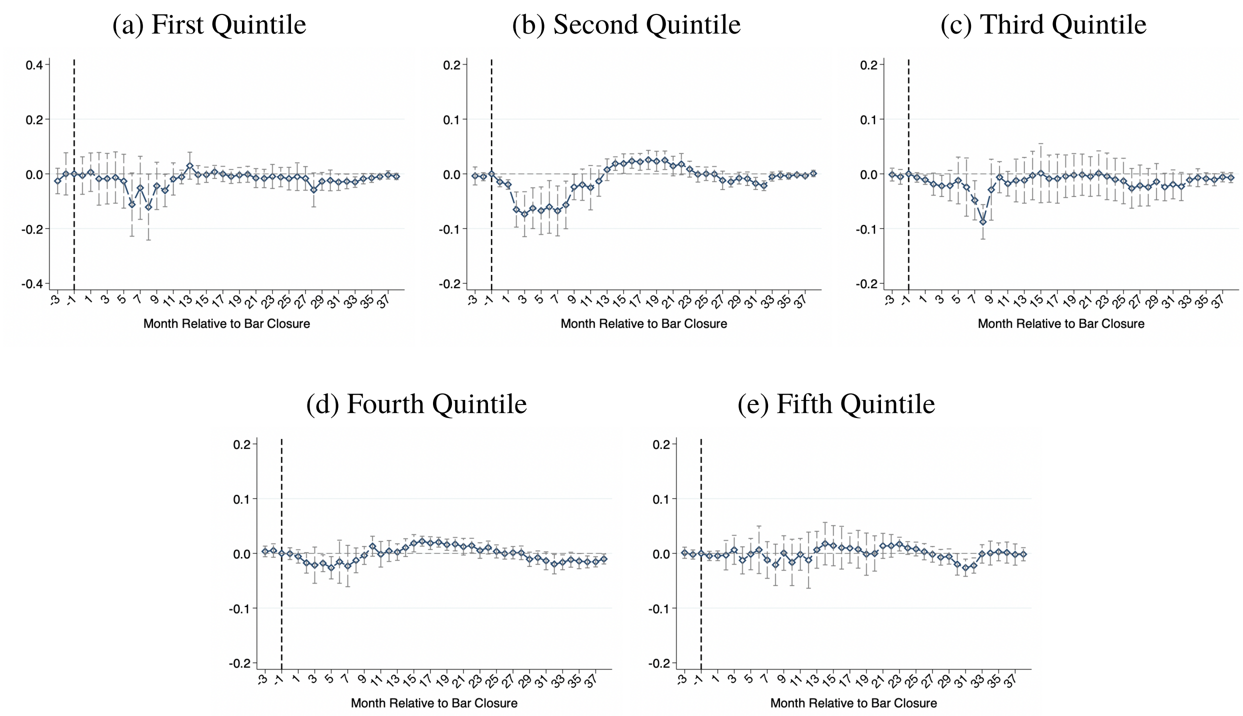
The results on the short-run effects, shown in Figure 7a, suggest that household alcohol consumption in bars fell sharply in the first eight months after introducing the ban. Starting from the 9th-month post-ban, the fall in liquor consumption weakens and goes back to its pre-ban level since the 10th month and generally stays there without exceeding the pre-ban level during the rest of the policy period. The pattern of alcohol consumption during the ban in Kerala is consistent with results from Miron and Zwiebel (1991) in the U.S. Results on the effects of policy removal, illustrated in Figure 7b, show a rebound in alcohol consumption in bars: a slight increase right after the bar reopening and a sharp and persistent increase after two years post-policy reversal, i.e., in June-December 2019. In the long run (after the policy reversal), household alcohol consumption in bars increased compared to the pre-ban level (Figure 7c). The consumption-increasing impact in the long term is stronger and more persistent than that observed in Figure 7b. These results from

[30] Khurana and Mahajan (2022) also test whether people are traveling to border districts in neighboring states to drink hard liquor in bars by excluding the border districts in Karnataka and Tamil Nadu from the control group. They similarly found no significant migration between the treatment and control regions.

the “first stage” effects indicate an overshooting impact of policy removal on liquor consumption in bars, and this overshooting effect outweighs the fall in alcohol consumption during the first nine months of the ban.

We then estimate the heterogeneous effects by household income as the targeted drink, hard liquor, is relatively expensive and is more afforded by higher-income households. We split our sample into five groups based on quintiles of household income distribution. In the short-run, the immediate fall in alcohol consumption in bars is concentrated among households in the second, third, and fourth quintiles of the income distribution (Figure 8, panels (b)-(d)). Household’s alcohol consumption in bars in the first quintile did not respond, indicating that they are not primary consumers of hard liquor in bars (Figure 8, panel (a)). The highest-income households in the fifth quintile are also not affected by the policy even though they are likely to be the primary consumers of expensive hard liquors (Figure 8, panel (e)). It may well be that individuals in this group predominantly consume hard liquor in five-star hotels and were thus unaffected by the policy.

Figure 8: Short-Run Effects on Household Alcohol Consumption in Bars, Heterogeneous by Household Income

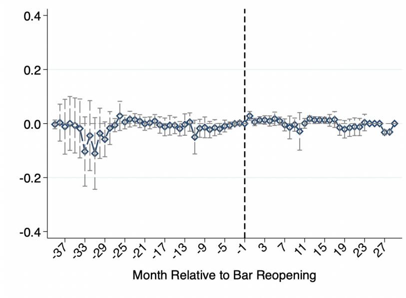


Notes: The figure presents the short-run effect of closing hard liquor-selling bars on (log) household expenditure on alcohol for consumption in bars during the policy period heterogeneous by household income. The treatment intensity is our baseline measure based on the district’s share of hard liquor consumption in the state’s total consumption. Panel (a)-(e) includes households in the first-fifth quintile of household income distribution, respectively. The event date I_0 in these event-study specifications is April 2014, and the time frame is between January 2014 and June 2017.

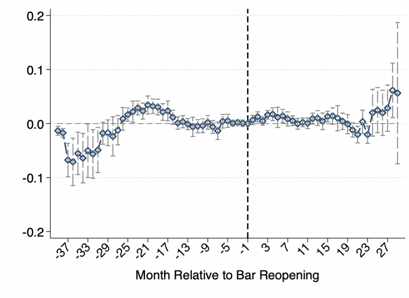
For the impact of policy reversal, shown in Figure 9, there was an immediate increase in alcohol consumption in bars for households in the second and third quintiles for up to 6 months. A more persistent and sharper increase in liquor consumption in bars post-policy removal is concentrated among the highest-income households in the fifth quintile.

Figure 9: Effects of Policy Reversal on Household Alcohol Consumption in Bars, Heterogeneous by Household Income

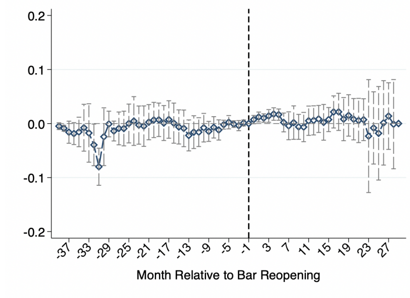
(a) First Quintile



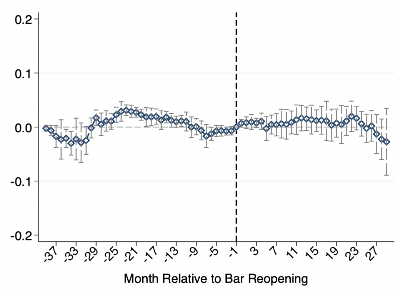
(b) Second Quintile



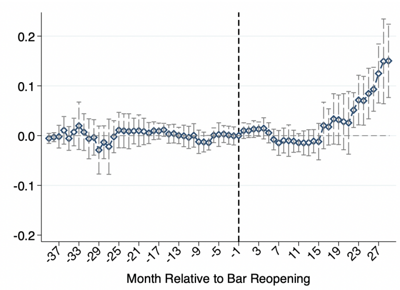
(c) Third Quintile



(d) Fourth Quintile



(e) Fifth Quintile

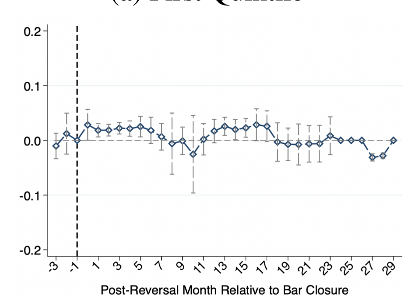


Notes: The figure presents the effect of policy reversal or reopening of bar hotels on (log) household expenditure on alcohol for consumption in bars heterogeneous by household income. The treatment intensity is our baseline measure based on the district's share of hard liquor consumption in the state's total consumption. Panel (a)-(e) includes households in the first-fifth quintile of household income distribution, respectively. The event date I_0 in these event-study specifications is July 2017, and the time frame is between April 2014 and December 2019.

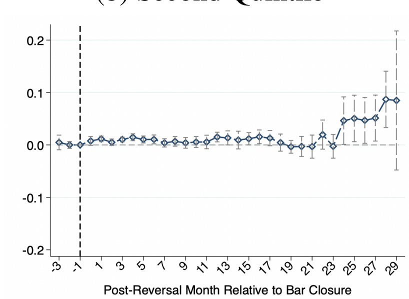
Finally, in the long run, we find that the overshooting increase in liquor consumption in bars is concentrated among the highest-income households in the fifth quintile (Figure 10, panel (d)). We also see some positive impact among households below the fifth quintile right after the policy removal (Figure 10, panels (a)-(e)).

