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Reducing Alcohol-Related Violence: A Field Experiment with Bartenders*

Andrés Ham†, Darío Maldonado, Michael Weintraub, Andrés Felipe Camacho, and Daniela Gualtero

Abstract

This paper studies whether bartenders that adopt standardized practices can promote responsible alcohol consumption and subsequently reduce alcohol-attributable violence. We conduct a randomized experiment in four localities of Bogotá in cooperation with Colombia's largest brewery and Bogotá's Secretariat of Security, Coexistence, and Justice. Our design allows estimating direct and spillover effects on reported incidents within and around bars. Results show that bartenders in treatment locations sell more water and food, thus contributing to more responsible alcohol consumption by patrons. We find no direct or spillover effects of these changes in consumption on brawls, but some improvement on other alcohol-related incidents.

Keywords: alcohol, bartenders, brawls, alcohol-related violence, crime.
JEL Classification: C93, D90, I10, K42, O12.

Resumen

Este trabajo estudia el rol que tienen los tenderos de barrio para promover el consumo responsable de alcohol y analiza si el consumo responsable reduce las riñas dentro y alrededor de tiendas. Este estudio presenta la evaluación experimental del programa "Buenos Tragos" en cuatro localidades de Bogotá. El diseño experimental permite estimar efectos directos e indirectos sobre reportes de riñas y otros incidentes dentro y alrededor de las tiendas de barrio. Los resultados indican que el programa Buenos Tragos hace que los tenderos vendan más agua y comida, por lo cual sí logran fomentar el consumo responsable entre sus clientes. Sin embargo, no encontramos efectos directos o indirectos del programa sobre riñas, pero si reducciones en otros incidentes relacionados con el alcohol como embriaguez y alteración del orden público.

Palabras clave: alcohol, tenderos, riñas, violencia relacionada al alcohol, crimen.
Clasificación JEL: C93, D90, I10, K42, O12.

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I Introduction

Excessive alcohol consumption is prevalent in many countries worldwide and is generally associated with negative consequences for individual and societal well-being. Heavy drinking is related to liver cirrhosis, hypertension and stroke, cancers of the mouth, pharynx, esophagus and liver, and the proliferation of infectious disease (Room et al., 2002), psychological disorders including suicide and depression (Sher, 2005), as well as criminal activity (Gerson and Preston, 1979; Zhang et al., 1997; Markowitz, 2005). A recent meta-analysis found that 3 million deaths in 2016 were directly related to alcohol consumption (Griswold et al., 2018), not including those caused by alcohol-attributable violence. Global alcohol-related violence is estimated to account for 248,000 annual deaths (Graham and Livingston, 2011), and observational studies frequently find alcohol consumption to be positively correlated with violent acts such as aggravated assault and domestic violence (Rossow, 2001; Card and Dahl, 2011). However, causal evidence of the relationship between alcohol consumption and alcohol-related violence is crucial, especially for policymakers.

This paper provides new evidence on the relationship between alcohol consumption and alcohol-attributable violence for the city of Bogotá, Colombia. Alcohol-related violence is widespread in the city and has become an ongoing policy concern for both the private and public sector. Incidents such as aggravated assault or brawls are commonly reported within and around bars. At least 62% of these brawls often result in more serious crimes, such as personal injuries, robberies, and homicides (SSCJ, 2018). However, finding a source of exogenous variation in alcohol consumption to establish a causal link with alcohol-related violence is empirically challenging.

Our approach considers an indirect pathway. We hypothesize that if bartenders adopt standardized practices, then their patrons may consume alcohol more responsibly, which could subsequently result in lower levels of reported brawls and other alcohol-related incidents within and around bars. If bartender actions change alcohol consumption practices among patrons, the presence or absence of an effect of these practices on alcohol-attributable violence will provide valuable information on the causal relationship between alcohol consumption and alcohol-related violence. The set of practices we consider draw from medical evidence which shows that responsible consumption, such as consuming water and food while drinking, reduces blood-alcohol content in consumers and therefore delays rapid inebriation (Swift, 2003; Parrott and Eckhardt, 2018).

