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Sensitive places, persistent violence: Effectiveness of "Bar Ban" laws in reducing gun violence near alcohol vendors

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ABSTRACT

Americans have differing opinions on whether greater regulation of firearms results in improved public safety. One area that seems to enjoy broad support is to limit firearm access in specific locations. "Bar Ban" laws — which prohibit firearms where alcohol is served — represent one such approach, yet their effectiveness remains largely unexamined nationally. This paper provides the first comprehensive evaluation of the impact of Bar Ban laws on shootings near alcohol-related establishments. Using a geospatial panel dataset of over 1.6 million alcohol vendors active across the United States between January 2019 and January 2025, we analyze the relationship between restrictions on carrying a gun where alcohol is served and gun violence. Results show that shootings occur close to alcohol-serving establishments: across 263,464 shooting incidents, the median distance to the nearest alcohol vendor was 222 meters. To assess the effectiveness of Bar Ban laws, we employ a stacked difference-in-differences design examining monthly shooting exposure rates of alcohol vendors across counties in five states (Hawaii, Maryland, New York, New Jersey, South Dakota) that adopted Bar Ban laws during our study period. Our findings reveal a policy puzzle: while spatial analyses confirm that shootings routinely occur near alcohol establishments, Bar Ban laws targeting these locations show no discernible effect on reducing shooting incidents. Thus, states adopting these restrictions saw no substantial change in county-level shooting exposure rates near alcohol vendors.

1. Introduction

In the early morning hours of August 17th, 2025, four gunmen opened fire at a restaurant and bar in Brooklyn, New York. After the shooting subsided, three people were dead and nine more were injured. A local resident interview by reporters from the New York Times placed the blame for the shooting not on the gang members that were suspected of starting the incident, but on the venue itself, describing Taste of the City as a "melting pot of violence, ignorance and liquor" (Cramer et al., 2025).

This is just one example of the thousands of shootings that occur in the United States each year. In 2023, firearm deaths exceeded 46,000 (13.7 per 100,000 population) in the United States (CDC, 2024). Over 27,000 of those deaths were due to suicide, a number that has continued to climb over the past twenty years, although age-adjusted rates per 100,000 population have been fairly steady since 2014. Nearly 18,000 firearm deaths were due to homicide in 2023, a decrease from a high in 2021. Firearm related deaths per 100,000 were highest in four states: Alabama, Louisiana, Mississippi and New Mexico, and lowest in Hawaii, Massachusetts, New Jersey, and New York (CDC, 2022). Deaths and injuries due to firearms have considerable physical, psychological and financial costs that were estimated at nearly $5 billion in 2020 (Miller et al., 2024).

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Numerous studies have documented the strong association between excessive alcohol use and violence, on outcomes ranging from intimate partner violence, to assault, to suicide (Miller and Hemenway, 1999; Hohl et al., 2017; KwanWoo Choi et al., 2018; NamkeeG Choi et al., 2018; Anderson et al., 2018; Kearns et al., 2015; Hedlund et al., 2018; Borges et al., 2017a,b; Park et al., 2017; Silver et al., 2019). The shooting at Taste of the City is indicative of a broader pattern of firearm violence near alcohol-serving establishments, underscoring both public health concerns and policy challenges.

States have substantial authority to pass laws that affect their firearm and alcohol policy environments. Efforts to address firearm deaths include those that aim to reduce violence overall; those that target the ownership, purchase, and use of firearms; and efforts to limit firearm ownership by felons, those with alcohol or drug disorders, or those who may be a threat to themselves or others. States also set prices and sales restrictions for alcohol and there is a strong evidence base demonstrating the effectiveness of such restrictions on limiting alcohol use (Kilian et al., 2023), especially on binge and heavy drinking (Cook and Tauchen, 1982; Xuan et al., 2015) as well as driving under the influence of alcohol.

All else equal, states with a stronger set of alcohol policies have lower prevalence of problem drinking behaviors such as binge and heavy drinking (Silver et al., 2019). Thus, restricting access to alcohol may also have spillover effects on preventing violence.

One study identified an association between the strength of the state alcohol regulatory environment and homicide (Naimi et al., 2017), which was supported by a systematic review that found evidence of protective effects of restrictive alcohol policies on state suicide rates (Xuan et al., 2016). A more recent study determined that stronger state alcohol policy environments between 2002–2018 were associated with a 6% decrease in firearm-related homicides (Murphy et al., 2024).

Other research points to the relationship between the concentration of alcohol outlets in a given area and violent crime, though the theoretical mechanism varies. Some studies treat alcohol outlet density primarily as a proxy for neighborhood-level consumption patterns. For example, one study assessed the cost-effectiveness of possible zoning regulations on alcohol licenses in Baltimore, finding that reducing the number of liquor stores in residential zones could lead to lower homicide rates without imposing too great of an enforcement burden (Trangenstein et al., 2020). Similarly, Pear et al. (2023) assessed the relationship between alcohol outlets and violence in California and found their presence to be associated with higher monthly risk of firearm assault per 100,000, but of very low magnitude (0.01 risk difference). Another study conducted in California using data on hospital discharges and deaths related to self-harm estimated that a 20% reduction in alcohol outlet densities would be associated with a −1.59 and −0.10 risk difference for nonfatal and for fatal self-harm injuries, respectively (Charris et al., 2025). In these contexts, the presence of outlets may signal higher alcohol availability within a community, which in turn increases the risk of violence occurring throughout the neighborhood.

Our study focuses on a different mechanism: alcohol establishments as specific geographic "hotspots" where violence occurs due to patron congregation and social conflict. Unlike zoning laws that aim to reduce broad alcohol availability, Bar Ban laws target the *direct site effects* of these venues. They regulate the armed status of patrons within the establishment itself, regardless of neighborhood consumption levels. By focusing on the spatial proximity of shootings to these specific points of interest (POIs), we isolate whether restricting firearms at the point of consumption — rather than reducing the total number of vendors — lowers the localized risk of violence.

The relationship between firearm violence and alcohol use might also work in the opposite direction: firearm violence may be a key driver of alcohol use. In support of this, a national study of mass shooting events found that following such event (in particular a mass shooting that takes place in a public place), alcohol sales increased by 3.5% to 5.5% in the affected neighborhood for a period of up to 5 years afterwards (Buttrick et al., 2025).

Besides enhancing the state's overall regulatory approach to either alcohol or firearms, there are several ways policymakers have more explicitly targeted the intersection of these products. These efforts have largely focused on specific people for whom access to firearms might be restricted (defined most commonly by current or habitual alcohol intoxication or treatment status) and specific places that serve alcohol, such as bars, restaurants, night clubs or sports stadiums (Carr et al., 2010).

While laws establishing "*gun-free zones*" have been criticized for potentially increasing risks — by ensuring that only some people are disarmed and thereby creating potential targets for attackers — these policies appear to enjoy broad public support. In a nationally representative 2016 survey, the locations with the highest approval for restricting firearm carrying were sports stadiums, bars, and schools, each with support of nearly 70% (Wolfson et al., 2017). A 2023 survey largely confirmed these findings (Crifasi et al., 2023).

The empirical evidence regarding the effectiveness of such place-based gun restrictions is somewhat more limited. One study assessed the effects of laws that allowed the concealed carrying of firearms on college campuses (Gius, 2019). The study found no statistically significant effects of such policies on crime on college campuses and evidence on this policy type has been classified as "inconclusive" (Smart et al., 2024). A later study examining similar laws passed in Arkansas, Georgia, and Texas similarly found no statistically significant association between passing such policies and rates of violence on affected college campuses in these states (Kagawa et al., 2025).

A study of school-based gun free zones in Saint Louis, Missouri found no effect as well, with the authors concluding that their null findings indicated that such policies did not raise the risk of firearm violence within such zones (Reeping et al., 2023). However, a larger matched case-control study examining all US establishments where active shootings took place between 2014 and 2020 found that the odds of an active shooting in a gun-free establishment were about 62% lower than in an establishment without such a designation (Reeping et al., 2024b).

Even fewer studies focus on gun free zones in places that serve alcohol. A scoping review that examined evidence between alcohol exposures and firearm violence concluded that state policies that target the availability of alcohol, as well as those that prohibit firearm access for those with alcohol use disorder and other alcohol-related offenses, may be helpful in reducing firearm violence (Matthay et al., 2025). Another study looked at the impact of a Texas law that established gun free zones in bars and restaurants that earned at least half of their revenue from alcohol (Reeping et al., 2024a). In this cross-sectional study, researchers examined shooting events within 50 m of businesses that prohibited versus did not prohibit guns. After controlling for several important confounders, those businesses that banned guns had 37% (95% CI 0.2% to 60%) fewer shooting events than bars without such a ban in place. Interestingly, this relationship did not hold when examining a distance of 100 m from such establishments.

Considering the mixed evidence regarding their effectiveness, the passage of Bar Ban laws suggests a strategic function that varies by political climate. In permissive firearm environments like South Dakota, these bans may be the only politically feasible restrictions, framed as "common-sense" measures that target high-risk locations without infringing on Second Amendment rights. In more restrictive states like New York or New Jersey, they may serve as incremental additions to an existing regulatory suite.

Political science theorists posit that such policies are often "symbolic"—crafted to signal moral priorities or respond to public crises rather than to deliver tangible results (Edelman, 1985; Boussaguet and Faucher, 2025). In this context, Bar Bans allow legislators to visibly address the intersection of firearms and alcohol — a highly salient public concern — even if the causal evidence for their success is thin. These laws perform an expressive function for their legislative champions: they convey and reap the electoral benefits of a commitment to safety while leaving the underlying drivers of violence and alcohol misuse unaddressed—not necessarily the intent of the lawmaker, but something that would require a great deal more time and energy than they can likely spare.

