

# **Alcohol Consumption and Intimate Partner Violence: Long-Term Effects of a Temporary Alcohol Ban<sup>\*</sup>**

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## **Abstract**

We trace the impact of a partial liquor ban – from launch to reversal – on alcohol consumption and women’s experience with intimate partner violence (IPV) in Kerala, India. Decomposing the policy-induced and reversal effects by employing difference-in-differences and event-study approaches, we identify a significant reduction in alcohol consumption (but only in liquor-serving bars) with an accompanying reduction in IPV during the policy period. However, both alcohol consumption and IPV rebounded to pre-ban levels after the policy removal. Heterogeneity analysis further reveals these effects to be confined only amongst high-wealth households. A battery of robustness tests confirms our findings.

**Keywords:** Alcohol drinking, Domestic violence, Prohibition, 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.<sup>1</sup> The channels triggering changes in alcohol consumption, in turn, are attributed to income effects caused by policies like taxes (Markowitz and Ding, 2020) and bans (Mukherji and Chakraborty, 2024) on alcohol consumption, and enforcement of minimum legal drinking age (MLDA) legislations (Coate and Grossman, 1988; Kaestner and Yarnoff, 2011; Yörük and Yörük, 2013). However, remarkably little is known about the persistence of *temporary* alcohol control policies on alcohol consumption and alcohol-related externalities, particularly domestic violence within the household,<sup>2</sup> 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<sup>3</sup> - 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 intervention affects 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

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<sup>1</sup>Alcohol consumption is associated with a variety of adverse outcomes. 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, 2019; Debnath et al., 2023; Ivandić et al., 2024; Dixit and Mukherjee, 2025), mortality (Carpenter and Dobkin, 2009; Carpenter et al., 2016; Yakovlev, 2018; Kueng and Yakovlev, 2021; Barron et al., 2024), traffic accidents (Jackson and Owens, 2011; Lindo et al., 2016; Sloan, 2020), women's agency (Mookerjee et al., 2025), and domestic violence (Angelucci, 2008; Angelucci and Heath, 2020).

<sup>2</sup>Intimate partner violence is a global public health, social, and human rights issue that has permanent adverse consequences (WHO, 2021). A growing literature on domestic violence studies various determinants of domestic violence, such as male and female employment (e.g., Erten and Keskin, 2024; Bhalotra et al., 2025), unearned income (Hidrobo et al., 2016; Haushofer et al., 2019; Calvi and Keskar, 2023), earned income (Sanin, 2025), work incentives (Byambasuren and Nguyen, 2025), and alcohol policy (Luca et al., 2015, 2019; Chakrabarti et al., 2024; Dar et al., 2024; Dixit and Mukherjee, 2025; Frezza, 2025). However, no study has yet examined whether a temporary alcohol ban can have long-term changes in behavior toward intimate partners.

<sup>3</sup>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.

alcohol-related policies for developed countries,<sup>4</sup> 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 guided by either revenue motives, health concerns, or cultural norms.<sup>5</sup> 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 a novel household-level monthly panel data from the Consumer Pyramids Household Survey (CPHS), which enables us to estimate the impacts of the policy on alcohol consumption in bars as well as at home. Results from the consumption effects also inform on the potential mechanisms through which the partial liquor ban impacts intimate partner violence in the short and the 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). The DHS data allows us to construct the incidence of physical violence with different frequencies and conduct heterogeneity analysis at the granular level (individual and household) before, during and after the ban - such granular analysis is not commonly found in the economics of crime literature.<sup>6</sup> 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 allows us to define a treatment variable for the lard liquor ban policy.

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 consumption increased back to the pre-ban level in the 10th month and remained stable for the rest of the policy

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<sup>4</sup>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.

<sup>5</sup>Papers analyzing alcohol policies in India can be grouped into two areas: estimating the price elasticity of demand 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, [Debnath et al. \(2023\)](#) and [Dixit and Mukherjee \(2025\)](#) document a decline in the likelihood of a woman experiencing domestic violence. Analyzing the same alcohol policy in Bihar, [Dar and Sahay \(2018\)](#) find an increase in crime, while [Chaudhuri et al. \(2024\)](#) find 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 \(2022\)](#) find no robust decline in the officially recorded complaints of violence against women.

<sup>6</sup>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).

period.<sup>7</sup> After the policy reversal, we identify a moderate hike immediately afterward and a sharper and persistent increase after about 20 months post-reversal, which was caused by a separate alcohol policy in Kerala's capital, Thiruvananthapuram, that extends the bar operating hours. Ignoring the policy period and comparing post-reversal and pre-ban alcohol consumption in bars yields similar, but relatively more pronounced consumption-increasing effects in the periods following the policy reversal. However, accounting for other alcohol policies in Kerala, we find that alcohol consumption generally reverse back to the pre-ban level in the long term.

Our analysis highlights a statistically significant drop in physical violence within high-wealth households 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.<sup>8</sup> Our analysis also suggests intimate partner violence rebounded in high-wealth households after the policy reversal. 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. 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. This overshooting of physical violence in the post-reversal period was caused by an increase in alcohol drinking due to a policy change in a particular district that allowed bars to remain open for an extra hour until midnight.

Our results from the consumption regressions suggest that there is no transition of alcohol consumption from bars to home in response to the closure and reopening of hard liquor-selling bars, which precludes the possibility of changes in home drinking via alcohol purchases from liquor shops over the policy episodes. Thus, the changes in intimate partner violence over the policy episodes are primarily explained by the changes in alcohol consumption in bars. 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.

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<sup>7</sup>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. In the context of a developing country, Mukherji and Chakraborty (2024) show that Bihar's complete alcohol ban persistently reduced alcohol and tobacco consumption, with a gradual rebound of smoking in about 18 months after the ban.

<sup>8</sup>Our results on the domestic violence-reducing effect of the policy in the short run are consistent with other studies. For example, Luca et al. (2015, 2019) 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. Dixit and Mukherjee (2025) also suggest that alcohol prohibition policy in the Indian state of Bihar leads to a reduction in domestic violence.

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

The State government imposed a partial liquor ban in Kerala to limit alcohol consumption and its related externalities, as the first step of a 10-year plan to make Kerala a dry state; however, it was only effective from April 2014 to July 2017. This policy 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.<sup>9</sup>

### 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<sup>10</sup> 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.<sup>11</sup> 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 closed on March 31, 2015.<sup>12</sup>

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.<sup>13</sup> 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.

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<sup>9</sup><https://www.bbc.com/news/world-asia-india-40213562>

<sup>10</sup>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.

<sup>11</sup>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.

<sup>12</sup><https://www.theguardian.com/world/2014/aug/29/kerala-alcohol-ban-hits-holidaymakers>

<sup>13</sup>With only 14 five-star hotels and 338 liquor shops owned by a state-run organization, Kerala State Beverages Corporation (KSBC) or BEVCO, from which people can freely purchase hard liquors (<https://www.bbc.com/news/world-asia-india-28892073>), 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.

Before the ban, bars in Kerala operated from 9 a.m. to 11 p.m. ([Khurana and Mahajan, 2022](#)). The bars were allowed to remain open until 10 p.m. during the ban. The removal of the ban also eased the bars' operating hours, allowing them to stay open until 11 p.m. (<https://www.bbc.com/news/world-asia-india-40213562>).

## 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 consumption in the neighboring States was 2.8 liters in Karnataka and 2.2. liters in Tamil Nadu,

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

Top 10 states	Alcohol		Toddy		Country Liq.	
	Mean (1)	Top 10 states	Mean (2)	Top 10 states	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	

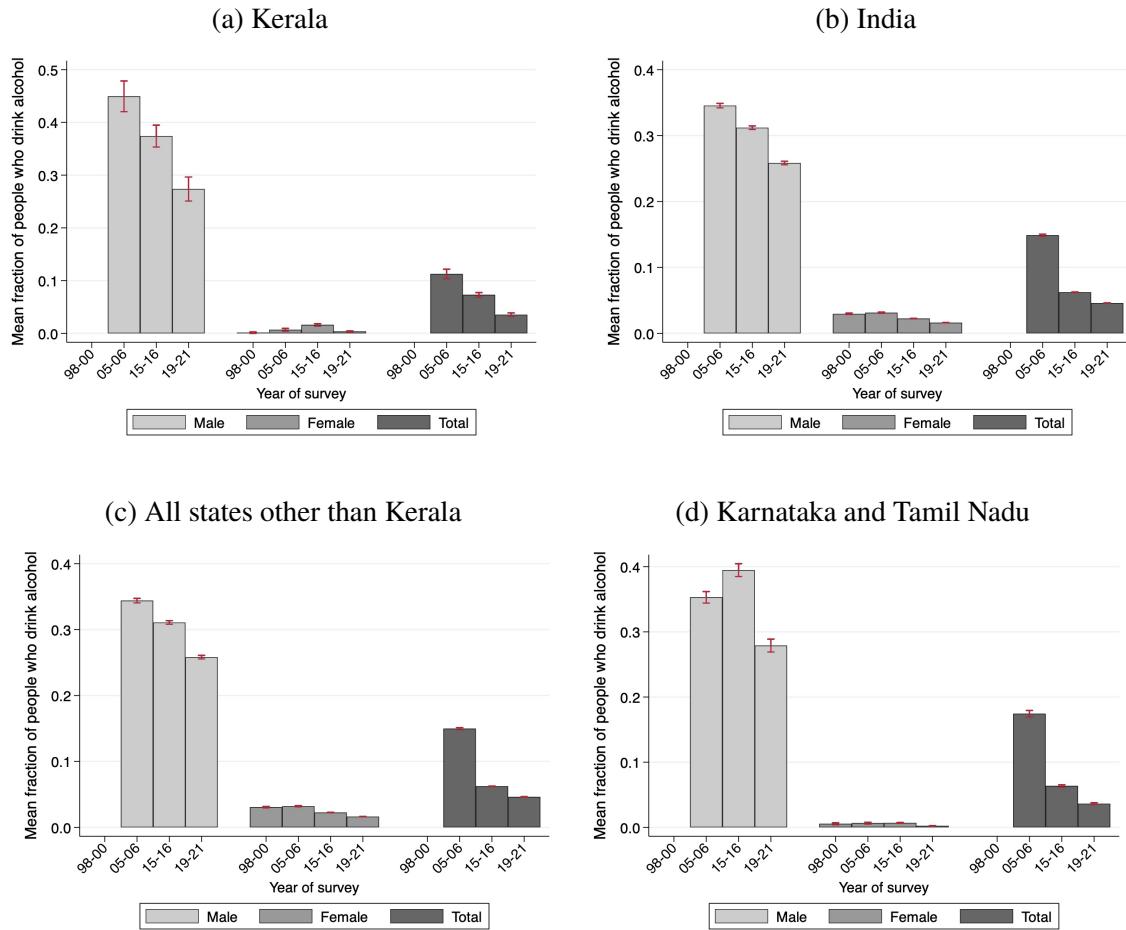
Top 10 states	Beer		Foreign Liq.	
	Mean (4)	Top 10 states	Mean (5)	
1 Chandigarh	2.994	1 Dadra & Nagar Haveli	0.633	
2 Daman & Diu	1.634	2 Gujarat	0.434	
3 Tripura	1.133	3 Jammu & Kashmir	0.405	
4 Karnataka	1.106	4 Kerala	0.404	
5 Arunachal Pradesh	1.004	5 Himachal Pradesh	0.395	
6 Sikkim	0.996	6 Chandigarh	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<sup>th</sup> round) through 2011-2012 (68<sup>th</sup> 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.