Figure 10: Long-Run Effects on Household Alcohol Consumption in Bars, Heterogeneous by Household Income

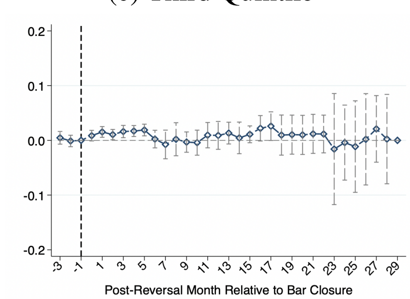
(a) First Quintile



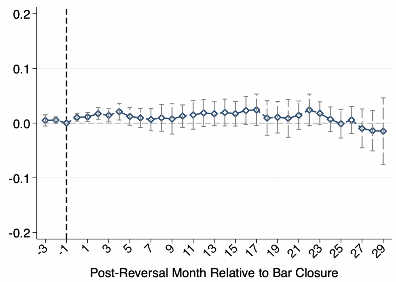
(b) Second Quintile



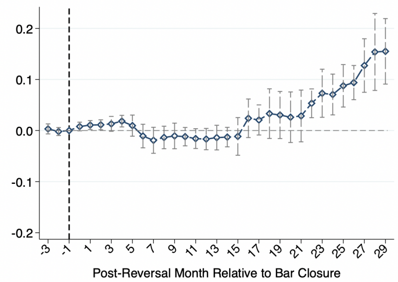
(c) Third Quintile



(d) Fourth Quintile



(e) Fifth Quintile

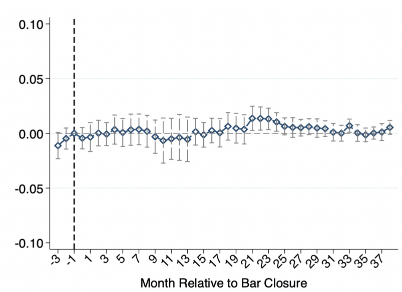


Notes: The figure presents the long-run effect of shutting down hard liquor-selling bars on (log) household expenditure on alcohol for consumption in bars heterogeneous by household income. The treatment intensity is our baseline measure based on the district's share of hard liquor consumption in the state's total consumption. Panel (a)-(e) includes households in the first-fifth quintile of household income distribution, respectively. The event date I_0 in these event-study specifications is Jul 2017, and the time frame covers Jan-Mar 2014 (pre-treatment) and Jul-Dec 2019 (post-treatment).

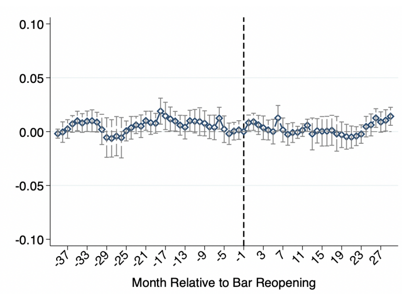
Effect on Alcohol Consumption at Home. Alcohol consumers who reduced their consumption in bars could switch to different venues or purchase from liquor stores and drink somewhere else, such as home drinking. We investigate whether there is any transition from bars to home drinking by estimating the effects of shutting down hard liquor-serving bars on household expenditure on alcohol for consumption at home. Figure 12 presents the results on the average effects over the short and long run. We find no evidence of a strong transition of alcohol consumption from bars to home. The heterogeneous effects by household income over the different episodes of the policy, shown in Figures B.3-B.5, suggest that home drinking was not strongly affected among households with different incomes.

Figure 12: Effects on Household Alcohol Consumption at Home

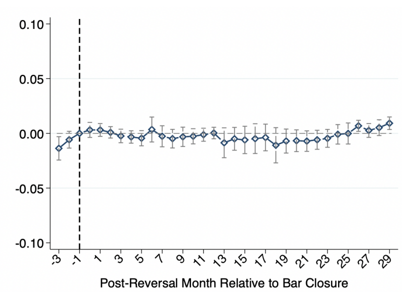
(a) Short-Run Effects



(b) Effects of Policy Reversal



(c) Long-Run Effects



Notes: Panel (a) shows the effects of shutting down hard liquor-serving bars during the policy period. Panel (b) presents the impacts of policy removal. Panel (c) shows the policy effects when the policy was no longer effective, i.e., long-run effects. The dependent variable in all regressions is (log) expenditure by a household on the purchase of alcohol for consumption at home. The treatment intensity in these event study regressions is our baseline measure based on district d's share of hard liquor consumption in the state's total consumption in 2012. The control group includes households in neighboring states of Karnataka and Tamil Nadu in panel (a) and in Karnataka only in panels (b) and (c). All specifications include baseline controls and fixed effects. Standard errors are clustered by districts, and 95% confidence intervals are shown.

4.4 Robustness

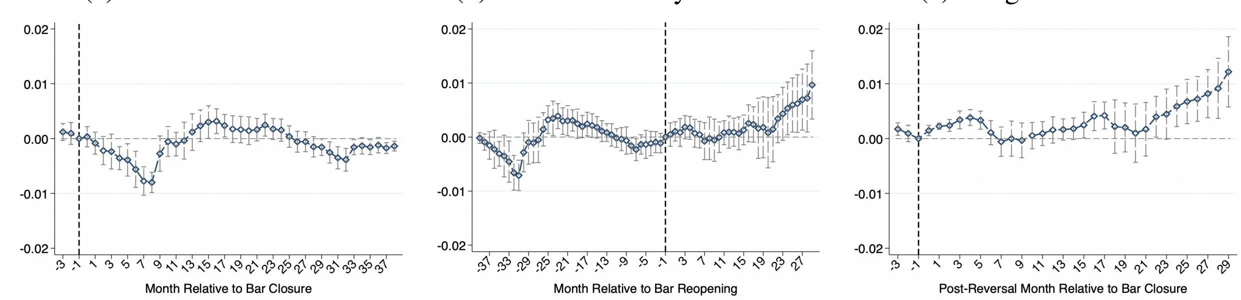
We conduct a battery of sensitivity checks to underline the robustness of our results, focusing on the effect on alcohol consumption in bars. We check the robustness by (i) using alternative treatment intensity measures based on district's average per capita consumption of hard liquor and number of liquor serving bars closed due to the ban, (ii) using only the State of Karnataka as the control group, and (iii) comparing interior/border districts in the treatment and control States via multiple permutations.

Alternative Treatment Intensity. We use two alternative measures of district-specific treatment intensity: (i) per-capita consumption and (ii) the number of hard liquor serving bars closed down.

Per-capita Consumption. Our baseline district-level treatment intensity variable based on districts' share in Kerala's total consumption of hard liquor could reflect the population differences across treated districts rather than an individual's drinking intensity. We thus perform a robustness check by using the per-capita consumption of hard liquor for each of the districts in Kerala as the treatment intensity measure. It measures the intensity of hard liquor consumption at the individual level, accounting for differences in population size among treated districts. The correlation coefficient between this alternative measure and our baseline measure of district-level treatment intensity is 0.73. Figure 13 presents the results from this analysis and shows that our baseline findings on the short- and long-term impacts of the policy and the effect of policy removal are remarkably robust to the use of this alternative treatment measure.

Figure 13: Effects on Household Alcohol Consumption in Bars
(Treatment intensity = District's per capita consumption of hard liquor)

(a) Short-Run Effects (b) Effects of Policy Reversal (c) Long-Run Effects



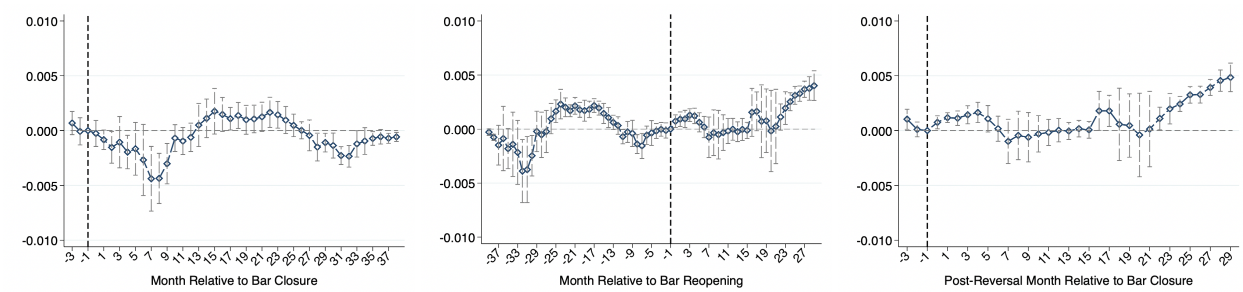
Notes: Panel (a) shows the effects of shutting down hard liquor-serving bars during the policy period. Panel (b) presents the impacts of policy removal. Panel (c) reports the policy effects over the period when the policy was no longer effective, i.e., long-run effects. The dependent variable in all regressions is (log) expenditure by a household on the purchase of alcohol for consumption in bars. *The treatment intensity in these event study regressions is based on the average consumption of hard liquor per 1000 population in 2012.* The control group includes households in neighboring states of Karnataka and Tamil Nadu in panel (a) and in Karnataka only in panels (b) and (c). The analysis uses household-level monthly panel data from the Consumer Pyramids. Each observation corresponds to (log) alcohol consumption in bars by household and year-month. All specifications include baseline controls and fixed effects. Standard errors are clustered by districts, and 95% confidence intervals are shown.