In this article, we assess the impact of a subset of gun-free zone laws by focusing on state-level policies that restrict the carrying of firearms in establishments that serve alcohol on premises. Our study is different from previous efforts for several reasons. First, we focus on multiple states at once to enlarge the scope of our analysis. Second, we pursue a study design that leads to credible causal inference regarding the relationship between a state passing a law restricting firearms in places that serve alcohol and subsequent changes in violence taking place in close proximity to affected business (on-premise alcohol serving establishments such as bars and restaurants). Third, we leverage original legal research to provide relevant insights on the characteristics of each state's law, how affected places are defined, and legal exemptions and penalties for noncompliance.

2. Data & methods

2.1. Data

2.1.1. Bar Ban laws

Data on Bar Ban laws, from Bae et al. (2025), was collected by the authors following a previously published protocol for creating a dataset of US state-level alcohol-related firearm laws (Silver et al., 2024) adhering to the best practices in public health law research and policy surveillance (Burris et al., 2016a,b). A team of law students supervised by a licensed attorney conducted the survey of state laws using Westlaw beginning with the following broad search terms applied to the entirety of each state's statutory codes: `[(influence intoxicat! drunk! alcohol) /50 firearm]` and `[(influence intoxicat! drunk! alcohol) /50 gun]`. Legal research began with the most up-to-date full text versions of the statutes, then followed the legislative history to download earlier versions and construct a timeline for each law's effective date. Any substantive amendments of existing laws are coded as a new event.

Additional Westlaw research was conducted to retrieve penalties and other relevant factors. After the collection was completed, law students received training in how to code these data, then analyzed and coded all statutory texts through an iterative process. The supervising attorney checked for inconsistencies and resolved them after reviewing the legal text.

As seen in Table 1, the five states that adopted Bar Ban laws during our study period exhibit considerable variation in their approaches to restricting firearms near alcohol establishments, though several common patterns emerge. Four states (Maryland, Hawaii, New Jersey, and New York) apply similar frameworks to open and concealed carry, targeting restaurants under open carry restrictions but excluding them from concealed carry prohibitions, thus reflecting differences in state concealed carry licensing regimes. South Dakota represents a notable exception, applying a revenue-based threshold (locations deriving over 50% of income from alcohol sales) rather than the more inclusive "*where alcohol is served*" standard used by other states.

In all states, it is the patron and not the establishment owner who is responsible for complying with the law. Penalty structures vary across jurisdictions, with New Jersey imposing the most severe sanctions (mandatory minimum 1095 days imprisonment and fines up to $15,000) while South Dakota imposes no penalties at all. Law enforcement exemptions follow a consistent pattern, with open carry restrictions typically exempting both law enforcement and other specified groups, while concealed carry restrictions exempt only law enforcement personnel. Despite these variations in scope and enforcement, no state includes provisions for carry or purchase permit revocation as a consequence of violations. Hawaii uniquely extends restrictions to adjacent parking areas, regulating a broader spatial zone around alcohol establishments.

To assess the effects of Bar Ban laws adopted during our period of study, we compare the 5 states adopting laws *for the first time* with those that did not have a Bar Ban law at any time during this same period, resulting in 25 control states: AL, CA, CO, CT, DE, GA, ID, IN, IA, KS, ME, MA, MN, MT, NV, NH, OR, PA, RI, UT, VT, VA, WA, WV, and WY. Due to the inferential assumptions associated with our identification, we do not include any states that were treated by Bar Ban laws prior (before 2019) to our study period—valid treated units can only be those that adopt Bar Ban laws during the study period, and valid control units can only be states that were never treated during the study period. While this approach excludes some states with previously-adopted laws, it provides a robust assessment of those that passed a new law within the time frame of our analysis.

2.1.2. Gun violence incidents

Data on the locations of 263,464 shooting incidents occurring between January 1st, 2019 and January 31st, 2025 were provided by the Gun Violence Archive (2025). The GVA is a non-profit organization that collects incident reports from over 7500 law enforcement, media, government, and commercial sources daily with the goal of providing free, online, and public access to accurate information about gun violence in the United States.

Because our identification strategy depends on precise and consistent location information, we re-geocoded all incidents using ArcGIS Pro 3.5.1 (Esri Inc., 2025), a commercial off-the-shelf geospatial platform. This ensured that incidents originally geocoded with varying services were standardized, reducing spatial inconsistencies (see Section A.1). A recent evaluation of ArcGIS geocoding capabilities found that geocoded addresses could be linked to external environmental and socioeconomic datasets with very high concordance (Johnson et al., 2025).

Our primary outcome includes all firearm-related incidents recorded by the GVA within our spatial buffers, which may include interpersonal violence, unintentional firearm injuries, and self-directed harm as well as the assaults and homicides that Bar Ban laws may be primarily intended to prevent. Theoretically, these laws aim to reduce armed presence in a broad "zone of influence" around alcohol establishments, so excluding specific incident types based on GVA classifications — which rely on media or police reports to be recorded in the data, and are not always present depending on the detail in the source report — could introduce non-random measurement error. Furthermore, GVA data is known to systematically undercount suicides and suicide attempts, as these incidents are less frequently captured by the media and public reports that form the basis of the archive (Gobaud et al., 2023). Consequently, self-directed shootings represent a very small fraction of our dataset. By retaining all verified incidents and not restricting by classification codes, we provide a less selective test of whether Bar Ban laws reduce the aggregate burden of firearm violence in the vicinity of regulated establishments.

2.1.3. Alcohol-related points of interest

Alcohol sales in the United States are highly regulated, with licensing systems that vary widely across and within states. Because licenses are issued by different authorities — state, county, or municipality — there is no comprehensive national dataset of alcohol vendors, nor data on their operating dates over multiple years. In the absence of such administrative data, we used Point of Interest (POI) data from Advan Research (2022), accessed via Dewey Data. Advan's Monthly Patterns dataset aggregates anonymized, location-based signals from a panel of mobile devices to estimate visits to specific businesses, supplemented with SafeGraph Places and Geometry data linked via Placekey identifiers. This integration of mobile device activity with standardized place identifiers provides consistent coverage of business activity

Table 1
Bar Ban laws in states recently passing new legislation.

State	Date	Type	Location	Description	Exemptions	Penalties
MD	10/1/23	OC	Where alcohol is served, including restaurants	Limited to places for on-site consumption; does not apply to CC	LEO and others	0–365 days, $0–1000 fine
		CC	Where alcohol is served, excluding restaurants	Limited to places for on-site consumption	LEO only	0–365 days, $0–1000 fine
NJ	12/22/22	OC	Where alcohol is served, including restaurants	Does not apply to CC	LEO and others	1095–1825 days, $0–15 000 fine
		CC	Where alcohol is served, excluding restaurants	N/A	LEO only	1095–1825 days, $0–15 000 fine
SD	7/1/19	CC	Where alcohol is served, including restaurants	If location derives over 1/2 of total income from alcohol sales	None	No penalties (no permit required)
		OC	No law			
HI	7/1/23	OC	Where alcohol is served, including restaurants	Full premises, including adjacent parking areas; does not apply to CC	LEO and others	0–365 days, $0–2000 fine
		CC	Where alcohol is served, excluding restaurants	Full premises, including adjacent parking areas	LEO only	0–365 days, $0–2000 fine
NY	9/1/22	OC	Where alcohol is served, including restaurants	Does not apply to CC	LEO and others	0–1460 days, no fine
		CC	Where alcohol is served, excluding restaurants	N/A	LEO only	0–1460 days, no fine

Note: OC = Open Carry, CC = Concealed Carry, LEO = Law Enforcement Officers. All laws apply to on-premise alcohol service locations. Penalties shown as prison days and fine amounts in USD.

across the country and, in lieu of a comprehensive national dataset of alcohol vendors, serves as a practical proxy for identifying active alcohol-related establishments. From this dataset, we identified all active alcohol-related businesses by filtering on North American Industry Classification System (NAICS) codes. While local-level regulations (e.g., "dry" jurisdictions) may restrict alcohol availability, these time-invariant factors are largely accounted for by our fixed-effects strategy, and such locations would naturally be excluded from our count of active alcohol-serving POIs, as discussed in the following sections.

We classify businesses into five categories based on their NAICS codes: (1) Covered POIs consisting of any place that serves alcohol for on-premise consumption; (2) "Drinking Places" POIs (a subset of Covered POIs); (3) Restaurant POIs (a subset of Covered POIs); and, as placebos since they are not subject to the bar ban laws, (4) "Liquor & Convenience" POIs; and (5) Tobacco Store POIs. The broad Covered POIs category is designed to provide a comprehensive, though potentially over-inclusive, view of the commercial establishments most likely to be directly regulated by Bar Ban laws. Drinking Places (NAICS 722410) are defined as establishments like bars and nightclubs that primarily serve alcohol for on-site consumption and represent the businesses we hypothesize would be most directly affected by Bar Ban laws. Restaurant POIs (NAICS 722511 and 722513) comprise all full- and limited-service restaurants—we assume these serve alcohol, though we acknowledge that including non-alcohol-serving restaurants may dilute treatment effects (a limitation discussed further in Appendix Section A.3).