We trace out this indirect causal pathway by evaluating the "Good Bartenders"[1] program, which was designed and implemented by a public-private partnership between Fundación Bavaria (FB), the social responsibility unit of Colombia's largest brewery, and the Secretariat of Security, Coexistence, and Justice of Bogotá (SSCJ). This program provides materials and training to bartenders and offers food and non-alcoholic drinks to patrons. The intervention was designed to achieve two objectives: i) provide bartenders with standardized practices that promote responsible alcohol consumption among their patrons, and ii) give information and strategies to bartenders on how to defuse conflicts that may result in alcohol-related incidents within and around their bars.

We test the effectiveness of the Good Bartenders program via a randomized experiment in four localities of Bogotá.[2] In parallel with the public-private partnership that designed and implemented the intervention, we designed the randomized procedure that allows estimating its effects. While the natural unit of analysis for this experiment seems to be the bar, we study what happens at the level of street segments: a road that lies between two intersections. This decision responds to results of ethnographic fieldwork in bars conducted before the intervention (Córdoba, 2018), the fact that multiple bars are often located on a single street, and precedent in studies that employ the same strategy (Blattman et al., 2017). Our experimental design avoids contamination issues prevalent in location-based interventions and overcomes challenges found in dense urban environments like Bogotá. The two-stage design allows estimating the direct and indirect effects of the Good Bartenders program on reported incidents within and around bars. The procedure creates three groups from a sample of 5,987 street segments: 228 *directly treated* street segments, 2,730 *indirectly treated* street segments, and 3,029 *control* street segments. We first study whether the intervention led to any changes in bartender practices and patron outcomes using survey data for bars in directly treated and control areas. We then estimate the direct and indirect effects of the program on brawls and other alcohol-related incidents using a georeferenced administrative panel on reported violent events that are merged to bar locations in our sample.

Results indicate that the Good Bartenders program changes bartender practices and promotes more responsible alcohol consumption. Bartenders sold more food and water after the intervention. While alcohol sales were unchanged, bartenders in the treatment group sold more water (56% increase) and food (67% increase). Estimated effects are larger when we adjust for compliance rates among treated bars. These

[1]The program is called *"Buenos Tragos"* in Spanish.

[2]Localities are the main administrative division in Bogotá. Each locality has a local authority which is selected by the city's mayor and makes decisions on public spending. There are 19 localities in Bogotá (18 urban and 1 rural).

findings are driven by intensive margin adjustments: bars that already sold food and water increased their sales. We also inquire about perceptions of alcohol-related violence within and around the bar. While the estimated coefficients suggest that bartenders perceive fewer brawls in their vicinity, the effects are statistically insignificant. These perceptions are in line with reports from georeferenced administrative data, since we find no direct or spillover effects of the Good Bartenders program on violent brawls using different model specifications, treatment definitions, and estimation procedures. Given the statistical power of the experiment and the precision of the estimates, we are confident the program has zero treatment effects on brawls.

We do find some direct effects on alcohol-related incidents related to coexistence outcomes. Reports of drunk and disorderly behavior fall by 22% and disturbances to the peace fall by 11%, with no significant spillovers to indirectly treated areas. These significant findings are robust to randomization inference, multiple hypothesis adjustments, are validated by placebo regressions that assume changes in treatment timing, and falsification tests on outcomes beyond the scope of the program. Overall, the Good Bartenders program did encourage bartenders to promote responsible alcohol consumption among patrons. However, these changes did not result in lower reports of brawls within and around bars, but do lead to some improvement on other alcohol-related incidents.

This paper provides new evidence on the relationship between alcohol consumption and alcohol-related violence. Tracing out this relationship is useful because these violent events often lead to more serious crimes, particularly in the case of brawls. At the time of writing, we are unaware of similar programs with the same objective, scale, and form of delivery. The experience of the Good Bartenders program provides a better understanding of three important aspects related to alcohol consumption and alcohol-attributable violence: i) the role of bartenders, ii) the experience of using less restrictive interventions for alcohol, and iii) the value of public-private partnerships for policy. We discuss each in turn.