Number of Bars Closed Down. The number of bars selling hard liquor that were shut down in each district is strongly consistent with our baseline measure of district d's share of hard liquor consumption in Kerala's total consumption of hard liquor in the pre-treatment period with a correlation coefficient of 0.86. As shown in Figure 14, our results remain the same when using this second alternative treatment intensity measure.[31]

[31] In Figures C.1 and C.2, we also check the robustness of our results on the alcohol consumption at home to the two alternative treatment intensity measures, respectively. The results remain qualitatively the same.

Figure 14: Effects on Household Alcohol Consumption in Bars
(Treatment intensity = Number of bars closed down)

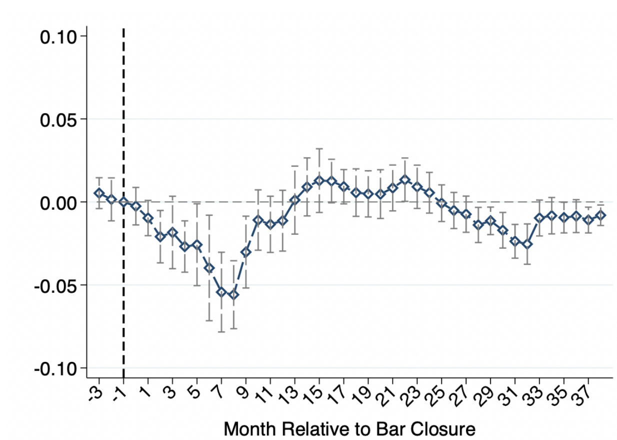
(a) Short-Run Effects (b) Effects of Policy Reversal (c) Long-Run Effects



Notes: Panel (a) shows the effects of shutting down hard liquor-serving bars during the policy period. Panel (b) presents the impacts of policy removal. Panel (c) reports the policy effects over the period when the policy was no longer effective, i.e., long-run effects. The dependent variable in all regressions is (log) expenditure by a household on the purchase of alcohol for consumption in bars. *The treatment intensity in these event study regressions is based on the number of bars closed down due to the policy at the district-level.* The control group includes households in neighboring states of Karnataka and Tamil Nadu in panel (a) and in Karnataka only in panels (b) and (c). The analysis uses household-level monthly panel data from the Consumer Pyramids. Each observation corresponds to (log) alcohol consumption in bars by household and year-month. All specifications include baseline controls and fixed effects. Standard errors are clustered by districts, and 95% confidence intervals are shown.

Alternative Control Group. There was a step-by-step shutdown of liquor shops in Tamil Nadu between 2016-2017. Since this policy overlaps with Kerala's partial liquor ban for several months, we check the robustness of our baseline results on the short-run impact by excluding Tamil Nadu and keeping only the state of Karnataka in the comparison group. This robustness check also serves as a foundation for the empirical specification to investigate the impact of the partial alcohol ban on alcohol consumption in the long term, i.e., after the policy reversal in July 2017. As shown in Figure 15, our results on the short-term effect of the ban remain the same regardless of the inclusion of Tamil Nadu in the control group.

Figure 15: Short-Term Effect on Alcohol Consumption in Bars (Control group = Karnataka)



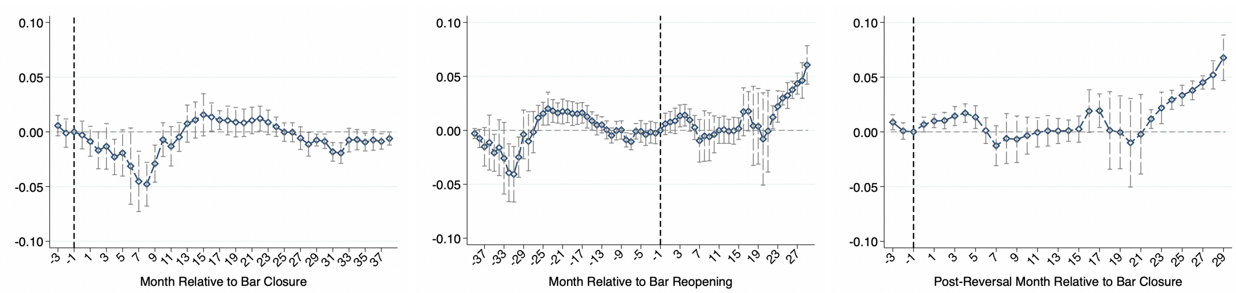
Notes: The figure shows the effects of shutting down hard liquor-serving bars on (log) expenditure by a household on the purchase of alcohol for consumption in bars during the policy period. The treatment intensity in this event study regression is our baseline measure based on district d's share of hard liquor consumption in the state's total consumption in 2012. The control group includes households in the state of Karnataka. The analysis uses household-level monthly panel data from the Consumer Pyramids. Each observation corresponds to (log) alcohol consumption in bars by household and year-month. All specifications include baseline controls and fixed effects. Standard errors are clustered by districts, and 95% confidence intervals are shown.

Cross Border Spillovers. We check whether our results change when only the bordering districts of neighboring States are used as control. This check is important because those close regions in the control group could be affected by the treatment through the immigration of individuals in response to the ban. It is also possible that smuggling takes place more readily across state lines. This exercise also provides credence to the stable assignment assumption discussed in Section 4.2. Our results remain unchanged, independent of whether we use border or interior districts of the neighboring States. Figures C.3-C.4 show that the treatment impacts on hard liquor consumption in bars and restaurants over the policy episodes are the same as the baseline.

Effects in Border and Interior Treated Districts. The effects of alcohol controls on drinking behavior could be heterogeneous by regions within the treated group. We might observe insignificant effects of partial liquor ban in border districts within the treated state of Kerala,[32] as individuals in the treated area can avoid the ban by traveling to the control group. This robustness check also enables us to confirm the assumption of stable assignment. We used three potential control districts: all, border, and interior districts of the neighboring state(s). However, we report results from using all districts in the control group, which are the same as those from the other two classes of districts in the control group. As presented in Figure 16, using the border districts of Kerala as the treatment group provides highly robust results.

Figure 16: Effects on Household Alcohol Consumption in Bars
(Treatment group = Border Districts of Kerala)

(a) Short-Run Effects (b) Effects of Policy Reversal (c) Long-Run Effects

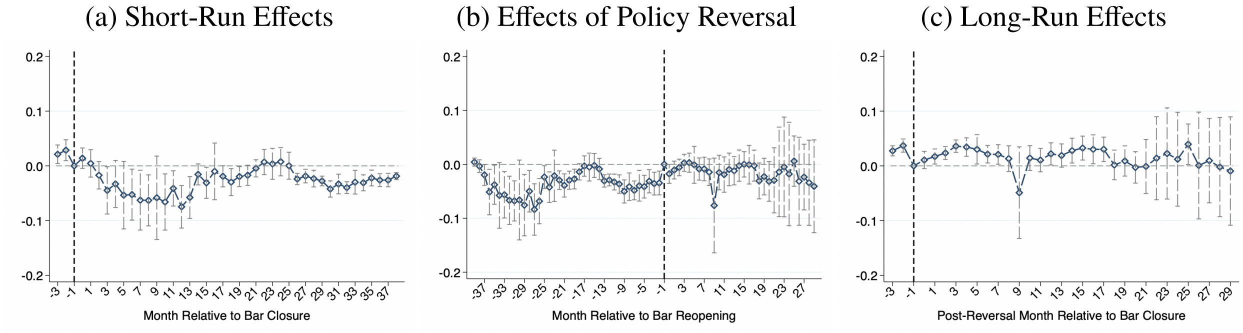


Notes: Panel (a) shows the effects of shutting down hard liquor-serving bars during the policy period. Panel (b) presents the impacts of policy removal. Panel (c) reports the policy effects over the period when the policy was no longer effective, i.e., long-run effects. The dependent variable in all regressions is (log) expenditure by a household on the purchase of alcohol for consumption in bars. The treatment intensity in these event study regressions is our baseline measure based on district d's share of hard liquor consumption in the state's total consumption in 2012. The treatment group consists of households in border districts of Kerala. The control group includes households in neighboring states of Karnataka and Tamil Nadu in panel (a) and in Karnataka only in panels (b) and (c). All specifications include baseline controls and fixed effects. Standard errors are clustered by districts, and 95% confidence intervals are shown.

Next, we use the interior districts of Kerala as the treatment group and compare them with the baseline control group, and results are shown in Figure 17. The short-term effect generally remains the same (panel (a)). However, the effect of policy reversal disappears, especially the effect that we

[32]The border districts of Kerala include Ernakulam, Idukki, Kannur, Kasaragod, Kollam, Malappuram, Palakkad, Pathanamthitta, Thiruvananthapuram, Thrissur, and Wayanad. The interior districts of Kerala include Alappuzha, Kottayam, and Kozhikode.