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 1a). This decline in the share of Kerala's alcohol consumers during the policy period was due to a decrease in the share of male consumers from 44.9 to 37.4 percent because the fraction of female consumers has been constantly increasing from 0.1% in 1998-2000 to 0.7% as 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.

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, showing a very similar pattern compared to that of the entire country, except Kerala. 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. 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

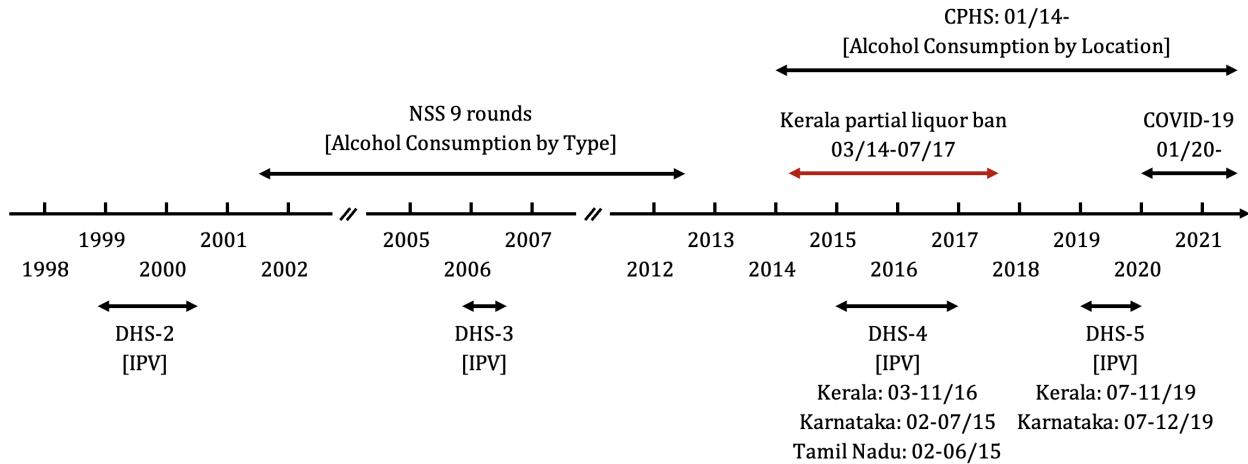
	State	1998-2000	State	2005-06	State	2015-16	State	2019-21
1	Tamil Nadu	0.360	1	Bihar	0.574	1	Manipur	0.511
2	Telangana	0.277	2	Madhya P.	0.454	2	Andhra P.	0.438
3	Bihar	0.264	3	Uttar P.	0.436	3	Bihar	0.430
4	Odisha	0.229	4	Tamil Nadu	0.421	4	Telangana	0.421
5	Uttar P.	0.218	5	Manipur	0.418	5	Tamil Nadu	0.393
6	Madhya P.	0.209	6	Rajasthan	0.416	6	Uttar P.	0.360
7	Jharkhand	0.200	7	Tripura	0.412	7	Chhattisgarh	0.360
8	Karnataka	0.196	8	Assam	0.385	8	Odisha	0.333
9	Arunachal P.	0.188	9	Arunachal P.	0.384	9	Jharkhand	0.325
10	Andhra P.	0.173	10	Jharkhand	0.368	10	Madhya P.	0.319
27	Kerala	0.075	24	Kerala	0.161	30	Kerala	0.133
	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) runs and evaluate potential mechanisms through which the policy could have affected intimate partner violence. Concurrently, we analyze alcohol consumption at home over the 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.<sup>14</sup> 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 alcohol 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.

<sup>14</sup>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).

### 3.1 Alcohol Consumption

Alcohol consumption data for pre- and post-ban periods come from two different sources. For the pre-policy period, alcohol consumption data is 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 January 2014, which provides only three months of pre-ban data. We use the CPHS dataset for our event-study analysis, estimating the short- and long-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

Existing literature suggests that alcohol consumption increases the likelihood of domestic violence.<sup>15</sup> 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

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<sup>15</sup>For example, [Luca et al. \(2015, 2019\)](#) 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.

types of physical violence.<sup>16</sup>

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 1, 2014. The DHS-4 wave was conducted in Kerala between March and November 2016, Karnataka between February and July 2015, and Tamil Nadu between February and June 2015. 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.<sup>17</sup> 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, women's self-drinking status is positively associated with the risk of experiencing intimate partner violence at the extensive margin. However, this latter magnitude is smaller when compared to the male partner's alcohol consumption 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.<sup>18</sup>

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<sup>16</sup>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.

<sup>17</sup>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.

<sup>18</sup>Studies in the recent literature documented a similar pattern of the link between alcohol consumption and violent behavior in other Indian states and in other countries. For example, [Barron et al. \(2024\)](#) show that an alcohol sales ban reduced violent crimes and fatal injuries in South Africa. For the relationship between alcohol drinking and violence within the household, [Dixit and Mukherjee \(2025\)](#) find that alcohol prohibition reduced domestic violence in Bihar, India.

Table 3: Correlation between Alcohol Use and Physical Violence

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

*Notes:* Based on India's pooled 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 \*\*\* $p < 0.01$ .

### 3.3 Treatment Intensity

The key regressor of interest 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.<sup>19</sup> We use 2012 as a pre-treatment period for the construction of the treatment intensity measure since the 68<sup>th</sup> 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. Previous studies, such as [Khurana and Mahajan \(2022\)](#), used the number of bars closed down in the treated district per million population as a proxy for treatment intensity. We supplement this by using the quantity of alcohol consumed, which is novel, as the sales per bar can differ across bars and change over time. Our baseline treatment intensity measure, pre-treatment per capita consumption of hard liquor, captures the liquor consumption in the intensive margin, which is not necessarily captured by the number of bars.

<sup>19</sup>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.

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 in the treated district per million population. We use the district-level 2011 Census population for the normalization.<sup>20</sup>

### 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 characteristics (age, education level, employment status, marital status, religion, and place of residence (urban or rural)) and household characteristics (wealth and size) - all of which are gleaned from the DHS survey.<sup>21</sup>

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 in addition to 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 effectively partials out the impact of any such changes at the district level. The

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<sup>20</sup>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, which was 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, or Kerala's capital city, 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. Most hard liquor-serving bars were closed down in the city of Thiruvananthapuram, introducing the most significant variation in treatment intensity even after normalization.

<sup>21</sup>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.

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 consumption of alcohol in bars and at home. Household consumption of alcohol in these two venues is evaluated across three distinct time windows: the short-term when the policy was in effect, the period immediately after the policy reversal and any long-term persistence of the policy.

### 4.1 Empirical Strategy

We employ difference-in-differences event-study specifications to quantify the effects of the policy on monthly alcohol consumption at the household level. The estimating equations essentially compares households 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:

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

where  $\text{Consumption}_{ht}$  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 and alcohol consumption at home. The parameter of interest is  $\beta_\tau$ , which captures the impact of the liquor ban in Kerala that was effective from April 2014 to June 2017. The terms,  $I_\tau$ , 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,  $\text{Treat}_d$ , which is the pre-ban per capita consumption of hard liquor that 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, 2025), or there is one treated group and one control group but multiple pre-treatment periods (Ferman and Pinto, 2019). Given that we have only one data point for the pre-treatment period and two control states, 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  $X_{ht}$  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.<sup>22</sup> The vector  $M_{d \times \text{Year}}$  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,  $\mu_h$ . The year and month fixed effects, Year <sub>$t$</sub>  and Month <sub>$t$</sub> , nonparametrically capture all unobserved contributing factors of consumption varying across years and months, respectively. The interaction between the State dummy ( $\pi_s$ ) with yearly (Year <sub>$t$</sub> ) and monthly (Month <sub>$t$</sub> ) dummies nonparametrically capture all changes at the State level over time, even within a year, such as liquor tax policies.<sup>23</sup> The error term,  $\xi_{ht}$ , captures the remaining unobserved, time-varying, and household-specific determinants of alcohol consumption. The standard errors are clustered by districts to

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<sup>22</sup>Household income was obtained from the Income Pyramids and merged with Consumer Pyramids by household and year-month.

<sup>23</sup>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.

allow for heteroskedasticity and serial correlation within clusters, assuming that treatment is varied by districts (Bertrand et al., 2004; Angrist and Pischke, 2009).<sup>24</sup>

**Effect of Policy Removal.** The key contribution of this paper is to understand the short- and long-term consequences (if any) of a policy affecting alcohol purchases temporarily. 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 alcohol consumption.

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

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

where  $t \in [\text{April 2014, December 2019}]$ , and the event date of policy removal ( $I_0$ ) is July 2017. 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.<sup>25,26</sup>

**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:

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

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<sup>24</sup>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).

<sup>25</sup>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>.

<sup>26</sup>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.

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. 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, prior to 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.

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.