While previous evidence has studied commitment devices for consumers to moderate heavy drinking (Schilbach, 2019), most of the literature tends to overlook the role of bartenders to achieve the same objective. Prevention research argues that changing social norms and preventing addictive behavior need not focus solely on the individual but should also rely on the community (Aguirre-Molina and Gorman, 1996; Holder, 2000; Hawkins et al., 2002). Our study contributes a better understanding of how to harness the potential of bartenders as community leaders in order to mitigate some of the negative consequences often associated with excessive alcohol consumption. In the Colombian context, bartenders operate similarly to community

leaders because they perform social control given that customers are usually neighbors or regulars (Córdoba, 2018). The results in this paper suggest that bartenders, as members of the community, have an important and complementary role to play in shaping alcohol consumption patterns among the population.

Most alcohol-related policies are traditionally restrictive (Nicholls, 2016).[3] This approach has yielded some positive results, but infringes upon individual liberties and generates unintended consequences (Cook, 2007; Adinoff, 2016; Fernandez et al., 2018). While previous evidence indicates that restrictive policies often result in short-term gains, these approaches do not address the underlying issues that cause individuals to abuse substances such as alcohol. Our findings suggest that less restrictive efforts are not the sole answer to reduce the consequences of excessive alcohol consumption, but may be part of the solution. Multiple efforts are likely necessary to reduce the immediate and visible consequences of heavy drinking while also attacking its root causes.

Partnerships between the state and the private sector, as in the Good Bartenders program, are essential to design, implement, and evaluate policies beyond alcohol regulation. While the effectiveness of these partnerships have mixed experiences in different areas (Fabre and Straub, 2019), the Good Bartenders program we study here would not have been feasible without this collaboration. Our experience suggests that promoting cooperation between governments, business, and academics may open new possibilities to develop wider-ranging and more effective policies.

The remainder of this paper is organized as follows. Section II provides background on alcohol-related violence and alcohol consumption in Bogotá, and their relationship. Section III describes the Good Bartenders program and the design of our field experiment to evaluate its effects. Section IV presents the data sources and empirical strategy that we employ to estimate the impact of the Good Bartenders program. Section V presents and discusses our results. The last section concludes.

II Violence and Alcohol Consumption in Bogotá

Latin America is the world's most dangerous region and Colombia remains among its most violent countries (UNODC, 2019). National and local authorities in Colombia have made significant progress in addressing lethal violence, especially in urban areas. The homicide rate in Bogotá reached its lowest level in 40

[3]Governments have levied higher taxes on alcohol, modified the drinking age, imposed curfews on alcohol-serving establishments, cracked down on public consumption through "open container" laws, limited alcohol availability by providing fewer liquor licenses, led campaigns to inform on the dangers of over-consumption, and encouraged responsible drinking (Pridemore and Snowden, 2009; Lovenheim and Steefel, 2011; Heaton, 2012; Markowitz et al., 2012; Grönqvist and Niknami, 2014; Hansen, 2015; Marcus and Siedler, 2015; Luca et al., 2015; Anderson et al., 2018).

years during 2018, at 12.7 per 100,000 people, a 43% reduction compared to 2012. Homicide rates in Bogotá are now the lowest among major Colombian cities, and tend to be geographically concentrated in economically and socially marginalized neighborhoods (Blattman et al., 2017). Other forms of violence, however, have not shown similar improvements. Brawls, personal injuries, and robberies in Bogotá have not fallen and, unlike homicides, these crimes tend to occur all over the city, creating generalized risk and perceptions of insecurity among the city's more than seven million residents.

One of the main concerns for the SSCJ in Bogotá has been the growth in alcohol-attributable violence: brawls and other alcohol-related incidents. Data on reported brawls are collected through the emergency services (NUSE 123, for its acronym in Spanish) and are defined almost identically to aggravated assault in the United States. The FBI's Uniform Crime Reporting (UCR) Program defines aggravated assault as "an unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily injury." Brawls in Colombia are considered "*any incident or altercation that may arise between two or more people causing physical aggression, which may even lead to endangering someone's life*" (SSCJ, 2016).