Figure 17: Effects on Household Alcohol Consumption in Bars
(Treatment group = Interior Districts of Kerala)



Notes: Panel (a) shows the effects of shutting down hard liquor-serving bars during the policy period. Panel (b) presents the impacts of policy removal. Panel (c) reports the policy effects over the period when the policy was no longer effective, i.e., long-run effects. The dependent variable in all regressions is (log) expenditure by a household on the purchase of alcohol for consumption in bars. The treatment intensity in these event study regressions is our baseline measure based on district d's share of hard liquor consumption in the state's total consumption in 2012. The treatment group consists of households in interior districts of Kerala. The control group includes households in neighboring states of Karnataka and Tamil Nadu in panel (a) and in Karnataka only in panels (b) and (c). All specifications include baseline controls and fixed effects. Standard errors are clustered by districts, and 95% confidence intervals are shown.

identified two years after the removal (panel (b)). Compared to the pre-ban level, we still identify an immediate increase in hard liquor consumption in bars and restaurants after the reversal; however, the sharp increase over the long term is invisible. These noisy results from using only interior districts of the treated state could be because there are only three such districts in the treatment group. The findings also imply that the sharp increase, in the long run, is driven by the border districts of Kerala.

5 Effect on Intimate Partner Violence

This section examines the spillover effects of the alcohol policy changes on intimate partner violence against women. We first describe the empirical strategy employed for quantifying the impacts and discuss the identification assumptions. Then, we present the baseline and heterogeneity results.

5.1 Empirical Strategy

The short- and long-run effects of the partial liquor ban and the impact of policy removal on intimate partner violence have been estimated using the difference-in-differences (DID) framework. For the domestic violence regressions, we use the treatment and control groups similar to the DID event study specification for consumption regressions.

Short-Run Effect of Partial Liquor Ban. To investigate the short-run causal impact of closing down hard liquor-selling bars on physical violence, we estimate the following equation:

$$\text{Domestic Violence}_{idst} = \alpha + \beta(\text{Treat}_d \times \mathbb{1}_{\{2015 \leq t \leq 2016\}}) + \boldsymbol{Z}'_{idst}\boldsymbol{\gamma} + \boldsymbol{X}'_{hdst}\boldsymbol{\delta} + \boldsymbol{M}'_{dt}\boldsymbol{\lambda} + \\ + \theta_d + \pi_{st} + \mu_t + \varepsilon_{idst} \tag{5}$$

where Domestic Violence_{idst} is one of the outcomes on incidence and frequency of physical violence against woman i living in district d of state s by the survey year $t = \{1999, 2015\text{-}2016\}$.[33] The outcome variables we consider include whether a woman has (i) ever experienced violence, (ii) ever experienced violence but not within the past 12 months, (iii) sometimes experienced violence over the past 12 months, (iv) often experienced violence over the past twelve months, (v) experienced any violence in the last 12 months, and finally (vi) the overall frequency of physical violence experienced by a woman captured as a categorical variable by combining (ii), (iii) and (iv), which respectively takes values of 1, 2, and 3. Treat_d is the baseline treatment intensity measure that varies across districts in the treated state. An indicator variable $\mathbb{1}_{\{2015 \leq t \leq 2016\}}$ equals to one if $2015 \leq t \leq 2016$ (the period when the policy of partial liquor ban was in place) and zero otherwise.

The vector $\boldsymbol{Z}_{idst}$ contains a set of woman i's characteristics such as age, education, employment status, marital status, religion, and place of residence. The vector $\boldsymbol{X}_{hdst}$ includes a set of household characteristics associated with individual i, such as household wealth and household size. The vector $\boldsymbol{M}_{dt}$ includes district d's (log) annual real net domestic product per capita. The district, θ_d, and year, μ_t, fixed effects nonparametrically control for all unobserved contributing factors to intimate partner violence varying across districts and time, respectively. The time-varying state fixed effects, π_{st}, control for all changes at the State level over time. The error term, ε_{idst}, captures the remaining unobserved, time-varying, and woman-specific determinants. In this DID setting, standard errors are clustered at the district level where treatment occurs.

We also examine the heterogeneous treatment effects by several individual and household characteristics, including a woman's education level, place of residence, ethnic status such as scheduled caste or tribe, whether a woman has given birth to a male child (as an indicator of status within the household), and age difference between the woman and her partner; and household wealth.

Effect of Policy Removal. To estimate the impact of policy reversal, we utilize the most recent two rounds of India's DHS data — DHS-4 (2015-2016) and DHS-5 (2019-2021), and estimate the following regression:

$$\text{Domestic Violence}_{idst} = \alpha + \beta(\text{Treat}_d \times \mathbb{1}_{\{t=2019\}}) + \boldsymbol{Z}'_{idst}\boldsymbol{\gamma} + \boldsymbol{X}'_{hdst}\boldsymbol{\delta} + \boldsymbol{M}'_{dt}\boldsymbol{\lambda} + \\ + \theta_d + \pi_{st} + \mu_t + \varepsilon_{idst} \tag{6}$$

where $t = \{2015\text{-}2016, 2019\}$. The parameter β captures the impact of policy reversal in 2017.

[33] We do not use DHS-3 (2005-06) because this third round of India's DHS lacks the district identifier and GPS information, although it reports the relevant variables on intimate partner violence.

An indicator variable $\mathbb{1}_{\{t=2019\}}$ equals one if $t = 2019$ (the period when the policy was completely reversed) and zero if $2015 \leq t \leq 2016$ (the period when the policy was effective). For the treatment intensity measure, Treat_d, in the regressions estimating the effect of policy reversal, recall that the control group consists only of Karnataka.[34] Other variables are similar to those in equation (5).

Long-Run Effect of Partial Liquor Ban. We estimate the following equation:

$$\begin{aligned}\text{Domestic Violence}_{idst} = \alpha + \beta(\text{Treat}_d \times \mathbb{1}_{\{t=2019\}}) + \boldsymbol{Z}'_{idst}\boldsymbol{\gamma} + \boldsymbol{X}'_{hdst}\boldsymbol{\delta} + \boldsymbol{M}'_{dt}\boldsymbol{\lambda} + \\ + \theta_d + \pi_{st} + \mu_t + \varepsilon_{idst}\end{aligned} \tag{7}$$

where $t = \{1999, 2019\}$. The term $\mathbb{1}_{\{t\leq 2019\}}$ takes one if $t = 2019$ and zero if $t = 1999$. The DHS-4 survey covering 2015-2016, during which Kerala's partial liquor ban was effective, is excluded from this analysis. Thus, we use DHS-2 and DHS-5 rounds to perform this analysis. The district-level treatment intensity measure, Treat_d, and other variables are similar to those in equation (6).

5.2 Identification and Assumptions

Similar to our consumption regressions, we discuss and test the main identification assumptions for the domestic violence regressions, focusing on the specification estimating the short-run effect of the ban.

Standard Parallel Trend Assumption. Data limitation on physical violence for multiple years precludes a direct test of the parallel pre-trend assumption at the individual level. However, we indirectly show that parallel pre-trend in individual-level physical violence is plausible in our setting based on parallel pre-trends in domestic violence against women at the district level across Kerala and its neighboring states. Since district-level domestic violence against women recorded at police stations should be a significant predictor of individual-level physical violence, parallel pre-trends in district-level records of domestic violence should have meaningful implications of pre-trends in individual-level physical violence.

Using data from the National Crime Record Bureau (NCRB) over the periods around the policy change but before the policy reversal (2001 through 2016) to consider only the event of policy introduction, we test parallel pre-trend assumptions in domestic violence (measured by incidents of cruelty by husband or his relatives) at the district level in the treatment and comparison groups.

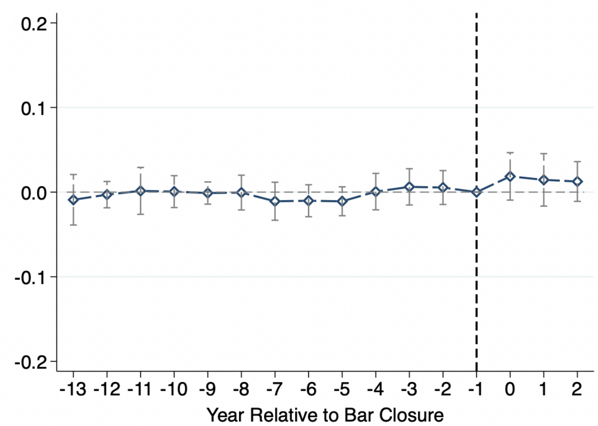
[34] Another reason to exclude Tamil Nadu in the domestic violence regressions relates to the outbreak of COVID-19 in India. The DHS-5 dataset we use to estimate equation (6) was collected for Tamil Nadu over January-March and Nov-Dec in 2020. However, the first confirmed case of COVID-19 in Tamil Nadu was recorded on March 7, 2020, and the nationwide lockdown was imposed on March 25, 2020. Hence, the latter period of data collection for Tamil Nadu overlaps with the pandemic and offers an intriguing study of isolating the effect of "stay-at-home" orders from the alcohol ban on incidences of physical violence – an exercise beyond the scope of this paper. However, the DHS-5 survey for Kerala and Karnataka was collected in 2019, before the first confirmed case of COVID-19 in Kerala on January 30, 2020 and the first confirmed case in Karnataka on March 8, 2020.