Liquor & Convenience POIs include off-premise alcohol retailers such as liquor stores, convenience stores, and gas stations (e.g., NAICS 445310, 445120, 447110), excluding supermarkets. Tobacco Store POIs (NAICS 453991) are included as an additional placebo test, as tobacco and alcohol are both "vice" goods and are often co-regulated at the state and local level, through age restrictions or zoning regulations, for instance (Pacula et al., 2014). However, while tobacco retailers are not typically the target of Bar Ban laws or similar interventions that aim to reduce social congregation or alcohol-related harms, similarly to alcohol vendors, these POIs have been identified as being potential hotspots of violence (Subica et al., 2018).

The inclusion of Liquor & Convenience POIs and tobacco stores thus serves two key theoretical purposes. First, from an identification strategy standpoint, they help us evaluate the extent to which any observed changes in behavior or outcomes are *specifically* driven by policies targeting alcohol-serving venues, rather than broader shifts in consumer behavior, commercial activity, or patterns of violence near commercial spaces. If Bar Bans were driving general changes in foot traffic or retail activity — irrespective of the business type — we might expect to see parallel changes at off-premise alcohol retailers or tobacco outlets. Second, drawing on insights from the difference-in-differences (DiD) and synthetic control literatures (Bonander et al., 2021), placebo units like off-premise alcohol retailers and tobacco stores provide a useful falsification test. Because these POIs are not directly exposed to the treatment, they help test for violations of the parallel trends assumption or for the presence of unmeasured confounders.

We provide additional detail on the included POI subclassification in Appendix Section A.3. In some of our descriptive findings, we include an additional category — All Alcohol-Related POIs — that is intended to be inclusive of all commercial establishments with direct ties to alcohol.

2.2. Identification strategy, defining exposure, & study period

Our identification strategy centers on carefully defining the spatial and temporal dimensions of alcohol-related establishments exposed to shootings, while delineating the units that could plausibly be affected by treatment or otherwise exposed. To do so, we develop empirically grounded buffer distances that capture both the uncertainty in recorded shooting locations and the "zones of influence" around alcohol-related POIs. This refers to the area within which activity at an establishment may reasonably be affected by an external event—accounting for factors such as pedestrian and vehicle traffic patterns, line-of-sight, and proximity to entrances and gathering areas. These POI attributes, combined with local geographic characteristics, allow us to more accurately represent the set of locations plausibly exposed to the Bar Ban laws by constructing buffers around each POI in our data.

We identify the set of exposed establishments using these buffers, as well as the full population potentially at risk, ensuring that our exposure metrics accurately reflect treatment intensity while reducing the risk of introducing bias. We select counties as our geographic unit of analysis due to their administrative relevance, availability of data, and spatial granularity. The limitations of relying on Euclidean distances are addressed further in the limitations section. The following sections detail our identification strategy, which is also visually demonstrated in Fig. 1. Our study period spans January 1st, 2019 to January 31st, 2025. This window provides a sufficient pre-treatment baseline for the five treated states and allows for 12 to 24 months of post-treatment observation, which is critical for capturing both the immediate and sustained impacts of Bar Ban laws. The 2019 start date is specifically dictated by the availability of high-quality Point of Interest (POI) data from Advan Research, as comprehensive tracking at the level of granularity required for this analysis did not exist prior to this year. While an earlier start date might theoretically offer a longer baseline, supplementing the Advan dataset with alternative sources would introduce measurement inconsistencies due to differing data collection methodologies. By anchoring the analysis to a single POI data source and extending the study through January 2025 — the most recent data available at the time of analysis — this timeframe ensures the highest possible internal validity and minimizes the risk of confounding from data-source heterogeneity.

2.2.1. Capturing spatial uncertainty around shooting locations

We implement a spatial buffering approach to account for potential positional uncertainty in geocoded shooting locations to account for measurement error that could systematically bias our distance calculations to nearby alcohol establishments. We create radial “shooting buffers” around each incident location that capture the spatial extent of uncertainty, tailored to the urban geography surrounding each incident.

We estimate each shooting buffer by calculating the mean block length for the area around each shooting by taking the average census block length of the five census blocks (the block in which the incident occurred and its four nearest blocks) most proximate to the shooting location. This approach recognizes that geocoding precision varies with local street network density and urban form—areas with shorter blocks typically have more precise address-based geocoding, while areas with longer blocks may experience greater positional uncertainty.

The typical block length for the areas surrounding a gun violence incident (shooting) is 467 m (approximately 1500 ft), which would yield a shooting buffer radius of 233.5 m (about 766 ft). This buffer distance represents a conservative estimate of potential geocoding error, assuming that the true shooting location could fall anywhere within approximately half a block’s distance from the recorded coordinates. By using half the mean block length, we account for the possibility that an incident geocoded to a specific address might actually have occurred anywhere along that block or at nearby intersections.

However, instead of using incident-specific buffers (which could prove to be far too computationally expensive, given the sheer number of incidents), we use one of two possible shooting buffers for each shooting, as seen in Fig. 1: (1) a buffer based on the average of all shooting buffers for each county (“county buffers”—the green circles in Fig. 1); or (2) a buffer based on the average of all shooting buffers for each state (“state buffers”—the red circles in Fig. 1). On average, the county buffer is smaller than the state buffer in more urban counties, and larger than the state buffer in more rural counties, as seen in Fig. 1. This standardization approach ensures that our proximity analyses between shootings and alcohol-related establishments are robust to geocoding imprecision, while avoiding overly broad spatial definitions that might obscure meaningful geographic relationships. Alcohol establishments intersecting with these buffers would therefore be considered exposed to a given shooting, while those outside of the shooting buffers bounds would make up the potentially-affected full population of units that act, in effect, as our offset.

2.2.2. Capturing an establishment’s “Zone of Influence”

Geocoding processes for POIs often place establishments at parcel centroids or nearest street addresses, potentially introducing positional error that could bias our proximity analyses. To address this limitation, we leverage building footprint and geofencing data to better understand the spatial extent of alcohol-serving establishments. This data is available for a subset of units in the Advan data, enabling more precise attribution of activity or events to specific establishments, which is detailed in Section A.2.

Using footprints for a random sample of approximately 820,000 of the 1.6 million POIs in our data (50%), we estimate the square-meter area of the footprint of each individual POI, and calculate the average square-meter area of POIs across different subcategories of alcohol establishments (restaurants, bars, liquor stores, etc., as seen in Figures A.1 and A.2).

To account for this spatial uncertainty and identify the average establishment’s zone of influence, we construct radial buffers around each POI location designed to encompass the same square footage as the building footprint, centered on the POI’s centroid (Figure A.3). Given differences in geographic density across the country (e.g., NYC bodegas versus Texas country clubs), we estimate the average footprint size of each POI subcategory, and calculate the radial distance required to construct a buffer around every POI that captures this computed mean area.

Based on our analysis of building footprints and establishment areas, we determine that a radius of approximately 25 m (82 ft) would be needed to capture the typical spatial extent and immediate vicinity of alcohol-serving establishments, on average. For our most conservative — and preferred — model specifications, we use a 25 meter (82 foot) radial buffer around the centroid of every POI. In sensitivity analyses, we relax this requirement and expand the POI zone of influence to be 50 m (164 ft).

2.2.3. Geographic and temporal unit of analysis

While Bar Bans are implemented at the state level, the experienced policy environment may vary within states due to differences in local enforcement, compliance, or implementation practices. To address this potential confounding, we conduct our primary analyses at the county-month level. This unit of analysis offers several methodological advantages in regards to geographic granularity, statistical power, administrative alignment, local confounder control, temporal precision, and meaningful exposure rate construction.

County-months capture within-state variation in policy implementation while maintaining sufficient geographic specificity to detect localized effects around alcohol establishments. The geographic scale provides sufficient observation counts while preserving meaningful variation in both alcohol establishment density and shooting patterns that might be obscured at broader scales (such as state-year, for example).

This level of analysis further aligns with the typical administrative structure for firearm and alcohol regulation enforcement, as counties generally handle day-to-day implementation of state laws in these domains. Our county-level aggregation also enables us to control for unobserved local characteristics such as economic conditions and demographic composition that might correlate with both policy adoption and shooting outcomes.

The monthly temporal dimension captures important seasonal and cyclical patterns in both alcohol-related consumer activity and gun violence that could confound analyses using longer time periods. Importantly, this approach allows us to construct meaningful exposure rates with a consistent denominator (total establishments per county-month) that accounts for business turnover and market dynamics, while ensuring sufficient establishment density to avoid excessive zero-inflation in our share-based outcome measure.

Despite these advantages, monthly aggregation may be too granular to capture policy effects that emerge gradually over time, while counties may still be too heterogeneous internally to fully control for local confounders. However, using smaller geographic units, such as census tracts, could improperly exclude establishments from our outcome construction if they fall near geographic boundaries—posing a risk of artificial discontinuities in our exposure measures that we feel the county-level analysis satisfies.