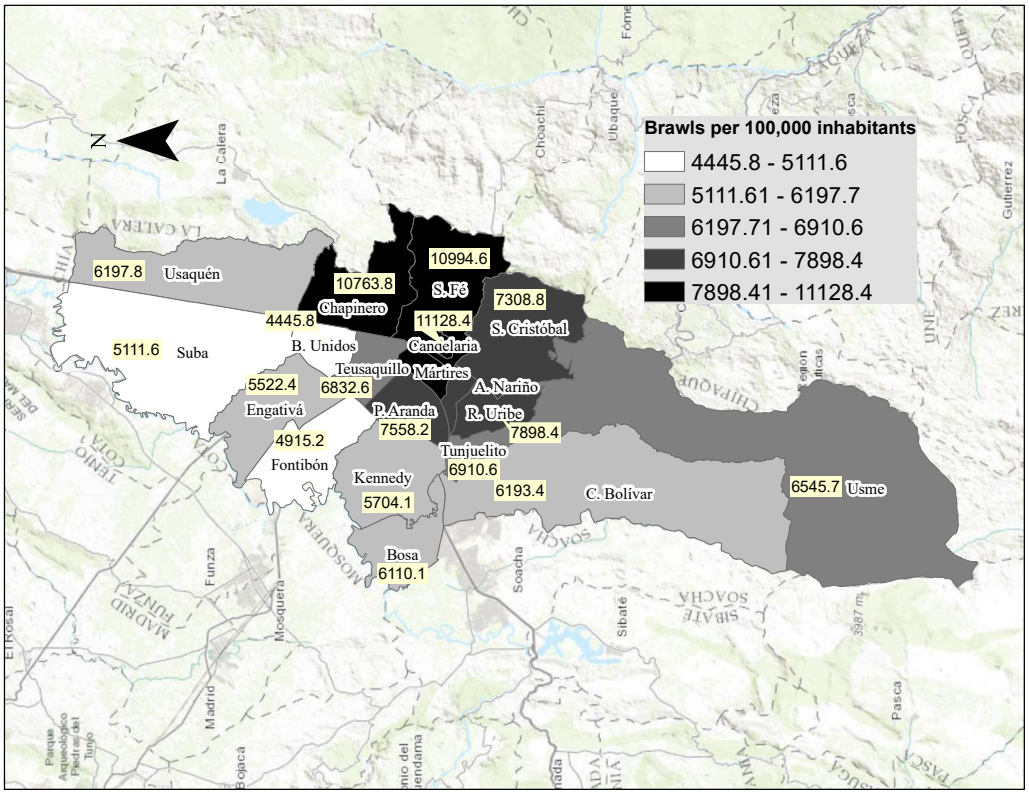

FIGURE 1. SPATIAL DISTRIBUTION OF REPORTED BRAWLS IN BOGOTÁ

Source: Authors' elaboration from georeferenced administrative data.

Figure 1 maps reported brawls per 100,000 people across the 18 urban localities in Bogotá during 2017. The figure shows high rates of reported assaults across the city, as well as considerable heterogeneity across localities. The aggravated assault rate for metropolitan areas in the United States was 248.9 per 100,000 people in 2017 (United States Department of Justice, 2017). Reports of physical violence in Bogotá for the same year are ten times higher than cities like Chicago (570.4 per 100,000 people) and New York (345.5 per 100,000 people). Using historic data on reported brawls from the SSCJ, we characterize the number of brawls, the day of the week they are reported, and the time citizens usually report brawls to better understand the nature of brawls.

Figure 2 presents citywide statistics for brawls in Bogotá. Reported brawls have remained stable over the selected period, both at monthly (Panel A) and weekly frequencies (Panel B). These trends also reveal cyclical behavior. Brawls are more common in March, May, and December, which coincide with three celebrations: Easter, Mother's Day, and the end of year holidays (Christmas and New Year's). Analyzing data for 2017, we also identify what days and hours brawls are most often reported. The figure shows that brawls tend to occur on weekends (Panel C), with a monotonic increase that begins on Friday and ends on Sunday. Brawls are most commonly reported early in the evening, between the hours of 8 PM and 11 PM.