Since the parallel trend assumption is conditional, we perform a formal test using event study analysis. Our NCRB dataset spans thirteen pre-policy and three post-ban but pre-reversal years, $\tau = -13, -12, \ldots, -2, -1, 0, 1, 2$, and we run the following regression:

$$\log(\text{Domestic Violence}_{dt}) = \alpha + \sum_{\tau \neq -1; \tau=-13}^{\tau=2} \delta_\tau \times I_\tau \times \text{Treat}_d + \boldsymbol{M}'_{dt}\boldsymbol{\lambda} + \\ + \theta_d + \pi_{st} + \mu_t + \varepsilon_{dt}, \tag{8}$$

where Domestic Violence$_{dt}$ is domestic violence against women per 1000 population[35] in district d at year t, I_τ are lags and leads in event time, with $\tau = -1$ as the reference category. The policy event date (I_0) is 2014. The remaining variables are similar to those in equation (5). Treat$_d$ is treatment intensity at the district-level. Figure 18 shows that parallel pre-trends assumption holds for domestic violence at the district level in all pre-closure years.

Figure 18: Event Study—Test of Parallel Pre-Trend in District-Level Domestic Violence



Notes: The figure shows results from event study analysis testing parallel pre-trends in domestic violence at the district level in Kerala and its neighboring states (Karnataka and Tamil Nadu). The analysis uses district-level data on crime records from the National Crime Record Bureau (NCRB) for 13 years before Kerala's liquor ban (2001 through 2013) and 3 years after the ban (2014 through 2016) but before the policy reversal in 2017, with a base year of 2013. Each observation corresponds to (log) the number of domestic violence incidents (number of cruelty by husband or his relatives) per 1000 population by district and year. The data includes all 14 districts in Kerala and 29 and 32 districts in Karnataka and Tamil Nadu, respectively. All specifications control for district-specific (log) real GDP per capita (net of depreciation), district and year fixed effects, state-by-year FEs, and a constant term. Standard errors are clustered by districts, and 95% confidence intervals are shown.

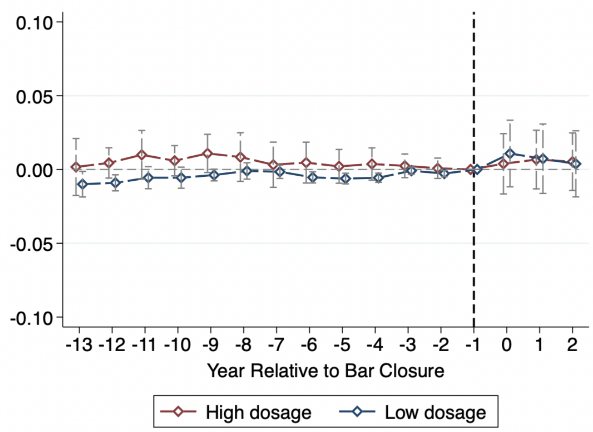
We also fail to find a significant impact of the ban on district-level domestic violence during the policy period (Figure 18), which formally shows that parallel post-trends are likely in our setting. We consider that the common trend assumption is satisfied in the post-ban period in the absence of treatment due to the following facts: First, while the partial alcohol ban in Kerala lasted between April 2014 and July 2017, our individual-level domestic violence data from the DHS during these three years of policy period covers only 22 months between February 2015 and November 2016.

[35] The population data come from the 2001 and 2011 Censuses. The number of crimes recorded between 2001-2010 is normalized by the 2001 population while the number of crimes recorded between 2011-2016 is normalized by the 2011 population at the district level.

No policy changes were affecting the treatment and control groups over these 22 months, which would undermine our identification of the causal impact of the ban on individual-level domestic violence using the DHS data. Starting in May 2016, there was a step-by-step shutdown of liquor shops in Tamil Nadu, one of Kerala's two neighboring states. However, this alcohol control policy in Tamil Nadu does not affect our analysis since the DHS-4 data for Tamil Nadu was collected in 2015. Second, state-by-time fixed effects, π_{st}, capture other state-level policy changes. Third, our results are robust to excluding Tamil Nadu from the control group (see Section 5.5).[36]

Strong Parallel Trend Assumption. We also check the strong parallel pre-trend assumption, suggested by Callaway et al. (2024), using a similar strategy that we employed for household-level alcohol consumption. Although the strong parallel trend is a much stronger assumption than the standard one, our results from event study analysis for district-level domestic violence (Figure 19) show that treated districts with high (above median) and low (below median) doses are generally the same to each other pre-ban and post-ban but pre-reversal period conditional on covariates. It suggests that a strong parallel pre-trends assumption is reasonable in our setting.

Figure 19: Event Study—Test of "Strong" Parallel Pre-Trend in District-Level Domestic Violence



Notes: Each panel shows results from event study analysis testing strong parallel pre-trends in domestic violence at the district level for treated districts with "dosage" of treatment above and below the median treatment intensity. We used district d's per capita consumption of hard liquor in 2012 as a treatment intensity variable in these event study regressions, and the qualitative results generally remain the same when we use the other two measures of treatment intensity. The control group includes districts in neighboring states of Karnataka and Tamil Nadu. The analysis uses district-level data on crime records from the National Crime Record Bureau (NCRB) for 13 years before Kerala's liquor ban (2001 through 2013) and 3 years after the ban (2014 through 2016) but before the policy reversal in 2017, with a base year of 2013. Each observation corresponds to (log) the number of domestic violence incidents (number of cruelty by husband or his relatives) per 1000 population by district and year. The data used in each event study includes 7 districts in Kerala and 29 and 32 districts in Karnataka and Tamil Nadu, respectively. All specifications control for district-specific (log) real GDP per capita (net of depreciation), district and year fixed effects, state-by-year FEs, and a constant term. Standard errors are clustered by districts, and 95% confidence intervals are shown.

[36] In Figure 18, we include 2016 ($\tau = 2$) under the baseline scenario with a control group that contains Tamil Nadu and Karnataka. The step-by-step liquor policy in Tamil Nadu overlaps with the first eight months of 2016. However, we find that dropping 2016 in equation (8) yields qualitatively the same results, although the results are not reported. Additionally, as shown in Figure B.2, excluding Tamil Nadu from the control group provides a remarkably similar pattern as in Figure 18, suggesting that the inclusion of Tamil Nadu and year 2016 do not present significant bias in the event study.

Stable Unit Treatment Value Assumption (SUTVA). Finally, the tests to ensure the validity of the SUTVA outlined in Section 4.2 are also employed for the individual-level domestic violence regressions. Section 5.5 discusses the findings in more detail.[37]

5.3 Baseline Results

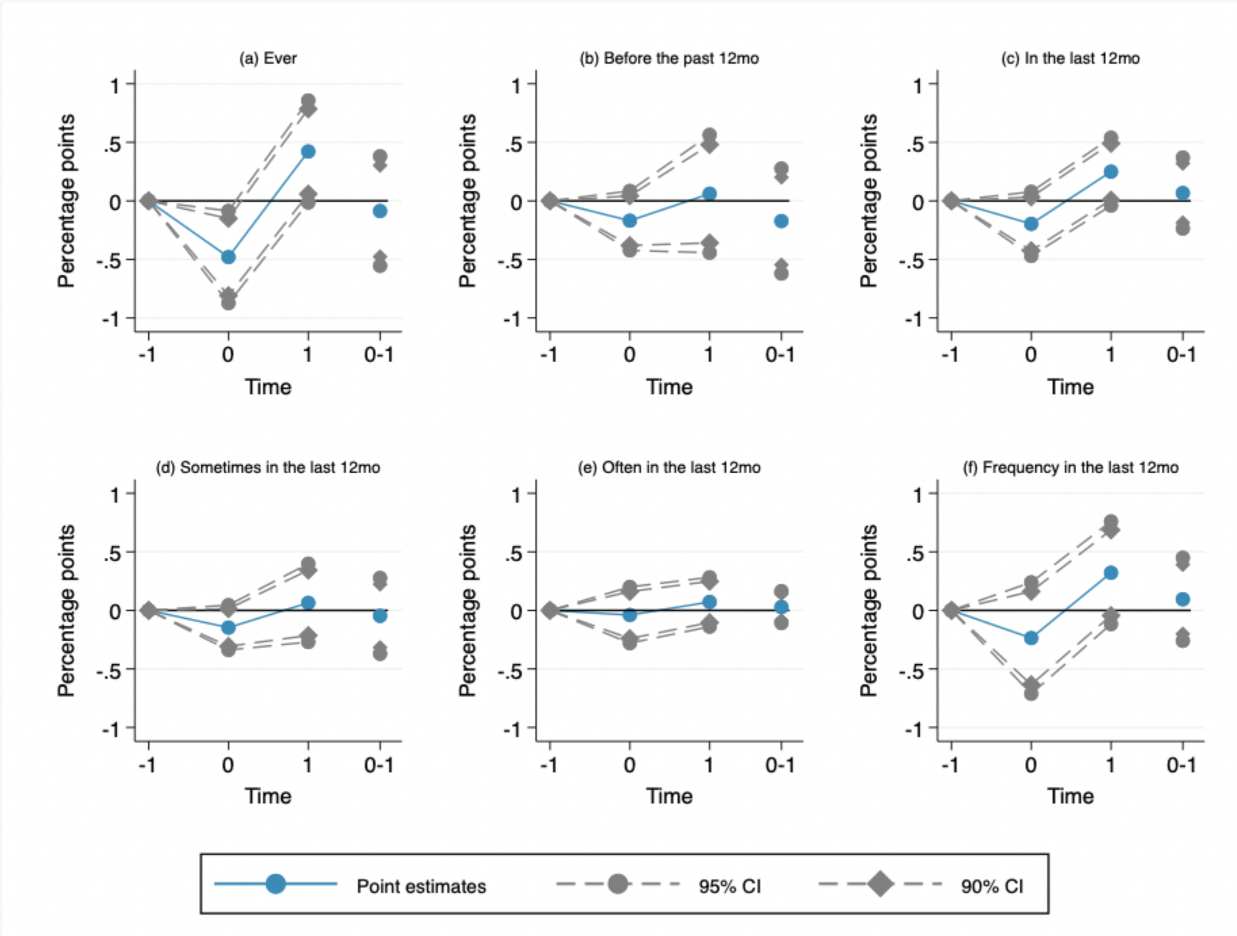
We present our main results on (i) the short-run impact of shutting down hard liquor-selling bars on the incidence of physical violence over the policy period, (ii) the impact of policy removal, and (iii) the net impact of the policy removal and the policy itself in the long run. We discuss the overall and dis-aggregated impacts of the partial liquor ban on physical violence with different timings and frequencies. We also present heterogeneous treatment effects.