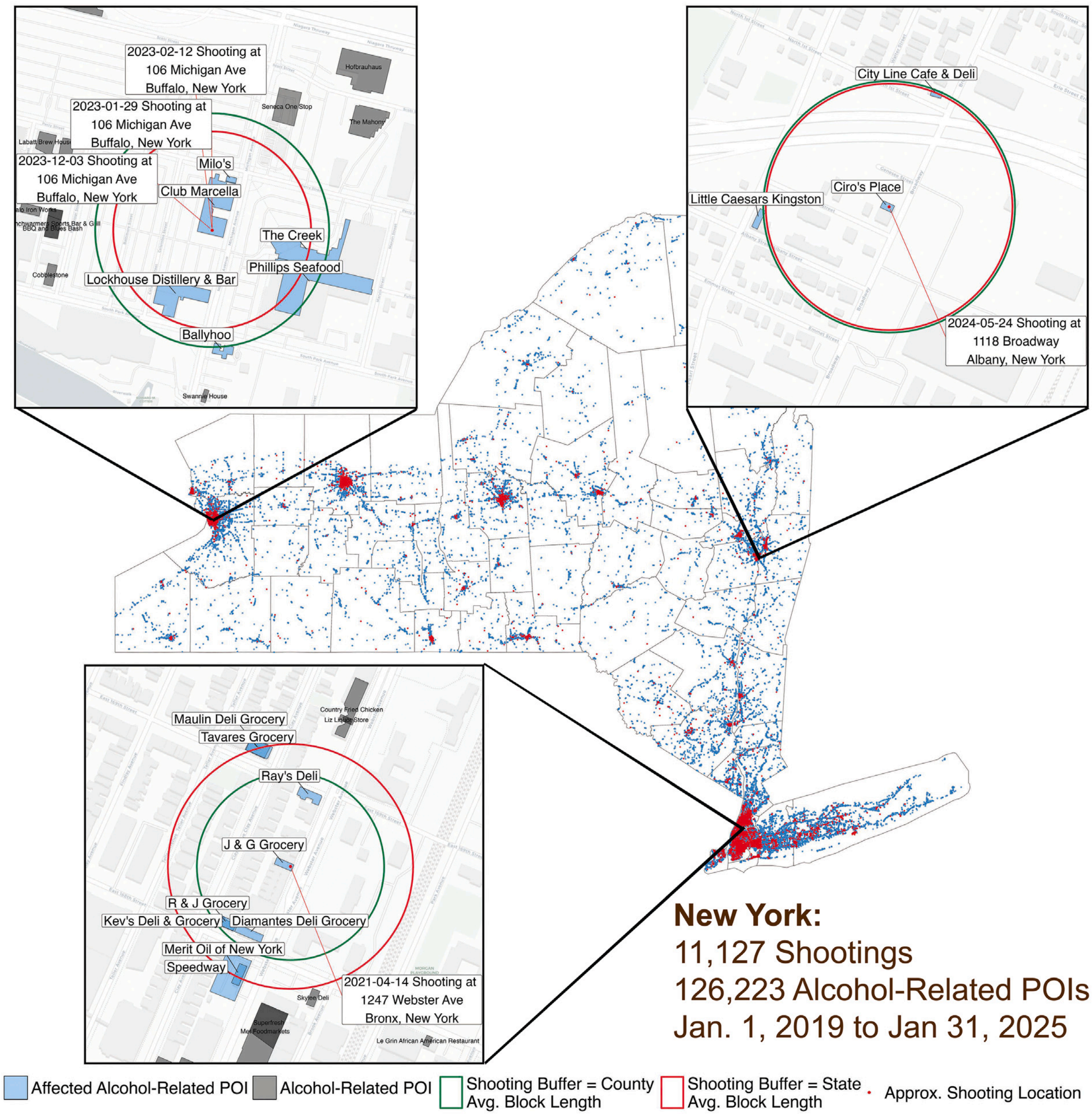

Fig. 1. Identification strategy captures spatial uncertainty around shootings and identifies exposed POIs from active POIs in county during month of shooting: An example inset map for the state of New York.

Note: The inset map of New York state shows all alcohol-related POIs active at any point between Jan. 2019 and Jan. 2025 as blue points, as recorded by Advan data. Red points show the approximate locations of shootings occurring over the same period, as recorded by the Gun Violence Archive. The zoomed maps (using OpenStreetMaps basemaps) show the buffering strategy for shooting locations, and identify POIs that are affected, or not, by the respective shooting(s). Buffers around shootings are defined as either the average block length within each county (green buffers) or the average across the entire state (red buffers). Building footprints from Advan Research, where available, are shown, though our identification strategy places either a 25 or 50 meter radius around the centroid of every POI, as not every POI has an associated footprint polygon. The 25 meter POI-buffer was computed based on the average square footage of alcohol-related POIs where footprint data was available. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

To address these concerns, we conduct a series of robustness checks on our main results using 6-month periods aggregated at the county level, which allows policy effects more time to manifest while smoothing short-term volatility in both shooting incidents and business operations that might obscure underlying treatment effects.

2.3. Outcome measure

For each county-month in our data, we calculate the total number of active POIs (by subcategory) and the total number of shootings. POIs are considered exposed if their buffers intersect with a shooting's buffer. When multiple shootings occur in the same county-month, each POI is counted as exposed only once, even if it intersects with several shootings. If a POI is exposed in multiple months, it is counted as exposed in each of those months, but never counted more than once within a single month.

One concern with this approach is that it treats POIs experiencing multiple shootings the same as those experiencing just one, potentially missing meaningful variation in exposure intensity. To assess whether this mechanism may affect our main results, we also report results of a sensitivity analysis by excluding all multiply-treated POIs from the analysis (Appendix A.8).

After calculating the number of unique exposed POIs, we divide this number by the population of total POIs active in the same county-month to construct the shooting exposure rate for the POI subcategory in each county-month. This construction mitigates concerns of zero-inflation and overdispersion that often arise when working with event count data (Hilbe, 2011).

By focusing on *proportions* of affected POIs rather than raw counts of shooting incidents or binary indicators of exposure, our outcome produces a continuous exposure rate that is more stable across time and space. Additionally, by ensuring that each unique POI can only be counted once per county-month, we reduce risks of artificial inflation of exposure rates that may be introduced due to spatial clustering of incidents. As we aggregate over a subpopulation of POIs that were verifiably open and active during each month, we avoid the inclusion of closed businesses that would be, by definition, structurally zero-risk units.

2.4. Statistical analysis

Following Cengiz et al. (2019), we estimate a stacked DiD specification that pools multiple treatment cohorts (g) with comparison units consisting of counties in never-treated states. This approach addresses potential biases in two-way fixed effects (TWFE) estimators when treatment effects are heterogeneous across cohorts and time (Goodman-Bacon, 2021; Baker et al., 2022; Sun and Abraham, 2021; Callaway and Sant'Anna, 2021). Our primary model estimates the impact of Bar Ban laws on the share of alcohol vendors in a county-month (y_{it}) that are located near a shooting, relative to all alcohol vendors active in that county-month. It is specified as follows:

$$y_{it} = \alpha_{ig} + \gamma_{tg} + \tau_{pg} + X_{it}\beta + \sum_{l=-K}^{-2} \mu_l D_{it}^l + \sum_{l=0}^{K} \mu_l D_{it}^l + \epsilon_{it} \tag{1}$$

Our specification includes a comprehensive fixed effects structure: county-cohort fixed effects (α_{ig}) control for time-invariant unobserved heterogeneity at the county level within each treatment cohort; month-cohort fixed effects (γ_{tg}) account for seasonal patterns and aggregate time trends that differ by cohort; and year-cohort fixed effects (τ_{pg}) control for longer-term temporal variation within treatment groups. Under our primary specification, cohorts are effectively the same as states, though we retain an indexing structure for clarity when we report the results produced by variations of this model where treatment adoption cohorts may consist of multiple states (such as the models estimating the effects of Bar Bans in the 6-month periods following policy adoption, in Section A.6).

The treatment indicators D_{it}^l represent relative time to policy implementation, where the first summation ($l = -K$ to -2) captures pre-treatment leads that serve as placebo tests for the parallel trends assumption, and the second summation ($l = 0$ to K) captures the immediate treatment effect and subsequent dynamic responses. We omit period $l = -1$ as the reference category to avoid perfect multicollinearity. We augment the model with time-varying covariates, X_{it}, that capture observable confounders not absorbed by our fixed effects structure. These include county-month measures of total points of interest in the (sub)category of interest and a county-month count of all shooting incidents, which may correlate with both policy adoption timing and our outcomes of interest, thereby improving precision and addressing potential omitted variable bias. Controlling for these variables removes systematic variation in exposure opportunities and baseline levels of violence that may be unrelated to the adoption of Bar Ban laws, which is intended to reduce residual variance and mitigate potential bias from correlations between these factors and treatment timing.

While state-level factors such as gun ownership prevalence or concurrent firearm policy shifts (e.g., changes in concealed carry regimes) could theoretically introduce time-variant confounding, we intentionally omit these from our primary specification for two reasons. First, proxies for gun ownership are not available at the county-month level, so including state-level annual or monthly proxies would absorb the granular temporal variation our design exploits without improving identification. Second, including broader policy indices can introduce post-treatment bias if Bar Bans are adopted as part of a legislative package. Instead, we rely on our POI placebo tests (such as the Tobacco Store analyses) to detect unmeasured confounders. Because tobacco retailers are subject to similar "vice" regulations and localized violence patterns but are not targeted by Bar Ban laws, stable trends in this category provide evidence that our results are not driven by broader shifts in the state policy environment or retail activity.

The estimated coefficients μ_l represent effects at relative time l. Under treatment effect homogeneity across cohorts, each μ_l provides an unbiased estimate of the average treatment effect at period l. When effects are heterogeneous, the μ_l coefficients represent implicit weighted averages of cohort-specific effects, where weights are determined by each cohort's sample size, the precision of its comparison group, and the availability of clean control units. The stacked design mitigates the negative weighting problems that can arise in standard TWFE estimators with staggered treatment adoption, ensuring that our estimates reflect positively-weighted averages of underlying causal effects.