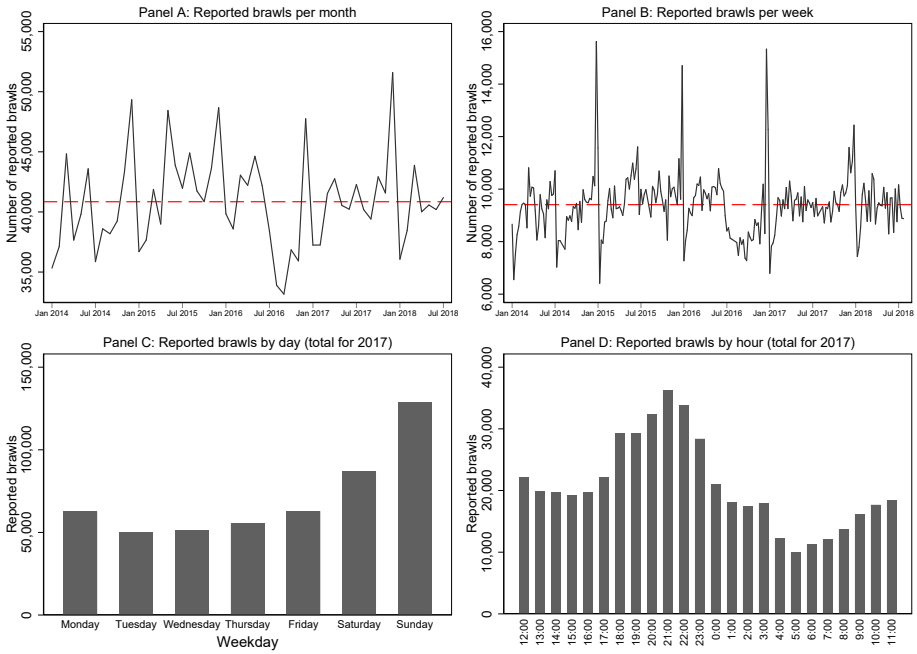

FIGURE 2. ATTRIBUTES OF REPORTED BRAWLS IN BOGOTÁ

Source: Authors' elaboration from georeferenced administrative data.
Notes: The top panels (A and B) present trends from January 2014 to July 2018. The bottom panels (C and D) present aggregate statistics for 2017.

These statistics suggest that brawls in Bogotá follow certain systematic patterns. They often occur during times of celebration, on weekends, and early in the evening. The intersection of these attributes has one common denominator: alcohol consumption. As reported by the 2014 National Study on Consumption of Psychoactive Substances in Colombia, the rate of alcohol consumption in Bogotá is above the national average. According to data from 2014, there are approximately 2.1 million consumers of alcoholic beverages in Bogotá (Ministry of Justice and Law et al., 2014). In terms of age, people between 18 and 24 years old have the highest rate of alcohol consumption (50.7%), followed 25-34 year olds (45.9%). There is also significant heterogeneity in alcohol consumption prevalence by locality (Bogotá Mayor's Office et al., 2016).[4]

There are three types of alcohol-serving establishments in Bogotá, which we refer to as neighborhood bars, commercial bars, and clubs. Neighborhood bars are small establishments with tables and chairs that sell alcohol between 10 AM and 11 PM. They often sell other products besides alcohol, including cleaning products, fresh produce, and food. Neighborhood bars are essentially corner shops, but with a liquor license and seating. Commercial bars are larger venues that mainly sell alcohol and can remain open until 3 AM, while clubs operate similarly but some may remain open until 5 AM. While some of the latter two establishments sell food on their premises, most do not. Residents of Bogotá generally begin their nights out at neighborhood bars and then transition towards commercial bars or clubs around the time the neighborhood bar closes at 11 PM.