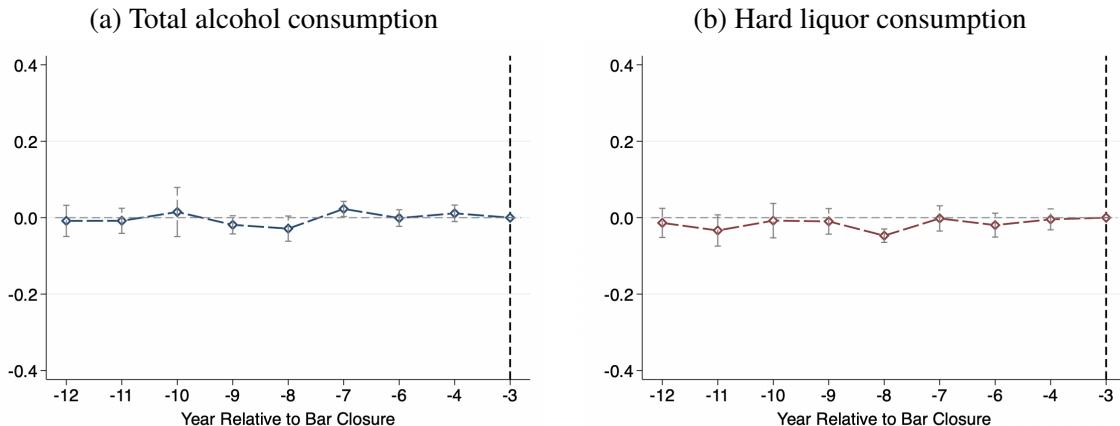
**Standard Parallel Trend Assumption.** Using data on household consumption for nine pre-policy periods,  $\tau = -12, \dots, -7, -6, -4, -3$ , we run the following regression:

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

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  $t$ ,  $I_\tau$  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  $\mathbf{X}_{hdst}$  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,  $\theta_d$  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 the control and treatment states.<sup>27</sup>

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

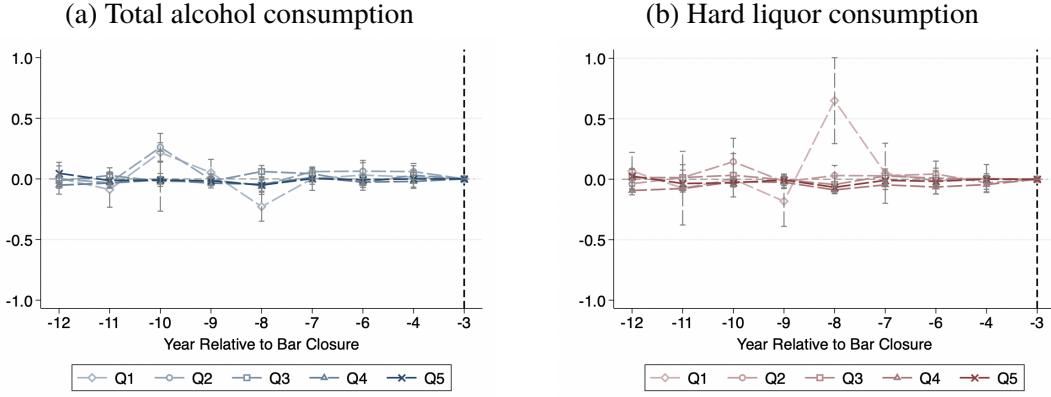


*Notes:* The figure shows results from event study analysis testing parallel pre-trends in household's total alcohol consumption and consumption of hard liquors in Kerala and its neighboring states (Karnataka and Tamil Nadu). The analysis uses the NSS for nine years before Kerala's liquor ban (2001-2002 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)).

<sup>27</sup>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.

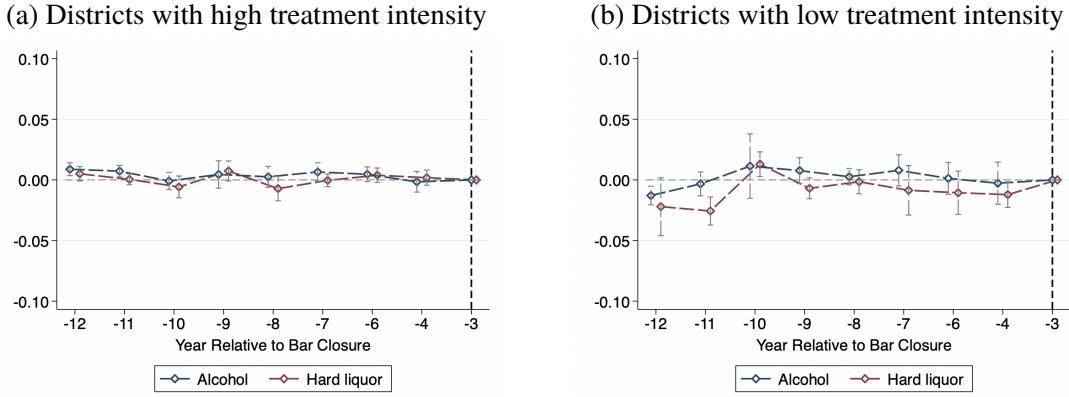
Figure 4: Event Study—Test of Parallel Pre-Trend in Household Alcohol Consumption among Heterogeneous Households



*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).<sup>28</sup> We check the 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. 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



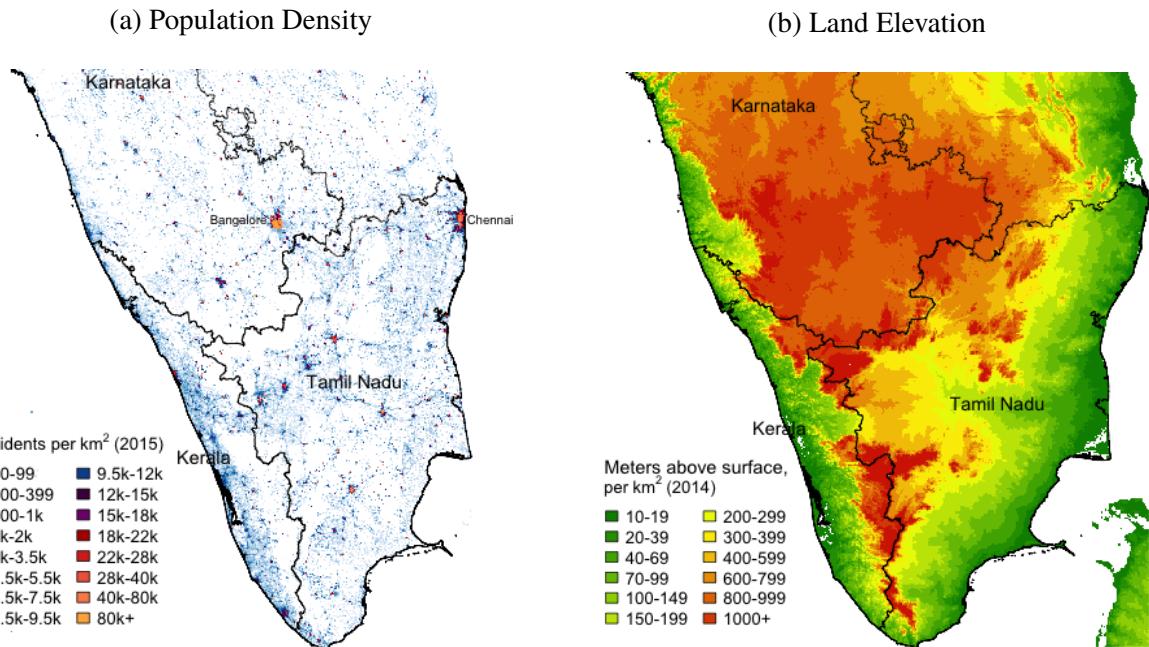
*Notes:* Panels (a) and (b) show results from event study analysis testing strong parallel pre-trends in household consumption of hard liquor and total alcohol consumption for treated districts with “dose” of treatment above (high) and below (low) the median treatment intensity, respectively. Treated 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. 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.

<sup>28</sup>Callaway et al. (2024) show that treatment groups with different “doses” of treatment could be inherently different 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.

As shown in Table B.1, the district's treatment intensity is uncorrelated with local economic conditions, further supporting the plausibility of the strong parallel pre-trends assumption.

**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).<sup>29</sup>

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



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

<sup>29</sup>The state of Kerala occupies the south-west corner of the Indian subcontinent with beaches spread along the 550 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.

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.<sup>[30](#)</sup> 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 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.<sup>[31](#)</sup>

### 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.

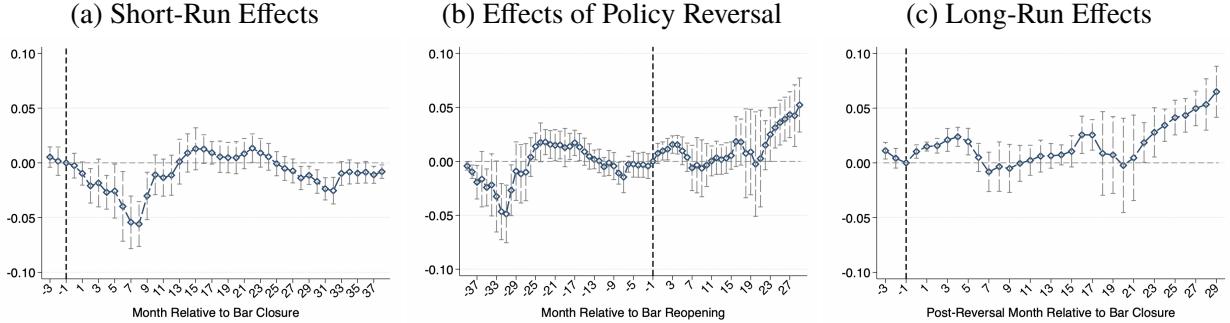
The results on the short-run effects, shown in Figure [7a](#), suggest that household alcohol consumption in bars fell sharply in the first eight months after introducing the ban. Starting from the 9th-month post-ban, the fall in liquor consumption weakens and goes back to its pre-ban level during the 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](#)). Alcohol consumption immediately increased for a few months right after the policy reversal and increased more significantly and persistently in about two years following the reversal.

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<sup>[30](#)</sup>The border districts include Karnataka (Dakshina Kannada, Kodagu, Mysore, and Chamarajanagar) and Tamil Nadu (Nilgiris, Coimbatore, Tiruppur, Dindigul, Theni, Virudhunagar, Tirunelveli, and Kanniyanakumari).

<sup>[31](#)</sup>[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.