The results on the short- and long-run impacts of the closure of hard liquor-selling bars on the incidence and frequency of physical violence using linear probability models (LPMs) are summarized in Figure 20. Each of the panels (a)-(f) estimates equations (5), (6), and (7) for different outcome variables. Panel (a) shows the results when the dependent variable is a dummy equal to one if physical violence was *ever*-experienced by the woman, and zero otherwise. We find a negative impact of the intervention on the incidence of physical violence ever experienced by women at the 5% significance level. Our estimate shows that a one percent reduction in a district's share in the State's total hard liquor consumption leads to 0.48 percentage points reduction in ever-experienced physical violence by women. It means that a 1-liter reduction in monthly per-capita hard liquor consumption leads to 0.06 percentage points reduction in ever-experienced physical violence. However, the incidence of physical violence ever experienced by women rebounded following the policy reversal. The estimate on the impact of policy removal is positive and statistically significant at the 10% level. The net impact on physical violence ever experienced by a woman, in the long run, is essentially zero.

We first disaggregate the impact of the treatment on physical violence with different timings to include (i) ever-experiences of physical violence before the past 12 months (panel (b)) and (ii) ever-experiences of physical violence in the past 12 months (panel (c)). Second, we further disaggregate the impact on domestic violence over the past 12 months to domestic violence with different frequencies, including (i) sometimes-experienced physical violence within the past 12 months (panel (d)) and (ii) often-experienced physical violence within the past 12 months (panel (e)). We finally consider the frequency of physical violence in the last 12 months using a categorical variable as a combination of the ever-experienced physical violence before the past 12 months (=0), sometimes (=1), and often-experienced (=2) physical violence within the past 12 months (panel (f)). The results suggest that the treatment did not have significant short- and long-run impact on any of these measures.

[37] It is also worth noting that we fail to find significant spillover effects of the policy on people who abstain from the treatment within the treatment group, as our finding suggests that low-income families were not affected by the policy that prohibits the product mainly consumed by high-income households.

Figure 20: Short- and Long-Run Effects of the Alcohol Ban on Physical Violence



Notes: The figure presents the OLS estimates on the short- and long-run effects of Kerala's temporary partial liquor ban on physical intimate partner violence. The dependent variables include a dummy indicating whether a woman ever experienced a physical violence (panel (a)), a dummy indicating whether a woman ever experienced a physical violence before the past 12 months (panel (b)), a dummy indicating whether a woman ever experienced a physical violence in the past 12 months (panel (c)), a dummy indicating whether a woman experienced a physical violence sometimes in the past 12 months (panel (d)), a dummy indicating whether a woman experienced a physical violence often in the past 12 months (panel (e)), and a categorical variable for frequencies of physical violence in the past 12 months (panel (f)). The key explanatory variable is our baseline measure of treatment intensity at the district level. First, $\tau = 0$ denotes the periods during which the policy was in place. The estimates at $\tau = 0$ are thus the short-run effects of the partial liquor ban during the policy period relative to the period before the ban was introduced ($\tau = -1$). The sample in the first specification includes a repeated cross-section of women from a balanced district-level panel of 14 treatment districts in Kerala and 61 control districts in Karnataka and Tamil Nadu across two DHS rounds (1999 and 2015-2016). Second, $\tau = 1$ denotes the periods after the policy was lifted. The estimates at $\tau = 1$ are thus the effects of policy removal relative to the policy period ($\tau = 0$). The sample in the second specification includes a repeated cross-section of women from a balanced district-level panel of 14 treatment districts in Kerala and 30 control districts in Karnataka across two DHS rounds (2015-2016 and 2019). Third, $\tau =$ 0-1 covers the periods before the partial liquor ban and after policy removal. The estimates at $\tau =$ 0-1 are the long-term treatment effects or the effects of policy removal relative to the pre-ban period ($\tau = -1$). The sample in the third specification includes a repeated cross-section of women from a balanced district-level panel of 14 treatment districts in Kerala and 30 control districts in Karnataka across two DHS rounds (1999 and 2019). All regressions, equations (5), (6), and (7), include a constant, district, year (round), and state-by-year fixed effects, district-specific (log) annual real GDP per capita (net of depreciation), and demographic characteristics. Demographic controls include individual characteristics: age, age squared, education, working status, a dummy indicating whether the woman has a male child, the age difference between the woman and her partner/husband, and dummies for religion, including Muslim, Christian, and others; and household characteristics: place of residence, household wealth index, household size, and caste status. 95% and 90% confidence intervals are presented, and standard errors are clustered at the district level. Specifications at $\tau = 0$ have 75 clusters. Specifications at $\tau = 1$ and $\tau =$ 0-1 have 44 clusters, and wild cluster bootstrap, following Roodman et al. (2019), with 999 replications, suggests that the effects are not statistically significant on the baseline.

Several potential reasons could lead to the alcohol ban having a statistically insignificant impact on physical violence. For example, the sample unaffected by the intervention could be blurring the policy impact on the sample directly affected by the ban on hard liquor, which is often more expensive than alternative drinks. Although the baseline estimates obtained from the LPMs are not statistically significant, the short-run effects are consistently negative. The impacts of policy reversal tend to be positive for different outcomes. Hence, we consider that the treatment impact of the closure of hard liquor-selling bars on domestic violence against women is strongly heterogeneous.

5.4 Heterogeneity Results

We first conduct heterogeneity analysis by household wealth[38] since the policy targets expensive drinks commonly afforded by high-wealth households. Figure 21 depicts the impact of the policy on physical violence heterogeneous by household wealth. Notably, the policy reduced the incidences of physical violence, except for incidents that occurred often in the past 12 months, but only within high-wealth households. The coefficient estimates on physical violence ever experienced by the women and physical violence ever experienced before the past 12 months are slightly larger in magnitude and more significant than the baseline estimates (panels (a) and (b)). The estimate shows that for each percent reduction in a district's share in the state's total hard liquor consumption, the likelihood of a woman ever experiencing physical violence overall and before the past 12 months decreased by 0.53 (panel (a)) and 0.29 (panel (b)) percentage points, respectively, in high-wealth households during the short run. The implication of these results is similar to what we find for the baseline above. Each liter reduction in monthly per capita hard liquor consumption leads to 0.03 and 0.05 percentage points reduction in the likelihood that a woman ever experiences physical violence overall and before the last 12 months, respectively. These ever experiences of physical violence, however, increased in high-wealth households following the reversal of the partial alcohol ban, indicating that although physical violence declined for women in high-wealth households over the policy period, this change was only transitory.[39] In the long run, impacts on overall domestic violence ever experienced by women and those before the last 12 months are both positive and statistically significant at the 10% level.[40]