Our study focuses on violence within and around neighborhood bars for two reasons. First, Figure 2 shows that brawls often occur between 8 PM and 11 PM, which coincides with times when neighborhood bars are open and serving alcoholic beverages. Second, commercial bars and clubs tend to cluster in certain areas, which may experience different dynamics of violence (Francesconi and James, 2019). Land use laws in Bogotá restrict the presence of commercial bars and clubs in residential neighborhoods but do not regulate neighborhood bars in the same manner. In order to capture a wider picture of the relationship between alcohol consumption and alcohol-related violence, neighborhood bars are better suited than the other types of establishments in Bogotá.

Brawls and alcohol tend to show significant spatial correlation in empirical data (Snowden, 2018), and Bogotá is no exception. Figure 3 maps the density of neighborhood bars and reported brawls in the city from 2014-2017. There is considerable overlap between areas that report more violent incidents and the availability

[4]For instance, localities with the highest prevalence of alcohol consumption in the city are Suba (46.5%), Chapinero (43.8%) and Usaquén (42.5%), followed by the joint zone of Santa Fé, Los Mártires, and Candelaria (34.9%).

of neighborhood bars. While the figure captures correlation between alcohol availability and brawls, the remainder of our study explores their causal link.

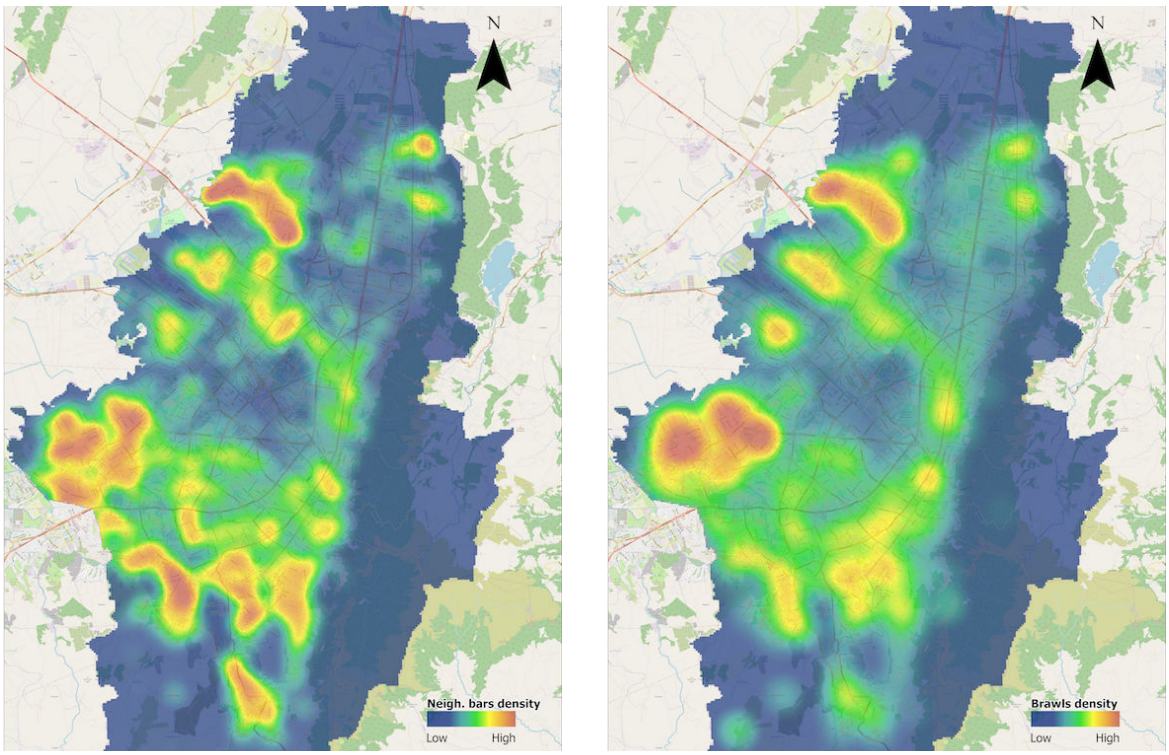

FIGURE 3. ALCOHOL AVAILABILITY AND REPORTED BRAWL DENSITY IN BOGOTÁ

Source: Authors' elaboration from georeferenced administrative data from January 2014 to July 2018.