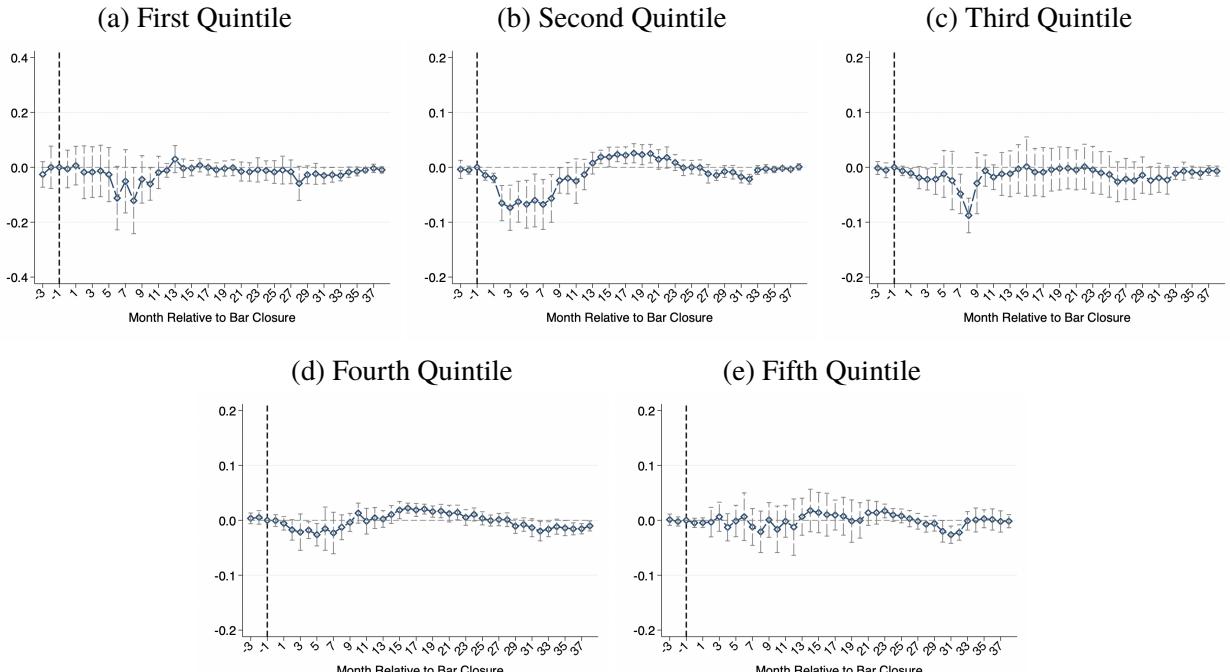
Figure 7: Effects on Household Alcohol Consumption in Bars



*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.

Next, we 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 (Figure 8).

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

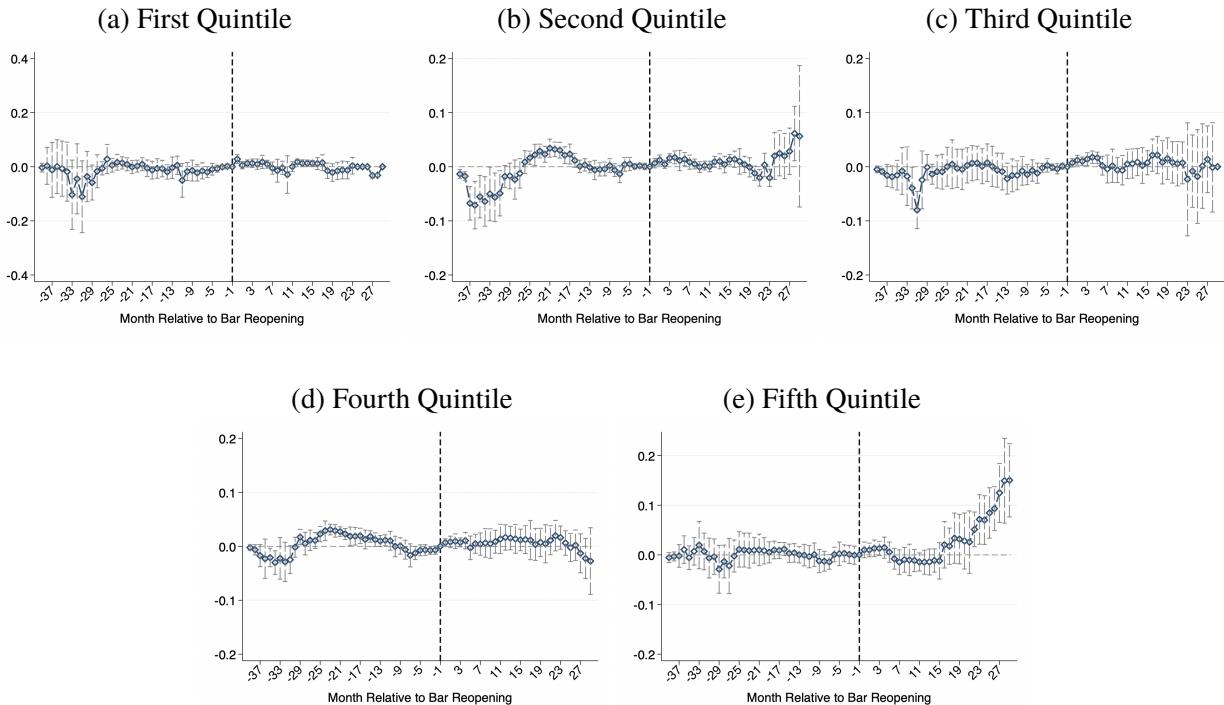


*Notes:* The figure presents the short-run effect of closing hard liquor-selling bars on (log) household expenditure on alcohol for consumption in bars during the policy period heterogeneous by household income. The treatment intensity is our baseline measure based on the district's share of hard liquor 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.

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.

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.

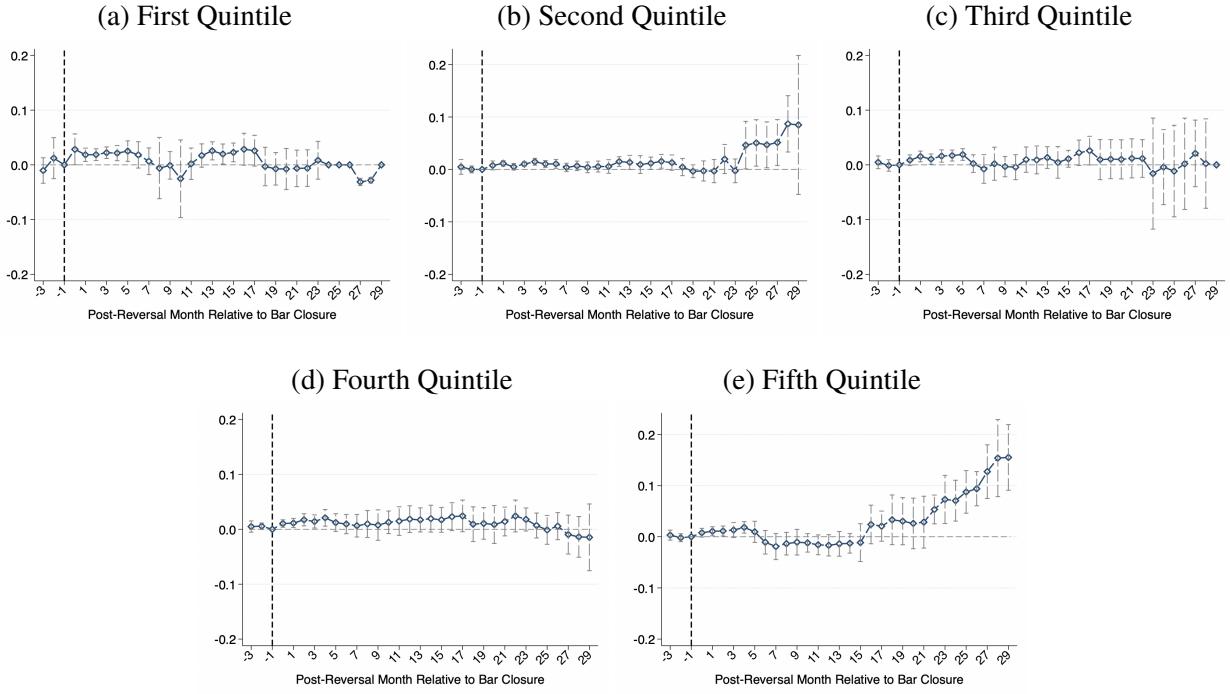
Figure 9: Effects of Policy Reversal on Household Alcohol Consumption in Bars, Heterogeneous by Household Income



*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 sharp and persistent increase in alcohol consumption in bars is concentrated among the highest-income households in the fifth quintile (Figure 10, panel (e)). We also see some positive impact among households below the fifth quintile right after the policy removal (Figure 10, panels (a)-(d)).

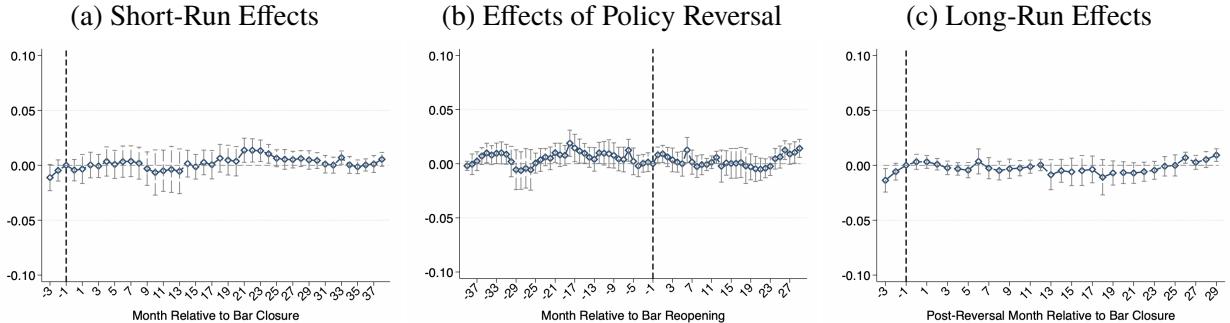
Figure 10: Long-Run Effects on Household Alcohol Consumption in Bars, Heterogeneous by Household Income



*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 11 presents the results on the average effects over the short and long run. We find no evidence of a transition of alcohol consumption from bars to home.

Figure 11: Effects on Household Alcohol Consumption at Home



*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.

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.

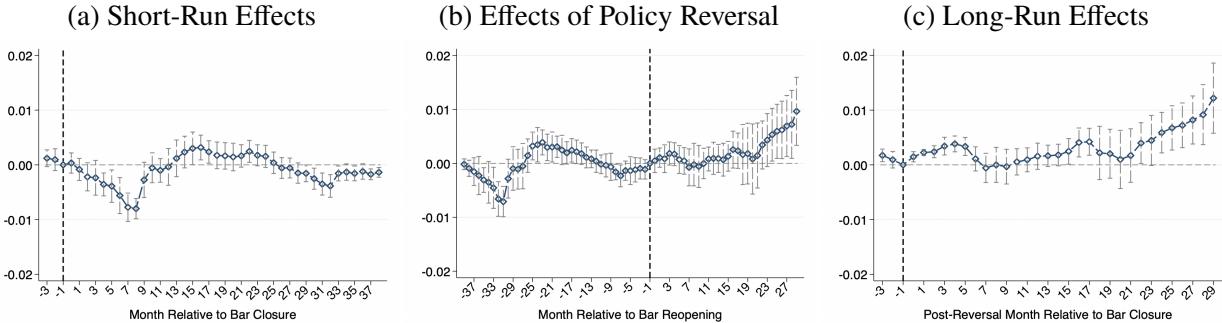
#### 4.4 Robustness

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 12 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.