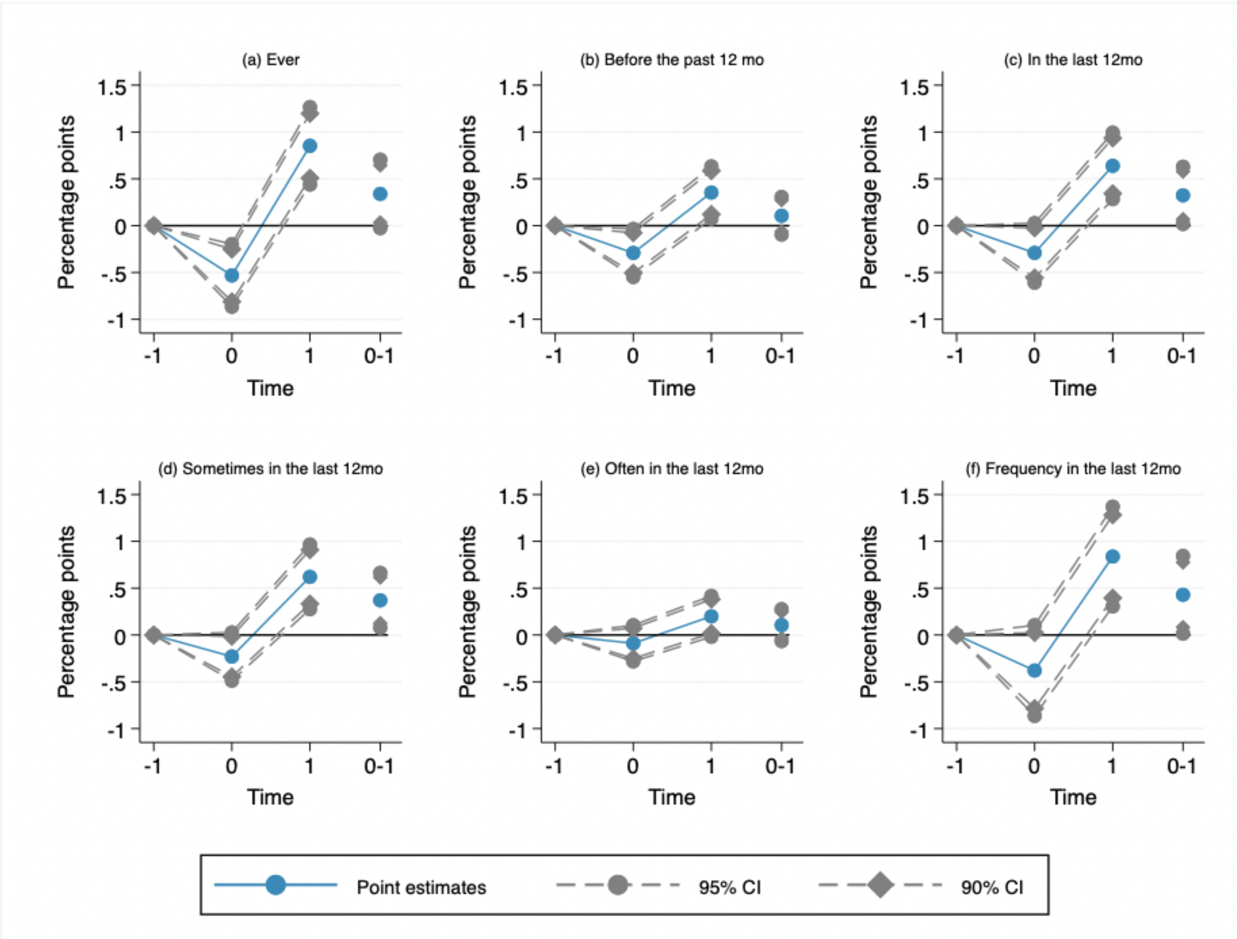
We focus on physical violence experienced relatively close to the policy periods, and our results show the impact of the policy is negative and statistically significant at the 10% level (panel (c)). But further decomposing this into violence with different frequencies indicates that the treatment more significantly reduced the likelihood of less frequent physical violence in the last 12 months

[38]We leverage household wealth because the DHS data we use to examine the domestic violence effects of the program reports household wealth index rather than household income.

[39]Noteworthy here is that the DHS-5 survey was conducted in July-November 2019 in Kerala and from July-December 2019 in Karnataka. Hence, the timing of women's experiences of physical violence in the last 12 months that we captured in our DHS sample overlaps well with changes in alcohol consumption in bars during the post-removal period.

[40]These impacts of the alcohol ban on intimate partner violence verify that the identified relationships are causal, not spurious, because if the relationship was spurious—people who are more likely to act violently towards their partners are, due to other unobservables, more likely to be drinkers—the ban should not necessarily affect intimate partner violence.

Figure 21: Heterogeneous Effects of the Temporary Liquor Ban on Physical Violence by Household Wealth



Notes: The figure presents the OLS estimates on the short- and long-run effects of Kerala's temporary partial liquor ban on physical intimate partner violence heterogeneous by household wealth. The key explanatory variable is our baseline measure of district-level treatment intensity interacted with household wealth index.

within high-wealth households. Particularly, physical violence experienced *sometimes* in the past 12 months dropped by 0.23 (panel (d)) percentage points in response to a 1 percent decrease in a district's share in the State's total hard liquor consumption. It translates into each liter reduction in monthly per capita hard liquor consumption, generating a 0.02 percentage point reduction in physical violence sometimes experienced by women within the past 12 months. However, there is no significant reduction in the likelihood of *often* experiencing physical violence within the past 12 months (panel (e)), which is consistent with the notion that alcohol controls are less effective in reducing extreme drinking (Carpenter et al. (2016)). It suggests that changes in the less frequent physical violence within the last 12 months drive our result. The frequency of physical violence in the past year (panel (f)) within high-wealth households was not affected by the ban in the short run. Similar to our baseline impact of the policy reversal, we find a positive effect on physical violence; however, the difference is that the positive impact is much stronger in magnitude and statistically significant in high-wealth households. For example, a woman's likelihood of experiencing *any* physical violence over the past 12 months increased by 0.07 percentage points in high-wealth households for each liter increase in monthly consumption of hard liquor, overshooting the 0.03 percentage point decrease during the policy period.

In the long run, we find a positive, although somewhat weakly significant, impact of the policy on physical violence within high-wealth households. In particular, our heterogeneity result shows increases in (i) physical violence ever experienced in the last 12 months (panel (c)) and (ii) sometimes-experienced physical violence in the past 12 months (panel (d)) within high-wealth households after the partial ban has been retracted. It provides evidence that the rebound in physical violence exceeded the original decline due to the treatment. Put differently, the temporary ban worsened physical violence that occurs within high-wealth households in the long run. However, this result is only statistically significant at the 5% level. Nevertheless, the evidence from this analysis shows that the temporary ban in Kerala, i.e., the closure of hard-alcohol-selling bars between 2014 and 2017, did not have a permanent or persistent impact on reducing physical violence.

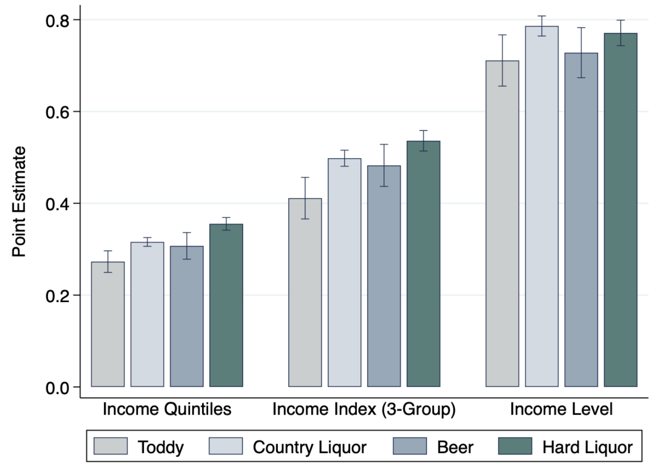
To further explore the impact of the policy on other potential groups, we conduct additional heterogeneity analyses by woman's educational attainment, place of residence, ethnic status (scheduled caste or tribe), whether the woman has given birth to a male child, and the woman's age difference with her partner. Results from heterogeneity by education, shown in Figure D.1, suggest that the ban reduced the prevalence of physical violence ever experienced (panel (a)) and physical violence experienced before the past 12 months (panel (b)) for more educated women, and the policy removal had a significant positive impact on these two outcomes. However, the policy did not have any short- or long-term heterogeneous impact by education on physical violence experienced within the past 12 months (panels (c)-(f)). We do not find any short- or long-run treatment impacts of the partial liquor ban and policy reversal on physical violence for any of the other characteristics (Figures D.2–D.5). It indicates that the impact of the partial alcohol ban was mainly confined within high-wealth households - which is intuitive since Figure 22 shows that hard liquor consumption is strongly associated with higher household income.

5.5 Robustness

For the domestic violence effects, we perform robustness checks similar to those in Section 4.4 for the impacts on consumption. Additionally, we check the robustness of our results on the domestic violence effects by (i) employing nonlinear—logit and probit—regressions, (ii) using an alternative method of sample splitting to estimate heterogeneous impacts by household wealth, and (iii) using district-level crime data on domestic violence obtained from the National Crime Record Bureau (NCRB).

Alternative Treatment Intensity. We first check the robustness of our domestic violence results by using two alternative treatment intensity measures.

Figure 22: Relationship between Income and Consumption of Different Alcoholic Drinks



Notes: Based on India's NSS datasets from 2001-2002 through 2011-2012. The figure plots the coefficient estimates and 95% confidence intervals from the OLS regressions where the dependent variable is (log) consumption of alcoholic drinks, and the key explanatory variable is different measures of household income stated on the horizontal axis. Alcohol consumption is measured by the average monthly amount (liters) of alcohol consumption per household member. The household income is measured by monthly per capita expenditure (Rs.0.00). The 3-group household income index takes a value of 1 if the household is in the bottom 40% of the household income distribution, 2 if the household is in the middle 40% of the household income distribution, 3 if the household is in the top 20% of the household income distribution. The sample contains individuals in all states and union territories (UTs). Each specification includes unreported district fixed effects (FEs), year of survey FEs, district-by-year FEs, household covariates, and a constant term. Household covariates include age, gender, education, and marital status of household head, scheduled caste or tribe, urban/rural dummy, and household's religion. The unit of observation is the household. The mean monthly consumption is 2.28 for toddy, 0.88 for country liquor, 0.92 for beer, and 0.35 for hard liquor. Confidence intervals are heteroskedasticity robust.

Per-capita Consumption. Figure E.1 presents the results on the short-run effects, which shows that our qualitative findings on the short-run impact of the partial liquor ban on physical violence remain robust. Specifically, the domestic violence impacts of the policy are not statistically significant on the full sample (i.e., ignore heterogeneity). However, when we undertake heterogeneity analysis, there is a significantly negative impact of the ban on physical violence in higher-wealth households in the short run.

Number of Bars Closed Down. Physical violence is more responsive to the imposition and the removal of the policy when using the number of bars shut down as a treatment intensity variable for the full sample of households (Figure E.2). Moreover, heterogeneity analysis by household wealth under this alternative measure (Figure E.3) indicates a stronger result than the one with the full sample households. Our baseline results thus remain robust to this alternative measure.