To estimate the average treatment effect across post-treatment periods, we implement the inverse-variance weighting procedure proposed by Goodman-Bacon (2021). This approach aggregates period-specific effects (β_t) into a single pooled estimate that minimizes variance while assigning greater weight to more precisely estimated periods. The pooled estimator takes the form:

$$\beta_{\text{pooled}} = \frac{\sum_{l=0}^{K} w_t \beta_t}{\sum_{l=0}^{K} w_t} \quad \text{with} \quad w_t = \frac{1}{\mathrm{Var}(\beta_t)} \tag{2}$$

Critically, we account for covariance between period-specific estimates when computing the standard error. Using the full variance–covariance matrix (Σ of the β_t estimates) the standard error is calculated as:

$$\mathrm{SE}(\beta_{\text{pooled}}) = \sqrt{\mathrm{w}'\Sigma\mathrm{w}} \tag{3}$$

This approach provides the minimum-variance linear unbiased estimator under the assumption of normally distributed, period-specific effects and is validated in Appendix Section A.7.

3. Results

3.1. Descriptive results

Table 2 reports descriptive statistics across all states in the US, the 25 control states in our study (those that have never had Bar Ban laws prior to January 31st, 2025), and each of the five states that adopted Bar Ban laws during the period between January 2019 and January 2025. These descriptive patterns reveal considerable variation across the treated states both in terms of the volume of shootings and the number of alcohol-related POIs. For example, New York accounts for over 11,000 shootings and more than 120,000 alcohol-related POIs during the study period, while states like Hawaii and South Dakota exhibit much smaller scales of both exposure and establishment density.

We also find that the gun-related death rates in 2022, sourced from the Centers for Disease Control and Prevention, National Center for Health Statistics (2024), differ across treated states compared to the average across all states and control states, with treated states such as New York, New Jersey, and Hawaii reporting rates well below national averages and reflecting divergent baseline risk environments in which these Bar Ban laws were adopted.

Table 2
Descriptive characteristics of treated and control states, 2019–2025.

	All	Control	Treated states				
			NY	NJ	MD	HI	SD
Counties (N)	3144	1843	62	21	24	5	66
Shootings (N)	263,309	91,067	11,127	3884	7761	258	284
Alcohol-Related POIs (N)	1,623,629	684,665	123,409	49,223	26,368	9473	4275
Monthly Avg. Active drinking places	91,441	35,795	7018	1726	1086	595	393
Monthly Avg. Active restaurants	949,310	404,674	74,259	31,225	16,053	6351	2045
Monthly Avg. Active liquor stores	295,077	119,349	15,499	7301	4580	1005	1204
Monthly Avg. Active tobacco stores	36,872	15,507	1782	807	509	175	63
Age-adjusted gun-related mortality rate per 100,000, 2022	15.8	14.1	5.3	5.0	13.6	4.5	15.7
2020 Census county Pop. (% of All)	327.8 million	41.91%	6.16%	2.84%	1.88%	0.44%	0.27%

Note: N Counties reports the total number of unique counties and county-equivalents per column. N Shootings reports the total number of GVA-recorded incidents. N Alcohol-Related POIs is the total number of unique POIs active for at least one month during the study period, while the Monthly Avg. is the average number of active POIs in each county-month, summed across counties. For the age-adjusted gun-related mortality rate per 100,000, the All column reports the average rate across all states, and the Control column reports the average across untreated states. Control states include AL, CA, CO, CT, DE, GA, ID, IN, IA, KS, ME, MA, MN, MT, NV, NH, OR, PA, RI, UT, VT, VA, WA, WV, and WY.

3.2. Alcohol-related points of interest are consistently proximate to gun violence incidents

Using our geocoded gun violence incident data, we calculated the Euclidean distance from each shooting to the nearest alcohol-related POI within the same state, across each of our five POI NAICS subcategories, though we also consider an aggregate "All Alcohol-Related POIs" category that combines all commercial establishments with direct ties to alcohol, regardless of whether they fall under Bar Ban laws. This broader category provides a baseline measure of overall alcohol-related commercial proximity to gun violence incidents and allows us to contextualize whether the patterns observed in policy-relevant categories (e.g., Covered POIs) are distinctive or simply reflective of the broader alcohol retail landscape. Fig. 2 presents the distribution of these nearest-neighbor distances — restricted to the interquartile range (25th–75th percentiles) — separately for all states, untreated (control) states, and each treated state. Median distances for each group are also reported.

Across more than 263,000 shootings and 1.6 million alcohol-related POIs nationwide, 21% of shootings (57,269) occurred within approximately 100 m of a Covered POI. The median distance between a shooting and the nearest alcohol-related establishment is 222 m (728 ft). For additional context, 222 m translates to approximately a 2-minute and 39-second walk for the average adult (estimating that the average adult walks at approximately 1.4 m per second), suggesting that alcohol-related establishments are routinely colocated with sites of gun violence. We assume that this level of proximity is unlikely to be random — though future work may benefit from testing this assumption — and it is likely that underlying social or economic dynamics may concentrate both alcohol access and gun violence in the same urban areas.

Fig. 2 also demonstrates that there is meaningful cross-state variation in the spatial relationship between alcohol-related POIs and gun violence incidents, and reveals further variation across NAICS subcategories. For instance, the average shooting in the US is nearly 100 m closer to covered POIs than all liquor and convenience stores, with densely populated states such as New York and New Jersey exhibiting medians that are under a 2-minute walking time. Restaurants are typically closer, on average, than Drinking Places. These results underscore that certain types of alcohol-related establishments — particularly convenience stores, liquor stores, and restaurants — are not just spatially proximate to gun violence, but consistently close across diverse geographic and policy environments.

3.3. Null effects of Bar Ban laws, on average

3.3.1. Bar Ban laws did not reduce shooting exposure rates among alcohol-related POIs

Table 3 presents the results of our stacked DiD models estimating the effect of Bar Ban laws on the shooting exposure rate experienced by varying subcategories of alcohol-related POIs. We run four standard models (differing the POI and shooting buffers) across 5 sets of data (varying POI subcategories), and report the average treatment effect (ATE) across the first twelve post-treatment months.

Across all model specifications, the estimated ATEs for alcohol-related POIs (row 1) are small in magnitude and statistically indistinguishable from zero. For instance, the estimated effect in Column 1, Row 1 (25 m POI buffer, county-level shooting buffer) is 0.00005. This coefficient can be interpreted as a 0.005% increase in the proportion of alcohol-related POIs exposed to a shooting in a given county-month.

Most estimated ATEs are statistically insignificant. For "Drinking Places", ATEs are positive and statistically significant only across specifications using the state buffers, though the magnitudes of these effects are under 0.1 percentage points.

Appendix A.8 reveals that multiple shooting exposures within a single month are rare events nationally, affecting approximately 0.05% of POI-month observations for the "All Covered POIs" category across all states during our study period. To assess whether the presence of multiply-treated units affects our main results, we estimated the main model using alternative exposure measures that either exclude multiply-exposed POIs or weight them by intensity of exposure. As shown in Appendix Table A.8.1, the results are virtually identical across all specifications, confirming that our findings are not sensitive to how we handle multiple exposures. This rarity means that a binary exposure measure captures 99.95% of the variation in exposure intensity while maintaining measurement consistency across observations. Moreover, this approach aligns with the policy mechanisms we study and avoids artificial inflation from spatially clustered incidents.

3.3.2. Isolated effects in a few post-treatment months do not drive reported results

The aggregate ATEs reported in the preceding section obscure a handful of statistically significant effects, though these are not consistently observed across the entire post-treatment period. Instead, as shown in Fig. 3, the observed increases in exposure to shootings near alcohol-related POIs are largely concentrated in a small number of months following policy implementation.