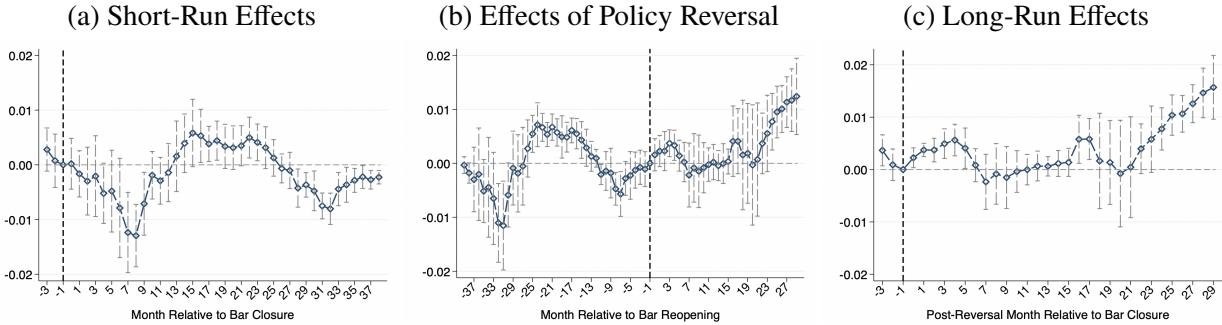
Figure 12: Effects on Household Alcohol Consumption in Bars  
(Treatment intensity = District's per capita consumption of hard liquor)



*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 treated district per million population 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 0.83. As shown in Figure 13, our results remain the same when using this second alternative treatment intensity measure.<sup>32</sup>

Figure 13: Effects on Household Alcohol Consumption in Bars  
(Treatment intensity = Number of bars closed down per million population)



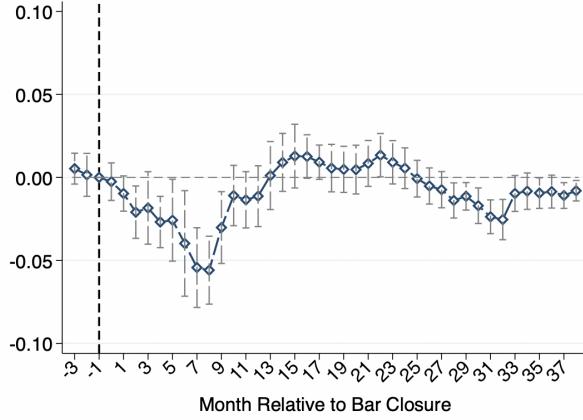
*Notes:* Panel (a) shows the effects of shutting down hard liquor-serving bars during the policy period. Panel (b) presents the impacts of policy removal. Panel (c) reports the policy effects over the period when the policy was no longer effective, i.e., long-run effects. The dependent variable in all 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 in the treated district per million population.* 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

<sup>32</sup>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.

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 14, 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 14: 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.

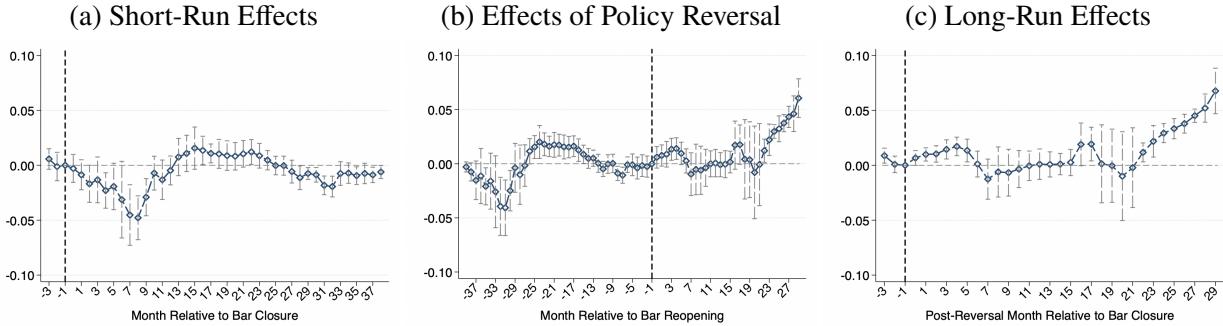
**Cross Border Spillovers.** We check whether our results change when only the bordering districts of neighboring States are used as control. This check is important because those close regions in the control group could be affected by the treatment through the immigration of individuals in response 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,<sup>33</sup> 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

<sup>33</sup>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.

in the control group. As presented in Figure 15, using the border districts of Kerala as the treatment group provides highly robust results.

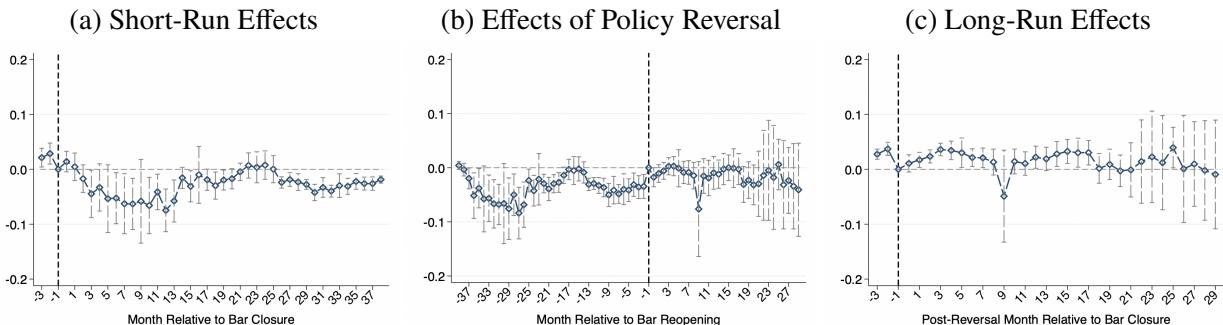
Figure 15: Effects on Household Alcohol Consumption in Bars  
(Treatment group = Border Districts of Kerala)



*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 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 16. The short-term effect generally remains the same (panel (a)). However, the effect of policy reversal disappears, especially the effect that we 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 erased. 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.

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



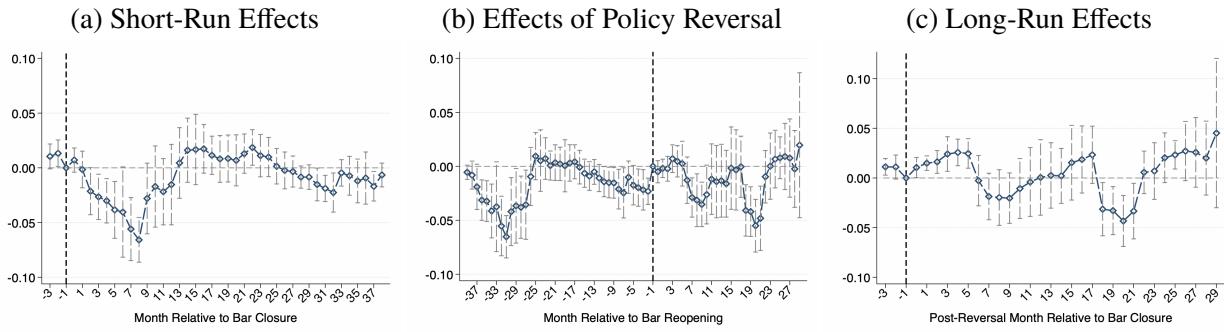
*Notes:* Panel (a) shows the effects of shutting down hard liquor-serving bars during the policy period. Panel (b) presents the impacts of policy removal. Panel (c) reports the policy effects over the period when the policy was no longer effective, i.e., long-run effects. The dependent variable in all 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.

## 4.5 Mechanisms and Sources of Changes in Alcohol Consumption

We evaluate and discuss several potential sources of changes in households' alcohol consumption, especially an increase in alcohol expenditure in bars in about two years after the ban reversal.

**Extended Bar Operating Hours.** In April 2018, the government of Kerala implemented a new liquor policy that extended the operating hours of bars in the state's capital, Thiruvananthapuram. The objective of this policy was to attract more tourists to the city, and the rule was not applicable in other districts of the State.<sup>34</sup> To examine if this policy plays a role in explaining the patterns of household alcohol consumption, we estimate our consumption regressions by excluding Thiruvananthapuram from the treatment group. Figure 17 presents the results.

Figure 17: Effects on Household Alcohol Consumption in Bars  
(Excluding Thiruvananthapuram from the Treatment Group)



*Notes:* The table shows the short-run effect of liquor ban, the impact of policy reversal, and the long-run effect of liquor ban on household's alcohol consumption in bars using specifications in which the capital of Kerala, Thiruvananthapuram, has been excluded from the treatment group. 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). Standard errors are clustered by districts, and 95% confidence intervals are shown.

Dropping the district that another alcohol-related policy after several months of policy reversal does not change the short-term effect of the ban and the immediate impact of the ban reversal (panels (a) and (b)). However, it accounts for the sharp and persistent increase in alcohol consumption in bars that starts approximately 20 months after the ban reversal (panel (c)). Therefore, consumers take advantage of an extra hour of bar operation, consume more alcohol, and spend more on drinks in bars. As expected, the new policy did not affect the alcohol consumption at home (Figure D.1).

**Aggregate Demographic Changes.** In our baseline analysis, we account for changes in the household size, which could affect the household's total alcohol expenditure, by controlling for household size in our regressions. However, household alcohol consumption may have been influenced by demographic shifts at the aggregate level, in addition to changes in local economic conditions. Thus, to evaluate the potential role of demographic changes, we add district-level population in our event study regressions using data on the projection of district-wise annual population from the

<sup>34</sup><https://www.onmanorama.com/news/kerala/2018/03/15/bar-timings-extended-tourist-zones-kerala.html>

International Institute for Population Sciences (IIPS).<sup>35</sup> Figure D.2 illustrates the dynamic treatment effects. The coefficient on the district-level population is not statistically significant, and the event-study plots are similar to the baseline plots. We thus rule out the role of aggregate population changes in explaining the effect of the liquor ban and its reversal on alcohol consumption.<sup>36</sup>

**Changes in the Number of Bars.** As discussed in Section 2, the partial liquor ban under consideration shuts down 719 bars that sell hard liquor. Our finding suggests that alcohol consumption in bars sharply declined for about a year following the closure and went back up thereafter. Alcohol consumption could have increased in the long term if more bars had opened compared to the pre-ban level. Kerala had 718 bars in November 2022 and 836 bars in October 2024, reaching a higher number of bars than pre-ban level.<sup>37</sup> However, the number of bars gradually increased over time, rather than immediately exceeding the pre-ban number of bars. In late 2019, there were 565 bars in Kerala.<sup>38</sup> Due to a lack of more frequent data on the number of hard liquor-selling bars, we are restricted to formally check the importance of changes in the number of bars over time. However, the general trend in the number of operating bars suggests that the number of bars is not an important mechanism to explain an increase in household expenditure on alcohol consumed in bars after the reversal.