Alternative Control Group. Although the time frame of the step-by-step shutdown of liquor shops in Tamil Nadu does not overlap with our sample during the policy period, we check the robustness of our results on the short-run impacts by excluding Tamil Nadu and keeping only the state of Karnataka in the comparison group. Figure E.4 shows that our results on the short-run effects are robust to excluding Tamil Nadu from the control group.

Cross Border Spillovers. Our results remain unchanged, independent of whether we use border or interior districts of the neighboring States in the control group. Figure E.5a shows that the treatment impacts on the incidence and frequency of physical violence estimated on the full sample are not statistically significant. Figure E.5b presents the heterogeneous impacts by household wealth, which are also robust to these different choices of the control group.

Effects in Border and Interior Treated Districts. When we restrict the treatment group to border districts of Kerala and compare them with all districts (panel (1)), interior districts (panel (2)), and border districts (panel (3)) in neighboring states, we find that the policy impact on physical violence is statistically insignificant if estimated on the full sample (Figure E.6a). Figure E.6b presents the heterogeneous impact under the same specifications and underlines the robustness of our heterogeneity analysis by household wealth.

Then, we use the interior districts of Kerala as the treatment group and compare them with the three different constructs of the control group. Results estimated on the full sample are different from our baseline and other robustness check findings. Figure E.7a reports the estimates, suggesting that the incidence and frequency of physical violence in these interior districts increased due to the hard liquor ban in bars during the policy period. Although the coefficient estimates are positive and strongly significant, the positive effects become essentially zero when the standard errors are not adjusted using wild cluster bootstrap. Then, when we introduce household wealth heterogeneity, coefficient estimates become negative (Figure E.7b). Although coefficients on the incidence of physical violence ever experienced by the woman are not statistically significant (first point estimate in panels (1)-(3)), some of the negative coefficients on physical violence with different timing and frequencies are statistically significant (second and fifth point estimates in panels (1)-(3)). But we also find that most of these negative heterogeneous treatment effects become essentially zero when we adjust the standard errors using wild cluster bootstrap. It is possible that Roodman et al.'s (2019) approach of wild cluster bootstrap over-rejecting in these specifications with fewer interior treated districts. Taken together, we show that our findings on short-run treatment impact, especially heterogeneous treatment impact by household wealth, are generally robust to these different constructs of the treatment group.

Nonlinear Regressions. Given the dichotomous nature of our outcome variables, we provide estimates from binary response models, including logit and probit as robustness tests. Figure E.8a shows the short-run effects estimated using logit (panel (a)) and probit (panel (b)) regressions on the full sample. The logit and probit models provide negative and statistically significant average marginal effects (AMEs) of the partial liquor ban on incidences of physical violence against women, except for only those that often occurred in the past 12 months. These negative impacts are slightly different from results from LPM in our baseline analysis, in which the negative impacts were statistically insignificant for less frequent intimate partner violence. However, results from logit and probit regressions estimating the AMEs of the treatment on physical violence heterogeneous by household wealth, shown in Figure E.8b, suggest that our wealth heterogeneity results are robust to alternative estimation approaches.

Heterogeneity by Sample Splitting. In our baseline heterogeneity analysis, we estimate the heterogeneous impacts by interacting with the terms, while we can also evaluate the baseline specification on sub-samples generated by splitting the sample. We thus perform the robustness check of our results on the heterogeneous short-run impact of the ban using this alternative method and focus on heterogeneity by household wealth. Figure E.9 shows that our results on heterogeneous impacts of the partial ban on physical violence are substantially robust to a sub-sampling method. We found no significant effect in low- and middle-wealth households, but the ban had a negative and statistically significant impact among high-wealth households.

Using District-Level Crime Data on Domestic Violence. We conduct the robustness check of our baseline results or the non-heterogeneous impact of the treatment over the short and long run by using district-level crime data recorded at police stations between 2001-2019.

To quantify the short-run effect, we estimate

$$\begin{aligned} \log(\text{Domestic Violence}_{dst}) = \alpha + \beta(\text{Treat}_d \times \mathbb{1}_{\{2014 \leq t \leq 2016\}}) + \boldsymbol{M}'_{dt}\boldsymbol{\lambda} + \\ + \theta_d + \mu_t + \theta_d \times t + \pi_{st} + \varepsilon_{dst} \end{aligned} \tag{9}$$

where an indicator variable $\mathbb{1}_{\{2014 \leq t \leq 2016\}}$ takes a value of one if $2014 \leq t \leq 2016$ and zero if $t < 2014$, $\theta_d \times t$ is district-specific time trends, and other terms are similar to those in equation (5). Note that state-by-time fixed effects are captured by π_{st}, and in this specification, we use both Karnataka and Tamil Nadu as a control group. Using only Karnataka does not change the qualitative results.

The long-term consequences of the policy reversal and the impact of the policy itself on district-level physical violence are undertaken as follows. First, to analyze the effect of policy reversal, we replace the indicator variable in equation (9) with an indicator variable which equals one if $t > 2016$ and zero if $2014 \leq t \leq 2016$. Second, to estimate the long-term impact of the partial alcohol ban, we replace the indicator variable in equation (9) with an indicator variable that takes a value of one if $t > 2016$ and zero if $t < 2014$. In both these specifications, we include only Karnataka in the control group.

Table E.1 presents the results. Consistent with Khurana and Mahajan (2022)'s findings on short-run domestic violence using crime data, we find no short-term impact of the intervention on district-level physical violence (Column (1)). Our novel findings of the effect of the policy reversal (Column (2)) and long-run consequences of the policy (Column (3)) on district-level physical violence are also statistically insignificant. All these results are consistent with our event-study results in Figure 18 and our baseline DID findings that indicate a null impact on physical violence in full-sample households over the evolution of this policy.

5.6 Mechanisms

We explore the potential mechanisms through which the different episodes of Kerala's partial liquor ban could have affected physical violence in high-wealth households. In doing so, we evaluate the channels primarily based on our results in Section 4 on the short- and long-term consumption effects of the partial liquor ban.

Substitution between Bars and Home. In Section 4, we find no evidence of a strong transition of alcohol consumption from bars to home, which confirms that changes in physical violence are not explained by changes in drinking establishments.

Neuro-Behavioral Response. The changes in alcohol consumption in bars are, however, consistent with physical violence changes over the policy episodes. Alcohol consumption affects neuro processes and leads to mood changes and the inability to moderate behavior. Thus, a reduction in alcohol intake, which alter the potency of the alcohol consumption, leads to a reduction in intimate partner violence and vice versa. For example, Markowitz and Grossman (1998) consider violence as a by-product of alcohol consumption, and Markowitz (2001) shows that drinking is positively associated with the probability of physical fighting and likelihood of weapon carrying by teenagers, i.e., drinking encourages violent behaviors. Card and Dahl (2011) also find that alcohol drinking due to the loss of the home team in football increases domestic violence in the U.S., and Ivandić et al. (2024) find that drinking on a football game day increases domestic abuse in the context of Greater Manchester, UK.

Changes in Alcohol Tolerance. The alcohol consumption in bars after the policy reversal is higher than the pre-ban level, which could be the reason for the overshooting impact of the policy removal on physical violence. Prolonged consumption of alcohol leads to neuro adaptation – the same amount of alcohol will have a weakened effect on brain functions and behavioral impacts. A period of abstinence during the ban, on the other hand, makes individuals less tolerant of alcohol. Thus, an alcohol ban reversal after a period of abstinence can give rise to an exaggerated behavioral response compared to the pre-ban level, even with the same amount of alcohol. Thus, changes in alcohol tolerance could have also contributed to overshooting the negative behavioral impact of the policy reversal.

Thus, we consider that the latter two channels explain our findings on the short- and long-term consequences of the temporary policy. For example, for the overshooting impact of policy reversal, alcohol tolerance weakens during the ban, and consumption of hard liquor with a strong intoxication effect at this low tolerance after the policy reversal results in a stronger neuro-behavioral response, which leads to more violence.

6 Conclusion

This paper studies the short- and long-run impact of a temporary alcohol control policy on alcohol consumption and physical intimate partner violence in the State of Kerala in India. Given that the literature investigating the consequences of temporary alcohol-related policies on drinking and its externalities is sparse, the policy reversal allows for a unique opportunity to analyze not just consumption and intimate partner violence when the policy was in place but whether such policies can be effective in permanently addressing serious problems. Leveraging multiple individual and household survey data and employing a difference-in-differences (DID) event-study design with a highly comparable and sufficiently isolated group as a control group for identification, we provide evidence of significant "first stage" effects—changes in alcohol consumption in bars during the different episodes of Kerala's temporary partial alcohol policy. We also find no substantial changes in alcohol consumption at home. At a granular level, we unravel a largely heterogeneous impact of the partial liquor ban between 2014 and 2017 on less frequently experienced physical violence for high-wealth households, who are the primary consumers of the prohibited type of alcohol. Unfortunately, not only did incidences of physical violence rebound in high-wealth households following the policy removal, but the rebound effect outweighed the physical violence-decreasing impact throughout the policy, leading physical violence to a level higher than the original pre-ban level. These results thus indicate that individuals' neuro-behavioral response to alcohol consumption and changes in alcohol tolerance over the policy episodes serve as the dominant channels explaining the short- and long-term effects of the temporary policy and the overshooting impact of the policy removal on intimate partner violence within high-wealth households.

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