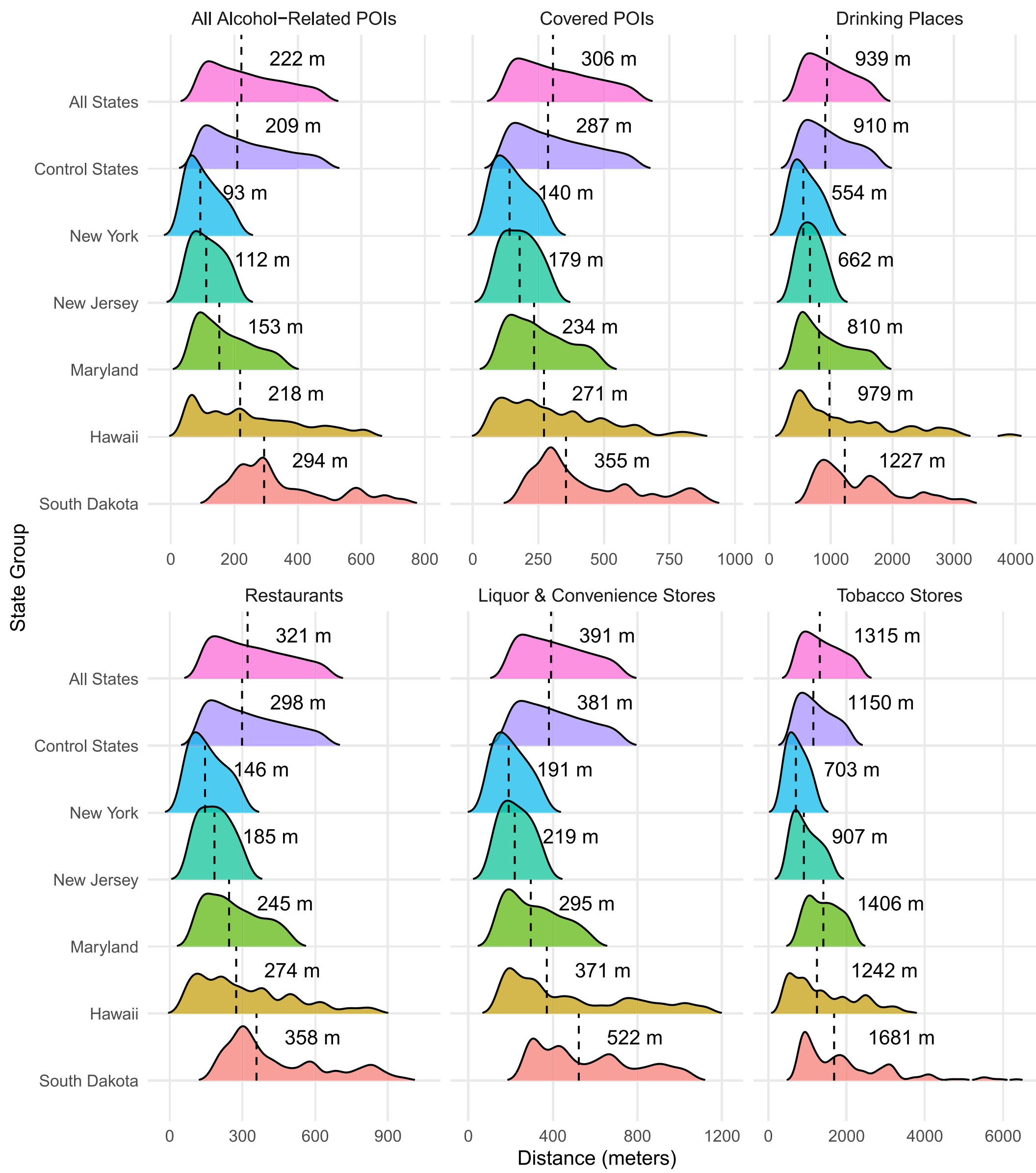

Fig. 2. Distributions of median distances between shootings and POIs, by subcategory, across states.
Note: Ridge plots display the distribution of Euclidean distances (meters) between every shooting and the nearest POI, grouped by state and faceted by POI [sub]category. Distributions are limited to the 25th–75th percentile range to reduce the influence of extreme values. Group-specific medians are labeled for each state/POI grouping, and denoted with dashed vertical lines.

The disaggregated event study estimates clarify that these isolated months are the only points at which treatment effects reach conventional levels of statistical significance. There is no evidence of a consistent pattern of decline in exposure across time, nor of cumulative or compounding effects as Bar Ban laws become more embedded.

The absence of a sustained or systematically increasing effect over the 12-month post-treatment window casts doubt on a straightforward interpretation of these laws as producing meaningful or durable changes in exposure to nearby shootings. Instead, the pattern suggests that any detected effects may be transitory, context-specific, or the result of random variation rather than a reliable consequence of policy implementation.

3.3.3. No evidence of county population density differences biasing estimates

It is possible that our models are biased by systematic differences across counties with varying levels of population density—for example, if more urban counties are disproportionately likely to adopt Bar Ban laws or to have higher baseline exposure to shootings near alcohol-related venues. To test for this, we estimate stratified models by tercile of county population density, allowing for heterogeneous treatment effects across low-, medium-, and high-density counties.

As shown in Figure A.5.1, we find no consistent evidence that treatment effects differ meaningfully by county population density. Across all outcome specifications and density terciles, the estimated effects are small and statistically indistinguishable from zero. These results suggest that our main findings are not driven by the differential composition of counties by population density.

3.3.4. State-level heterogeneity

To determine whether our findings are driven by heterogeneous treatment effects among the treated states (which could reasonably be the case given differences in the way each state's laws were designed), we disaggregate our stacked DiD estimates by treatment group (state)

Table 3
Bar Ban laws have no detectable impact on shooting exposure rate among alcohol-related POIs, on average.

Shooting exposure rate	Average 1-Year post-treatment effect			
	(1)	(2)	(3)	(4)
All Covered POIs	0.00005 [−0.00055; 0.00064]	0.00007 [−0.00054; 0.00067]	0.00067 [−0.00045; 0.00180]	0.00003 [−0.00042; 0.00048]
Drinking Places (Covered POIs)	0.00054 [−0.00016; 0.00123]	0.00056 [−0.00018; 0.00129]	0.00064 [0.00011; 0.00116]	0.00061 [0.00008; 0.00113]
Restaurants (Covered POIs)	0.00005 [−0.00057; 0.00067]	−0.00002 [−0.00064; 0.00060]	0.00020 [−0.00061; 0.00100]	−0.00009 [−0.00061; 0.00044]
Liquor & Convenience stores (Placebo POIs)	−0.00020 [−0.00081; 0.00041]	−0.00010 [−0.00072; 0.00053]	−0.00017 [−0.00081; 0.00047]	−0.00018 [−0.00082; 0.00047]
Tobacco stores (Placebo POIs)	0.00013 [−0.00062; 0.00088]	0.00003 [−0.00081; 0.00088]	0.00045 [0.00004; 0.00086]	0.00037 [−0.00006; 0.00080]
POI buffer (m)	25	50	25	50
Shooting buffer	County	County	State	State
# of Treated counties	178	178	178	178
N	503,554	503,554	503,554	503,554

Note: All models include county-, state-, month-, and year-fixed effects. Robust standard errors are clustered at the county and month level, and 95% Confidence Intervals are reported in brackets. The Average 1-Year Post-Treatment Effect is the average of the ATEs estimated for each of the 12 months following the enactment of Bar Ban laws for treated counties. A coefficient of 0.00003, as seen in the first row for Model (1), can be interpreted as a 0.003% increase in the share of alcohol-related vendors in a county being exposed to a shooting incident in the year after the law's adoption.

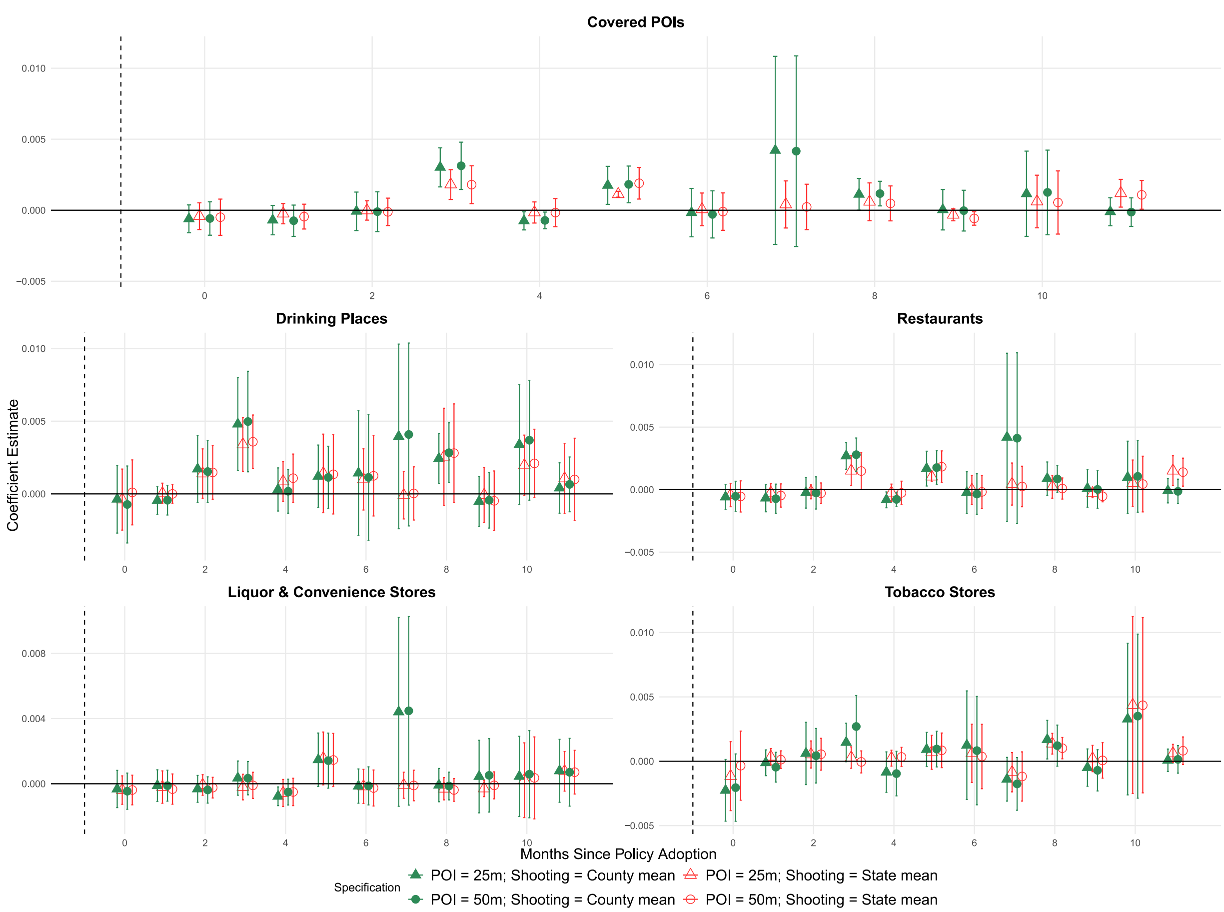

Fig. 3. Event study results show no effect of Bar Ban laws.
Note: Each panel displays coefficient estimates from four stacked DiD models by POI subcategory, varying POI (25 m or 50 m) and shooting (county- or state-level) buffer distances. Estimates reflect the first 12 months post-policy, with 95% confidence intervals. The outcome is the share of active POIs in a county-month exposed to at least one shooting. Exposure is defined by spatial buffer intersection, with each POI counted once per month. All models include county, state, month, and year fixed effects; standard errors are clustered by county and month. Full event-time results are shown in Figure A.4.1 in Appendix A.4.