**Changes in Hard Liquor Price.** Our proxy measure of alcohol consumption is expenditure on alcohol drinks, and thus, a change can be attributed to either changes in quantity or prices. Given that there were fewer bars in Kerala during the early periods after the policy reversal compared to the pre-ban period, the price of hard liquor could have been higher than the pre-ban level, potentially explaining an immediate increase in household expenditure on alcohol in bars to some extent. However, the potential price effect is likely not to last for a long time, as the price goes back to normal as more bars open in the longer term. Although we cannot disentangle the quantity and price effects due to data limitations, the long-term pattern of alcohol expenditure in bars is likely explained by changes in the quantity consumed associated with changes in bar operating hours, as discussed above.

## 5 Effect on Intimate Partner Violence

This section examines the spillover effects of the alcohol policy changes on intimate partner violence against women. Given that the frequency that we observe domestic violence in the DHS

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<sup>35</sup>The annual population data was computed by projecting periodic Census population data, and the IIPS report includes methodological details.

<sup>36</sup>Figure D.3 reports the effects on households' alcohol consumption at home when controlling for the district's population, and results show that aggregate demographic changes have no significant impact on home drinking.

<sup>37</sup><https://www.onmanorama.com/news/kerala/2024/10/23/kerala-thiruvananthapuram-gets-one-hundred-eighteen-new-bars.html>

<sup>38</sup><https://englisharchives.mathrubhumi.com/news/kerala/number-of-bars-rises-from-29-to-565-after-ldf-govt-came-to-power-nim4sj9a>

data is different from alcohol consumption in the CPHS data, our specifications in this section are different from those in the previous section. We first describe the empirical strategy employed for quantifying the impacts and then discuss the identification assumptions. We follow up by presenting results on the average and heterogeneous effects. Finally, we discuss robustness checks and potential mechanisms.

## 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 are estimated using the difference-in-differences (DID) framework. For the domestic violence regressions, we use 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:

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

where  $\text{Domestic Violence}_{ihdst}$  is one of the outcomes on incidence and frequency of physical violence against woman  $i$  living in a household  $h$  in district  $d$  of state  $s$  by the survey year  $t = \{1999, 2015-2016\}$ .<sup>39</sup> 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.  $\text{Treat}_d$  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  $\mathbf{Z}_{idst}$  contains a set of woman  $i$ 's characteristics such as age, education, employment status, marital status, religion, and place of residence. The vector  $\mathbf{X}_{hdst}$  includes a set of household characteristics associated with individual  $i$ , such as household wealth and household size. The vector  $\mathbf{M}_{dt}$  includes district  $d$ 's (log) annual real net domestic product per capita. The district,  $\theta_d$ , and year,  $\mu_t$ , 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,  $\pi_{st}$ , control for all changes at the State level over time. The error term,  $\varepsilon_{idst}$ , 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.

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<sup>39</sup>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.

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 two most recent rounds of India’s DHS data — DHS-4 (2015-2016) and DHS-5 (2019-2021), and estimate the following regression:

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

where  $t = \{2015-2016, 2019\}$ . The parameter  $\beta$  captures the impact of policy reversal in 2017. 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,  $\text{Treat}_d$ , in the regressions estimating the effect of policy reversal, recall that the control group consists only of Karnataka.<sup>40</sup> Other variables are similar to those in equation (5).

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

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

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,  $\text{Treat}_d$ , and other variables are similar to those in equation (6).

## 5.2 Identification and Assumptions

Similar to our consumption regressions, we 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

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<sup>40</sup>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.

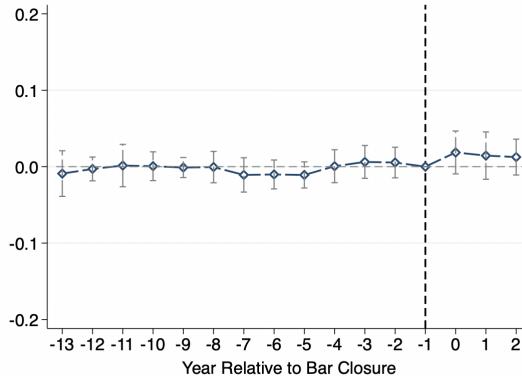
stations should be a significant predictor of individual-level physical violence, parallel pre-trends in district-level records of domestic violence should have meaningful implications of pre-trends in individual-level physical violence.

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. 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,  $\tau = -13, -12, \dots, -2, -1, 0, 1, 2$ , and we run the following regression:

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

where  $\text{Domestic Violence}_{dt}$  is domestic violence against women per 1000 population<sup>41</sup> in district  $d$  at year  $t$ ,  $I_\tau$  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).  $\text{Treat}_d$  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.

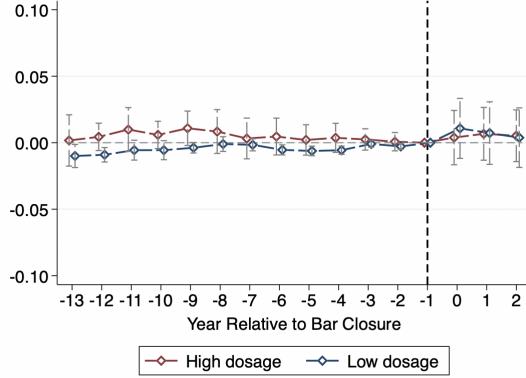
We also fail to find a significant impact of the ban on district-level domestic violence during the policy period (Figure 18), which formally shows that parallel post-trends are likely in our setting.

<sup>41</sup>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.

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. No policy changes affected 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 embedded in 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,  $\pi_{st}$ , capture other state-level policy changes. Third, our results are robust to excluding Tamil Nadu from the control group (see Section 5.5).<sup>42</sup>

**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.

<sup>42</sup>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.

**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.<sup>43</sup>

### 5.3 Average Effects

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.

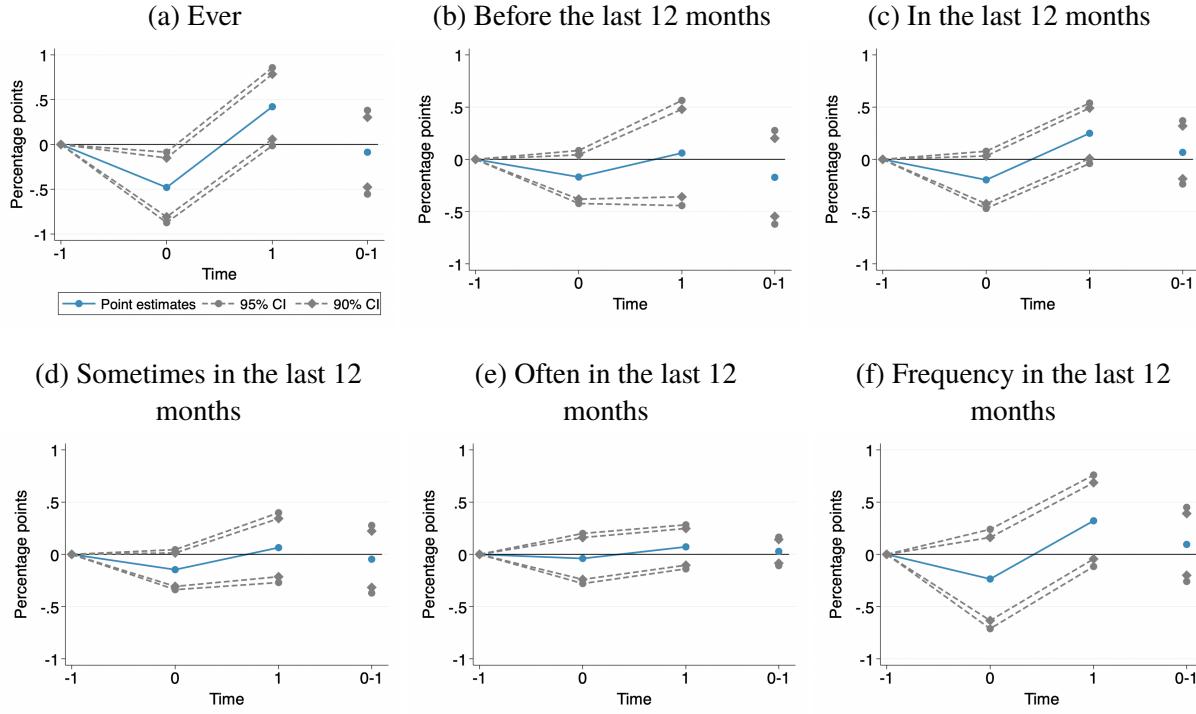
We first disaggregate the impact of the treatment on physical violence with different timings to include (i) ever-experiences of physical violence before the past 12 months (panel (b)) and (ii) ever-experiences of physical violence in the past 12 months (panel (c)). Second, we further disaggregate the 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 treatment did not have significant short- and long-run impact on any of these measures.

Several potential reasons could lead to the alcohol ban having a statistically insignificant impact

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<sup>43</sup>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.

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 Heterogeneous Effects

We first conduct heterogeneity analysis by household wealth<sup>44</sup> 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.<sup>45</sup> 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.<sup>46</sup>

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 most significantly reduced the likelihood of less frequent physical violence in the last 12 months 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

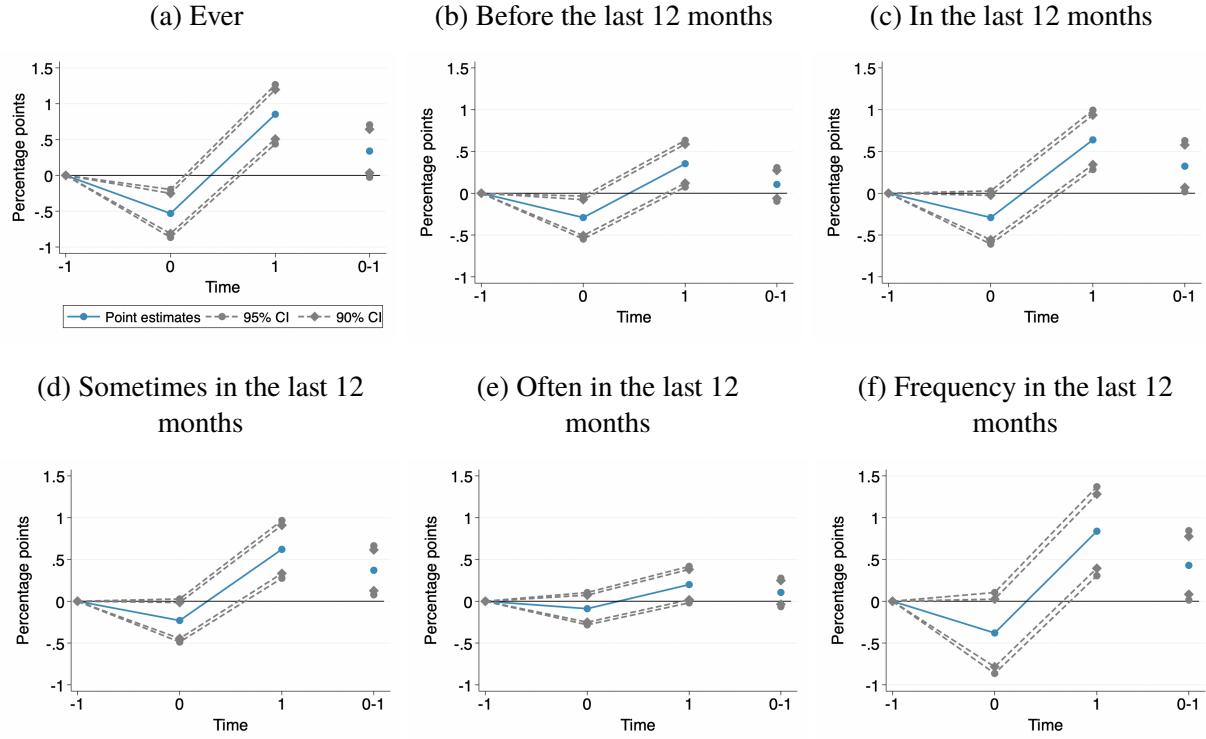
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<sup>44</sup>We leverage household wealth because the DHS data reports household wealth index rather than household income. Because the DHS is cross-sectional data, there is no initial or pre-treatment wealth index for a given household to be used for estimating heterogeneous effects by wealth index. Therefore, we use the household wealth index in the survey year, which was not affected by the treatment. To evaluate the policy effect on household wealth, we estimate the household wealth regression using specification (5) with wealth index excluded from the  $\mathbf{X}_{hdst}$  vector, and the effect was essentially zero ( $\hat{\beta}$ : 0.01, SE: 0.01,  $p$ -value: 0.34).

<sup>45</sup>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.

<sup>46</sup>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. Outcomes in panels (a)-(f) are the same as those in panels (a)-(f) of Table 20. The key explanatory variable is our baseline measure of district-level treatment intensity interacted with the household wealth index. The point estimate, plotted in this figure, is the estimated coefficient on the triple interaction term,  $\text{Treat}_d \times \text{Post}_t \times \text{Wealth Index}_{hdst}$ .

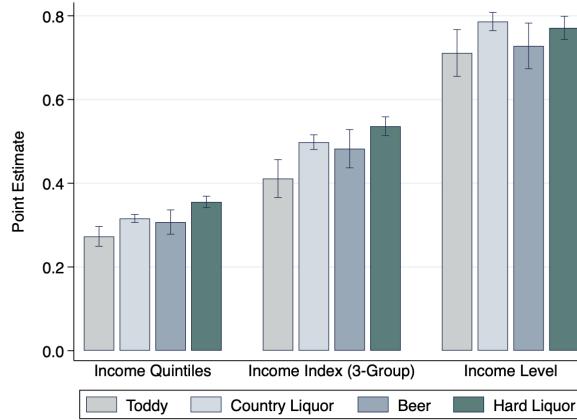
in monthly per capita hard liquor consumption, generating a 0.02 percentage point reduction in physical violence sometimes experienced by women within the past 12 months. However, there is no significant reduction in the likelihood of *often* experiencing physical violence within the past 12 months (panel (e)), which is consistent with the notion that alcohol controls are less effective in reducing extreme drinking (Carpenter et al. (2016)). This suggests that changes in the less frequent physical violence within the last 12 months drive our results. 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.

In the long run, after the policy reversal period, physical violence increased substantially amongst high-income households. Particularly, 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, outweighing the 0.03 percentage point decrease during the policy period. 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 was removed. 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 against women.

These results indicate 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.

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.

Second, since we find that a woman's own alcohol drinking is positively associated with her likelihood of experiencing domestic violence in Table 3, we estimate heterogeneous effects by a woman's alcohol drinking status. Before estimating the domestic violence effects heterogeneous by women's drinking status, similar to the household wealth heterogeneity, we check if the policy had an effect on women's alcohol drinking itself. The results from women's drinking status regression using specification (5) suggest that the effect of the ban is essentially zero ( $\hat{\beta}$ : -0.001, SE: 0.001, *p*-value: 0.31). Next, we estimate heterogeneous effects on domestic violence by women's drinking status and find that the effects of the ban on all domestic violence outcomes were not statistically significant (the results are available on request).<sup>47</sup> Table 4 presents the results of the heterogeneous effect of women's drinking status across households with different wealth status.

The ban reduced physical violence against women who drink alcohol in wealthier households during the policy period (Panel A). As shown in Table 3, they are the ones who experience more violence within the household. The effect of the ban on women who do not drink is also statistically significant for some domestic violence outcomes; however, the magnitude of the effect is negligible.

<sup>47</sup>Husbands' alcohol drinking status has many missing values and is only available for 10% of the sample, which limits us from consistently estimating the effect of the policy on husbands' drinking behavior using the DHS data.

Table 4: Heterogeneous Effects of the Temporary Liquor ban on Physical Violence by Household Wealth and Woman's Drinking Status

	Dependent variables: Domestic violence outcomes					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Short-Run Effects						
Treat × Post × Wealth × Woman drinks	-0.410*** (0.079)	-0.190** (0.077)	-0.218*** (0.056)	-0.208*** (0.050)	-0.014 (0.021)	-0.233*** (0.070)
Treat × Post × Wealth	-0.005*** (0.002)	-0.003** (0.001)	-0.003* (0.002)	-0.002* (0.001)	-0.001 (0.001)	-0.004 (0.002)
N	15705	15703	15703	15703	15703	15703
R <sup>2</sup>	0.14	0.07	0.12	0.12	0.03	0.10
Panel B. Effects of Policy Reversal						
Treat × Post × Wealth × Woman drinks	0.083** (0.037)	0.019 (0.029)	0.062* (0.035)	0.044 (0.033)	0.037 (0.029)	0.099* (0.057)
Treat × Post × Wealth	0.009*** (0.002)	0.004** (0.001)	0.006*** (0.002)	0.006*** (0.002)	0.002* (0.001)	0.008*** (0.003)
N	5925	5925	5925	5925	5925	5925
R <sup>2</sup>	0.17	0.04	0.17	0.16	0.06	0.16
Panel C. Long-Run Effects						
Treat × Post × Wealth × Woman drinks	-0.362*** (0.080)	-0.193** (0.079)	-0.175** (0.070)	-0.179*** (0.056)	0.018 (0.039)	-0.156 (0.100)
Treat × Post × Wealth	0.003* (0.002)	0.001 (0.001)	0.003** (0.002)	0.004** (0.001)	0.001 (0.001)	0.005** (0.002)
N	9072	9071	9071	9071	9071	9071
R <sup>2</sup>	0.18	0.05	0.21	0.20	0.06	0.18

*Notes:* The table presents the heterogeneous effects of Kerala's liquor ban episodes by women's drinking status among high-income households on six different outcomes of domestic violence. Outcomes in Columns (1)-(6) are the same as those in panels (a)-(f) of Table 20, respectively. Specifically, the outcomes are a dummy indicating whether a woman ever experienced a physical violence (Column (1)), a dummy indicating whether a woman ever experienced a physical violence before the past 12 months (Column (2)), a dummy indicating whether a woman ever experienced a physical violence in the past 12 months (Column (3)), a dummy indicating whether a woman experienced a physical violence sometimes in the past 12 months (Column (4)), a dummy indicating whether a woman experienced a physical violence often in the past 12 months (Column (5)), and a categorical variable for frequencies of physical violence in the past 12 months (Column (6)). The key explanatory variable is the baseline treatment variable interacted with household wealth index and the woman's alcohol drinking status. Panels A uses DHS rounds for 1999 and 2015-2016, panel B uses DHS rounds for 2015-2016 and 2019, and panel C uses DHS rounds for 1999 and 2019. The control group includes women in Karnataka and Tamil Nadu in Panel A and women in Karnataka only in Panels B and C. All regressions include an unreported constant term, individual terms, double and triple interaction terms, and baseline controls. Standard errors clustered at the district level are in parentheses. Significance: \* $p < 0.10$ , \*\* $p < 0.05$ , and \*\*\* $p < 0.01$ .

The effect of policy removal on domestic violence is more significant for women who do not drink alcohol in wealthier households in terms of magnitude. For women who drink alcohol in wealthier households, the effect of policy removal on their likelihood of experiencing physical violence is slightly noisily estimated, yet the effect is positive and about ten times larger in magnitude (Panel

B). Finally, in the long run, the policy reduced the risk of women who drink experiencing domestic violence and slightly increased the violence against women who do not drink alcohol (Panel C). Overall, the impacts of the policy episodes are heterogeneous by the women's drinking status, and the ban presents a net benefit for those who drink alcohol and are subject to a higher risk of domestic violence.

To further explore the impact of the policy among 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 E.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 E.2–E.5).

## 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.

*Per-capita Consumption.* Figure F.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 in the treated district per million population as a treatment intensity variable for the full sample of households (Figure F.2). Moreover, heterogeneity analysis by household wealth under this alternative measure (Figure F.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 F.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 F.5a shows that the treatment impacts on the incidence and frequency of physical violence estimated on the full sample are not statistically significant. Figure F.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 F.6a). Figure F.6b presents the heterogeneous impact under the same specifications and underlines the robustness of our heterogeneity analysis by household wealth.

We also 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 F.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 adjusted using wild cluster bootstrap. However, when we introduce household wealth heterogeneity, coefficient estimates become negative (Figure F.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 F.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 F.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 the terms. Here we perform the robustness check of our results on the heterogeneous short-run impact of the ban by splitting the sample to focus on heterogeneity by household wealth. Figure F.9 shows that our results on heterogeneous impacts of the partial ban on physical violence are substantially robust to this 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

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

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  $\pi_{st}$ , 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 F.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.