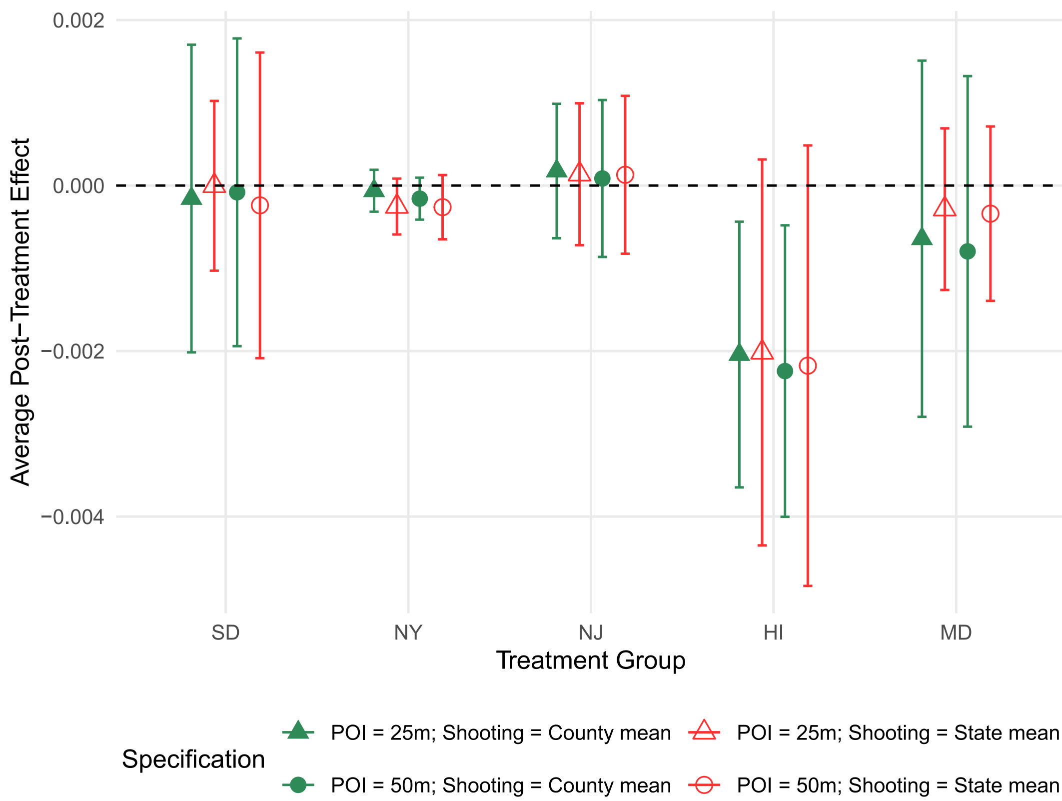

Fig. 4. Disaggregating ATEs by state reveals heterogeneity in effects of Bar Ban laws.
Note: This figure displays coefficient estimates from four stacked DiD models for Covered POIs with varying POI (25 m or 50 m) and shooting (county- or state-level) buffer distances. We run 5 sets of models separately for each state as a unique treatment group. Estimates reflect the average treatment effect over the first twelve post-treatment months, with 95% confidence intervals. All models include county, state, month, and year fixed effects; standard errors are clustered by county and month.

and report the results of each of our four model specifications for models where only one state is included in each treatment group in Fig. 4. This is equivalent to running a canonical two-way fixed effects DiD for each treated state.

Fig. 4 shows no statistically significant reductions in exposure for most states, with exceptions for some models Hawaii. These results suggest a small decrease in the shooting exposure rate for Covered POIs, ranging from an estimated effect between −0.00204 (p-value = 0.01) and −0.00224 (p-value = 0.01).

3.3.5. *Temporal aggregation and seasonality*

One concern with our primary analysis is that by estimating monthly treatment effects over a single post-treatment year, we may fail to capture longer-term or delayed policy impacts. Additionally, short monthly intervals may be vulnerable to confounding from seasonal fluctuations in gun violence and alcohol-related activity (such as summer surges or holiday effects), which could obscure underlying treatment effects if our fixed-effects structure was not performing properly. To address these concerns, we conduct a robustness check using a six-month aggregation strategy, as reported in Section A.6 of the Appendix. Specifically, we redefine the outcome to be the total number of unique POIs exposed to a shooting in a given six-month window, divided by the average number of active POIs in the county during that same period (or the unique count if the number of POIs does not change over time). This approach allows us to (1) detect longer-run or lagged policy effects, and (2) smooth over short-term volatility that may be driven by seasonal patterns or idiosyncratic month-level sources of confounding.

Even under this alternative temporal structure, the results remain consistent: we find no evidence that Bar Ban laws reduce the rate at which alcohol-related POIs are spatially exposed to shootings. Across all POI subcategories, buffer distances, and modeling specifications, the six-month aggregated estimates closely mirror the null findings of the monthly models. This reinforces our core finding that, on average, Bar Ban laws do not appear to reduce shooting exposure risk near alcohol-related POIs—even when allowing for delayed or seasonal dynamics in the policy's effects.

4. Discussion

Our study, which uses a stacked DiD approach and adjusts for differing Euclidian distances surrounding alcohol establishments, finds no substantial policy impacts of state Bar Ban laws on shootings in locations where alcohol is served or bought for off-premise consumption. These results are consistent across dense and less dense counties, and across time and season.

These findings are consistent with the Pear et al. (2023) study of California, which found very small effects, but conflict with the results of the Reeping et al. (2024a) cross-sectional study of Texas, which reported a substantial 37% reduction in shootings within 50 m of gun-prohibiting bars. By using causal models, our study reveals no durable effect of these policies across five states. It may be that the effects observed by Reeping et al. did not persist over time—a limitation the authors acknowledge considering the cross-sectional nature of their study. However, further limitations constrain the validity and generalizability of their findings. The analysis was limited to less than half of all eligible establishments, and the included venues were systematically different from excluded ones (higher revenue, fewer gun-prohibiting signs), introducing potential selection bias. The protective effect of Texas' law also vanished when a 100-meter buffer was used, suggesting the significant 50-meter result may reflect noise or unmeasured confounders specific to the immediate vicinity. However, even the significant results have enormous confidence intervals that indicate uncertainty. In conclusion, while Reeping et al. provide an intriguing associative finding, the results cannot be definitively attributed to the policy itself. By employing causal models, our study finds no durable effect of these policies across five states, suggesting the large reduction observed by Reeping et al. may not be replicable in other state contexts or persistent over time.

The pattern of isolated early effects (Fig. 3) that do not persist could reflect several mechanisms: (1) initial compliance that decays as awareness fades, or initial compliance/enforcement weakens; (2) temporary deterrence effects as communities adjust to new laws before behavioral norms reassert; (3) displacement of incidents to slightly farther locations that later return to baseline; or (4) simply random variation in sparse event data. Distinguishing these mechanisms would require enforcement data, public awareness surveys, and longer post-treatment windows—all valuable directions for future research. Despite some studies indicating a relationship between "lax" alcohol-regulatory states and increased firearm homicide (Naimi et al., 2017), and scoping reviews (Matthay et al., 2025) that also suggest the need for more research in this area, our results are robust to these states and indicate that recent bar-specific gun prohibitions are not a reliably effective intervention, on average.

We note that several studies have addressed the policy question of whether Bar Ban laws increase shootings near places where alcohol is consumed, and our findings confirm that this is not the case, as was largely found in the gun-free zone literature. We note that gun free zone laws are predicated on the idea of rational actors reasonably weighing the costs and benefits of carrying a gun near or in sensitive locations. A potential perpetrator of violence would need to voluntarily comply with the policy after weighing the consequences of violating it. Further, gun free zones are often identified through posted signs that serve as nudges to steer the behavior of individuals by reminding them of the consequences for violating the law, while also signaling the value of the stated law and creating a sense of security among those under the law's protection (Reeping et al., 2024b). Bar Ban laws may work differently, since not all states required posting signs, and many patrons may be unaware of the regulations.

Given our findings of the proximity of gun violence to places that sell and serve alcohol, it is important to consider why such laws may not be effective. The disconnect between the popularity of these laws and their lack of practical results is reflected in their legal design. We identify four characteristics suggesting that Bar Bans are designed for signaling rather than enforcement. First, the burden of compliance rests solely on the patron—owners are not required to frisk entrants or even hang signage in many states. Second, the laws lack proactive enforcement mechanisms, such as requirements for security screenings or routine state monitoring. In the case of the August 2025 shooting at Taste of the City in New York City, reporting by Cramer et al. (2025) revealed that the venue had publicly posted that it forbade weapons on its premises, suggesting that even when establishments take proactive steps to discourage firearms on-site, patrons may still carry weapons, limiting the law's practical impact. Third, penalties vary wildly—South Dakota's statute, for instance, carries no penalty at all. Finally, no state in our study mandates the revocation of firearm permits for a violation. This suggests that Bar Bans may function primarily to reassure a concerned public while avoiding the political and logistical costs of a rigorous, state-led intervention.

Second, it is also true that while the numbers of shootings are high in some of the states we examined, these are densely populated states with restrictive gun laws (excluding South Dakota). Thus, the high number of shootings in these states may not necessarily heighten patrons' perceived risk. Furthermore, alcohol consumption can affect self-control, decision-making, and emotional processing, all of which could threaten the standard approach to rational decision-making, such that patrons may be less worried about shootings after consuming alcohol (Karlsson et al., 2022). In states that have a stronger gun rights policy framework, these results could be different. However, we note that in disaggregating our results, we did not find differences between South Dakota, which has largely permissive gun laws, and the remaining states which have more restrictive gun laws.