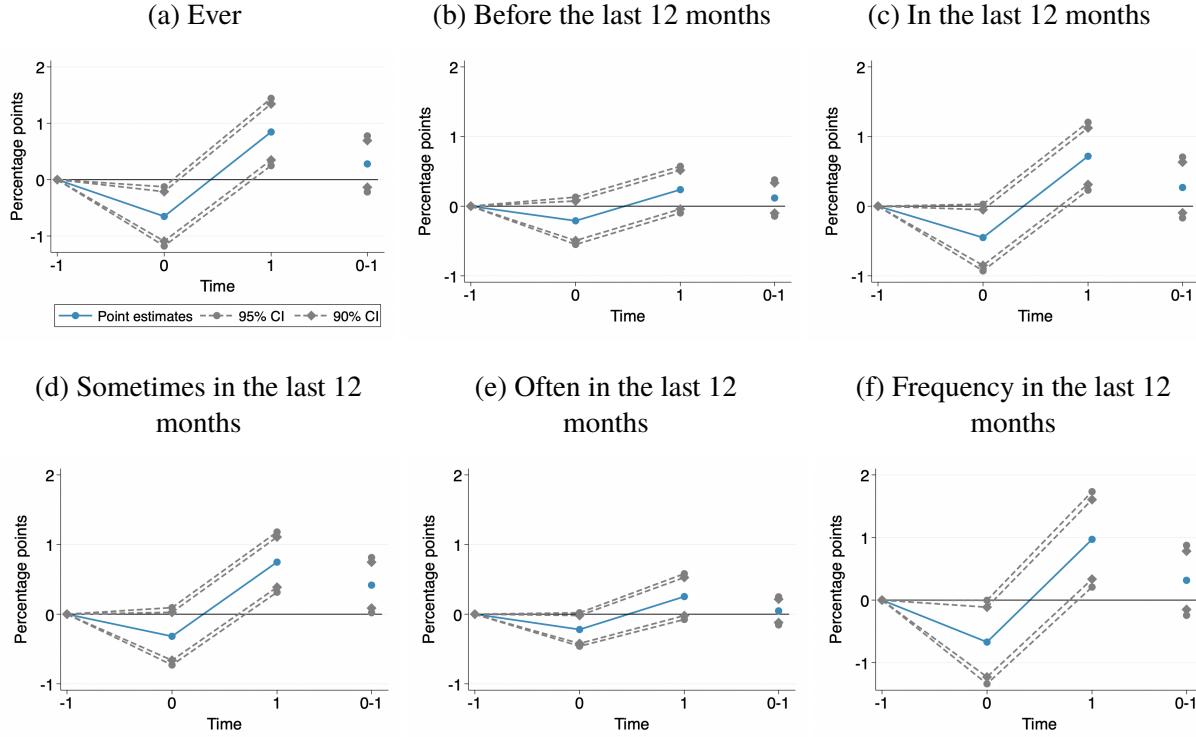
## 5.6 Mechanisms

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. Changes in alcohol consumption are the main mechanism that leads the regulation change to affect the physical violence within the household.

**Alcohol Consumption and Neuro-Behavioral Response.** The changes in alcohol consumption in bars are 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 intoxication level, leads to a reduction in intimate partner violence. 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 the likelihood of weapon carrying by teenagers, i.e., drinking may encourage violent behaviors. [Card and Dahl \(2011\)](#) also find that alcohol drinking in the aftermath of a home team loss in American football increases domestic violence in the U.S., and [Ivandić et al. \(2024\)](#) find that drinking on a soccer game day increases domestic abuse in Greater Manchester, UK.

Compared to the level during the ban, we also find a sharp increase in alcohol consumption and an associated rebound in domestic violence immediately after the reversal. We further examine if a sharp increase in alcohol consumption after Kerala's new policy in 2018, which extends the bar operating hours in the capital city, explains the weakly significant positive net effect on domestic violence in 2019. In doing so, we employ the same strategy as in Section 4 and estimate the domestic violence regressions by dropping Thiruvananthapuram from the treatment group. Figure 23 illustrates the results. The net positive effects in the long run that we identified in Figure 21 disappear when isolating the effect of the new alcohol policy in the capital city. This means that the overall impact of the ban and its reversal is null, and an increase in domestic violence in the long run is mainly because of an increase in alcohol consumption in Thiruvananthapuram due to extended bar operation hours.

Figure 23: Heterogeneous Effect on Physical Violence by Household Wealth  
(Excluding Thiruvananthapuram from the Treatment Group)



*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 using specifications in which the capital of Kerala, Thiruvananthapuram, has been excluded from the treatment group. The key explanatory variable is our baseline measure of district-level treatment intensity interacted with the household wealth index.

**Changes in Alcohol Tolerance.** In addition to an increase in alcohol consumption following the ban reversal, reduction in alcohol tolerance during the ban could also contribute to a rebound of domestic violence compared to the level during the ban period. 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 (e.g., [Ziókowski et al., 1995](#); [Martinez et al., 2010](#)). 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. In effect, changes in alcohol tolerance accompanied with some increase in alcohol consumption could have also contributed to large and strongly significant increase in violence within the household.

**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 substitution between drinking venues.

**Unemployment, Job Loss, and Absenteeism at Work.** Individuals and households that own or work in establishments selling hard liquor are adversely affected by the ban, as they lose wages, prof-

its, and jobs, and may become unemployed. Although the gender composition of the workforce is shifting in the hospitality industry, men traditionally and predominantly work more in bars in India, including Kerala, due to social norms, legal barriers, and safety concerns (Francis et al., 2023). The hard liquor ban, thus, is likely to increase male unemployment more than female unemployment. Male unemployment and job loss may have opposite effects on domestic violence. Male unemployment may reduce violence against women by improving females' relative bargaining power within the household, as in a basic intra-household bargaining model (Aizer, 2010; Anderberg et al., 2016). In contrast, male unemployment can also increase domestic violence via increased intimate partner interaction at home (Chin, 2012; Bhalotra et al., 2021, 2025). The net effect of unemployment on domestic violence thus depends on which force dominates in our context. We are constrained in evaluating the unemployment mechanism due to data limitations on the husband's employment status, occupation, and industry in India's DHS datasets. However, the role of unemployment in explaining the effect of the hard liquor ban on domestic violence in our context could be minimal since the closure of hard liquor-selling bars could raise an unemployment problem for a negligible number of men and households compared to the population.

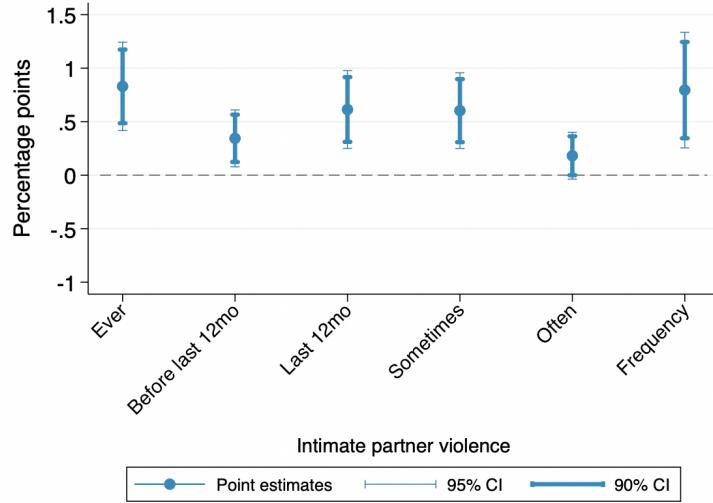
**Deviant and Offensive Behavior.** Domestic violence offenders are often involved in different forms of crimes (Cook et al., 2005; Piquero et al., 2006) and have deviant, illegal, or socially proscribed behavior (Norwood et al., 2004). While the incidence of crimes and criminal behavior can also be triggered by alcohol consumption, we check the role of changes in criminal behavior due to the alcohol policy episodes in explaining the policy impact on violence within the household by controlling for the crime incidences in the domestic violence regressions. If Kerala's liquor policy affected the criminal behavior, which explains the changes in domestic violence, the effect of policy episodes on domestic violence that we identified would disappear when we account for changes in crimes. Since the district-level crime data in India, which was obtained from the NCRB, is only available since 2001, we are constrained to estimate the impact of policy reversal using the DHS rounds for 2015-2016 and 2019 and control for the prevalence of crimes.<sup>48</sup>

We estimate the heterogeneous effects of the policy reversal by household wealth using a specification that includes the (log) total number of cognizable crimes per 1,000 population as an additional control. Figure 24 illustrates the results for the six outcomes of domestic violence. The effects are remarkably similar to those in Figure 21, indicating that the potential changes in criminal behavior hardly explain the effect of alcohol policy on intimate partner violence. We can thus rule out this potential channel.

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<sup>48</sup>We are unable to estimate the short- and long-run effects of the ban on domestic violence while controlling for district-level crimes because the regressions in (5) and (7) use the DHS round for 1999 for the pre-treatment period, and the available crime data starts in 2001.

Figure 24: Heterogeneous Effects of Bar Reopening on Physical Violence by Household Wealth  
(Controlled for Cognizable Crimes)



*Notes:* The figure presents the OLS estimates on the heterogeneous effect of policy reversal on physical violence within the household by household wealth using the same specification as in Figure 21, with the (log) district-wise total cognizable crimes per 1000 population added to the  $M_{dt}$  vector. Domestic violence outcomes are stated on the horizontal axis and are the same as those in Figure 21: (i) a dummy indicating whether a woman ever experienced a physical violence, (ii) a dummy indicating whether a woman ever experienced a physical violence before the past 12 months, (iii) a dummy indicating whether a woman ever experienced a physical violence in the past 12 months, (iv) a dummy indicating whether a woman experienced a physical violence sometimes in the past 12 months, (v) a dummy indicating whether a woman experienced a physical violence often in the past 12 months, and (vi) a categorical variable for frequencies of physical violence in the past 12 months. The key explanatory variable is our baseline measure of district-level treatment intensity interacted with the household wealth index. The point estimate, plotted in this figure, is the estimated coefficient on the triple interaction term,  $\text{Treat}_d \times \text{Post}_t \times \text{Wealth Index}_{hdst}$ .

## 6 Conclusion

This paper establishes the link between alcohol consumption and domestic violence by studying the short- and long-term impacts of a temporary alcohol control policy 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. We leverage multiple individual and household survey data and employ difference-in-differences (DID) and event-study designs, with a highly comparable and sufficiently isolated group as a control group to identify causal impacts. Using a unique household-level consumption panel data, we provide clear evidence on changes in alcohol consumption in bars during the different episodes of Kerala's temporary partial alcohol policy. We also find no substantial transition between bars and home drinking in response to the closure and reopening of bars.

At a granular level, we unravel a large 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. The incidences of physical violence rebounded in high-wealth households following the policy removal and nullified the decline in

physical violence during the policy period. Domestic violence among high-wealth households increased to a level higher than the pre-ban level due to another policy in Kerala's capital during the post-reversal period, which extends the bar operating hours. These results, overall, provide clear and causal evidence that a particular type of alcohol ban presents a differential impact on a particular type of household in terms of alcohol consumption and a decline at the extensive and intensive margin in domestic violence in these households.

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