Given our findings, it is critical to evaluate why Bar Ban laws appear to have a negligible impact on localized gun violence. The observed null effects likely stem from several distinct, though potentially overlapping, mechanisms. First, the lack of an instrumental effect may be a matter of behavioral non-compliance or insufficient deterrence — as we have noted, the onus for compliance rests entirely on the patron, and the absence of severe penalties or certain enforcement may fail to alter the calculus of a rational actor — especially one whose decision-making is further impaired by alcohol consumption (Karlsson et al., 2022). Second, it is possible that these laws result in spatial displacement rather than prevention, shifting the location of social conflict from the immediate vicinity of the establishment to slightly more distant areas not captured by our primary buffers.

Third, beyond these behavioral and spatial factors, the design of the policies themselves may reflect a broader political phenomenon. One may ask why states continue to pass Bar Ban laws if they consistently fail to produce measurable results. One reason is that policy makers are responding to widespread public support for what appear to be common-sense interventions (Crifasi et al., 2023). Yet, if the primary intent were to substantially curb violence, many of these laws lack the "teeth" associated with high-stakes regulation. They rarely grant bar owners explicit authority to screen or disarm patrons before entry, nor do they create procedures for proactive state-led enforcement.

This absence of enforceable provisions suggests these laws may be primarily "symbolic" in nature—designed to signal a state's moral (or partisan) priorities without incurring the political costs of stricter, more burdensome ownership or enforcement regimes (Edelman, 1985). As we noted in the introduction, this framework applies across the political spectrum: in permissive states like South Dakota, a Bar Ban may be the only politically viable compromise; in states like New York, it reinforces an existing restrictive gun policy regime. In both cases, the law serves an expressive function, reassuring the public that the government is "doing something" about the intersection of alcohol and violence while avoiding the logistical and political friction of true proactive enforcement.

Finally, it could be that Bar Ban laws are indeed effective, but only on an ad-hoc, localized level for a small number of incidents. This would not be captured in our population-level analyses. Moreover, these laws may be effective *after* a shooting event–rather than preventing violence, the presence of a Bar Ban law may aid law enforcement and the judicial system in prosecuting perpetrators who were unlawfully armed. To this end, such laws may represent a retroactive strategy for accountability rather than a preventive public health intervention.

5. Limitations

Evaluating the causal effects of firearm policies presents unique methodological challenges that demand particularly rigorous identification strategies. Unlike other policy domains where randomized controlled trials provide clear casual leverage, gun policy research must typically rely on observational data where treatment assignment is likely endogenous to political, social, or crime-related factors (to name a few) that almost certainly also influence outcomes of interest.

Bar Ban laws are not immune to such confounding — these policies are adopted by states in response to complex political pressures, public safety concerns, and existing patterns of violence — and, thus, exposed to many potential threats to credible causal inference. Furthermore, the high-stakes nature of gun policy debates amplifies the importance of methodological rigor, as policy recommendations based on spurious correlations could have serious public safety consequences. Given these inherent challenges, our analyses prioritize identification strategies that provide the strongest possible foundation for causal claims—even when such approaches may limit statistical power or require more conservative interpretations of results.

Our stacked DiD estimates suggest that Bar Ban laws have no substantive effects on alcohol-related POIs. While Drinking Places exhibit small but statistically significant increases in exposure rates in the year following policy adoption, these effects are negligible in practice: the largest observed increase of 0.064% (Column 3, Row 2 of Table 3) implies, on average, just one additional exposed establishment per month in a county with approximately 1563 bars. Estimates for liquor & convenience stores and restaurants are statistically null, reinforcing the absence of generalizable post-treatment effects that we also observe among our combined category (bars and restaurants) that captures all units likely subject to the laws' restrictions.

Our analysis finds that at the population-level, there is no discernible effect. However, this does not mean that specific incidents may not have been prevented in certain locations. These laws could potentially be used to change the behavior of some individuals. But, based on the evidence, they have likely not done so in a systematic manner. And, while our outcome measure considers a POI exposed once per month as equivalent to a POI exposed multiple times per month, we find that less than 0.05% of POIs are exposed more than once per month (Appendix Section A.8). While examining intensity of exposure at chronic violence hotspots represents a valuable avenue for future research, it requires different methods and research questions than those pursued in this study.

However, there are important limitations to our study. Our conclusions only apply to the 5 states included in the analysis. It is possible that states that had already passed a Bar Ban law (like Texas) prior to 2019 experienced different impacts. Data quality and measurement error are also primary concerns. The GVA relies on secondary reports from multiple sources, which introduces incomplete coverage, reporting biases, or inconsistent definitions of incidents across jurisdictions. This includes under counting of shooting events in less populated areas where media coverage is less complete. We attempt to overcome some of GVA's reporting limitations through the use of an inclusive shooting outcome measure, but this approach may also capture some incidents that are theoretically outside the target scope of Bar Ban laws—our spatial buffers in high-density urban areas may inadvertently capture "in-residence" shootings or suicides occurring in private dwellings near alcohol establishments. Further, we opted against filtering the GVA data by incident type or whether the shooting led to a fatality. This decision attempts to avoid any non-random measurement error inherent in GVA's reporting tags and to maintain a more comprehensive and policy-relevant measure of total firearm incident exposure in these zones. Even after regeocoding the GVA data, positional inaccuracies may bias spatial proximity measures. Similarly, the POI dataset is not a comprehensive administrative record of alcohol-serving or -selling businesses, which may result in misclassification and missingness.

Spatial and temporal uncertainty also constrains inference. Shooting buffers are based on mean block lengths at the county or state level rather than incident-specific precision, potentially misclassifying exposure. POIs within high-density areas may be counted as exposed despite limited meaningful interaction with nearby shootings. "Zones of influence" around POIs are estimated from average footprint data, which may not reflect the true spatial extent of activity, particularly for large or multi-unit venues. County-level aggregation may dilute localized effects, while monthly periods may fail to capture short-term spikes or more gradual policy impacts.

The study period also encompasses societal disruptions caused by the COVID-19 pandemic, which influenced both alcohol consumption patterns and firearm violence trends. Because our treated states adopted laws at different stages of the pandemic — South Dakota in 2019 and the other states in 2022 and 2023 — systematic confounding could occur if pandemic-related shifts in commercial activity or violence coincided uniquely with policy adoption. While our use of month-cohort and year-cohort fixed effects is designed to absorb common temporal shocks, we cannot entirely rule out idiosyncratic local responses to the pandemic. However, our use of off-premise alcohol retailers (liquor and convenience stores) as placebo units provides a critical safeguard, as the absence of significant shifts in these categories suggests that broader pandemic-driven spikes in alcohol purchasing or retail-centered violence are not the primary drivers of our null findings.

Our modeling approach also faces some limitations, including heterogeneity in how states define and enforce Bar Ban laws, which may not be fully captured in our specification. Placebo categories (liquor & convenience stores and tobacco stores) help detect — but cannot fully rule out — spillover effects or broader confounding. Our analysis also assumes relatively sharp policy implementation dates, yet policy awareness and behavioral responses may evolve gradually or in anticipation of adoption. Our analysis of South Dakota is limited by the inability to identify which establishments derive over 50% of income from alcohol sales due to statutory confidentiality protections (SDCL 10-1-28.2). This likely biases our South Dakota estimates toward the null by including establishments not subject to the law. Finally, inverse-variance weighted pooling may overstate precision in cohorts with sparse data, giving disproportionate weight to low-variance estimates.

Together, these limitations underscore the challenges of measuring the causal effects of Bar Ban laws and suggest that observed null or negligible effects should be interpreted in the context of both data and methodological constraints.

6. Conclusions

This study employed a plausibly causal research design to assess the impact of recent Bar Ban laws on the incidence of shootings near places that serve alcohol in 5 states. Results show that while gun-related violence frequently takes place in close proximity to these locations, such laws had no significant effects on reducing gun violence in the 5 states examined. These findings further suggest that "gun-free zone" designations, when not paired with proactive enforcement or establishment-level accountability, are likely insufficient to disrupt the geospatial concentration of violence near alcohol outlets.

Future work is needed to assess how to best reduce the geospatial concentration and temporal persistence of violent events near places that sell and serve alcohol. Until then, Bar Ban laws may remain more successful as common sense interventions that appeal to populations across the political spectrum rather than evidence-based approaches to violence prevention.

CRediT authorship contribution statement

Jack Kappelman: Writing – review & editing, Writing – original draft, Visualization, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Diana Silver:** Writing – review & editing, Writing – original draft, Validation, Supervision, Resources, Project administration, Methodology, Investigation, Funding acquisition, Conceptualization. **Jin Yung Bae:** Writing – review & editing, Methodology, Investigation, Data curation. **Kevin Butler:** Writing – review & editing, Validation, Software, Resources, Formal analysis, Data curation. **Tanvi Shinkre:** Writing – review & editing, Visualization, Methodology, Investigation, Formal analysis. **Falco J. Bargagli Stoffi:** Writing – review & editing, Writing – original draft, Visualization, Validation, Methodology, Investigation, Formal analysis, Conceptualization. **James Macinko:** Writing – review & editing, Writing – original draft, Validation, Supervision, Resources, Project administration, Methodology, Investigation, Funding acquisition, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.socscimed.2026.119304.

Data availability

The authors do not have permission to share data.

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