In response to this quantitative diagnosis of alcohol-related brawls and their patterns, our implementation partners commissioned an ethnographic study to further explore the relationship between alcohol consumption and alcohol-related violence in four localities: Suba, Engativá, Fontibón, and Los Mártires. These localities were selected because they account for more than a quarter of all reported brawls in the city (see Figure A.1 in the Appendix), are not among the most violent areas where brawls may be caused by other factors besides alcohol (e.g. gangs, drugs, and/or lethal violence), and are home to 2.6 million people (about one third of the city's total population).

Ethnographic fieldwork was carried out between December 2017 and January 2018 (Córdoba, 2018). These data provided evidence on the causes of alcohol-attributable violence and how bartenders deal with such incidents in their establishment. Researchers visited neighborhood bars as hidden clients to observe bartender and patron behavior, and then identified themselves to bartenders to conduct semi-structured interviews. The study identified several causes of brawls: intolerance due to excessive alcohol consumption, consumption of drugs with alcohol, incidents of jealousy and *machismo* between patrons, and "tribal" behavior between

regulars and strangers. Bartenders in general felt unequipped to deal with alcohol-related violence in their establishments, due to the lack of standardized practices and lack of information. Most bartenders use ad hoc reactive strategies: denying entry to certain patrons, using price discrimination to drive out unwanted customers, installing security systems, and/or hiring private security firms. Bartenders were also generally unaware of how involve the police when incidents occur within or around their bars.

Bartenders also revealed that most of their clients were regulars, usually neighbors. Several interviewees mentioned that if regulars were drinking too much, they would stop serving them or encourage them to go home. However, bartenders are more skeptical about unfamiliar customers, and thus less likely to implement these strategies instead of price discrimination or refusing entry. Therefore, bartenders in neighborhood bars in Bogotá are similar to community leaders because they often perform social control and can impact their patrons' behavior. Most alcohol-related interventions often focus on consumers (Schilbach, 2019), but this evidence suggests that bartenders may play a significant role to help reduce the negative consequences of alcohol consumption.

While lethal violence in Bogotá has fallen in recent years, alcohol-attributable violence has not, especially aggravated assault or brawls. This section provides evidence that such incidents are widespread across the city and tend to be more prevalent in some localities than others. Certain attributes also stand out, since brawls tend to be reported during times of celebration, on weekends, and early in the nighttime. Alcohol consumption in the city usually begins in neighborhood bars, which implies that neighborhood bartenders may play a key role in how the rest of the night unfolds. Given the high levels of alcohol consumption in Bogotá, the correlation between alcohol availability and reported alcohol-related incidents, and the qualitative findings from the ethnographic study, we evaluate a randomized program directed at bartenders in order to estimate the causal relationship between alcohol consumption and alcohol-related brawls and other incidents.

III Experimental Setting

We carried out a field experiment in the four localities selected for the ethnographic study: Suba, Engativá, Fontibón, and Los Mártires (see Figure 1). The main objectives of the intervention are to test whether bartenders that adopt standardized practices in their establishments can promote more responsible alcohol consumption by their patrons and if these practices subsequently result in lower levels of alcohol-attributable brawls and other incidents within and around bars. If bartenders' actions can change alcohol consumption

practices among patrons, the presence or absence of an effect of these practices on alcohol-related incidents will provide information on the causal relationship between alcohol consumption and alcohol-related violence. We first present the Good Bartenders program and then proceed to describe its randomized implementation in the field.

A The Good Bartenders program

Most alcohol-related interventions tend to focus on the final consumer (Schilbach, 2019). We test the role of bartenders to promote responsible drinking which may in turn, reduce alcohol-attributable violence. Medical evidence has shown that responsible alcohol consumption, such as consuming water and food, reduces blood-alcohol levels (Swift, 2003; Parrott and Eckhardt, 2018). In this sense, our intervention frames bartenders in a similar light as community leaders, since neighborhood bars in Bogotá are often located in residential areas and their owners and patrons usually live nearby. Moreover, the mixed-methods analysis summarized in Section II indicates that brawls and other alcohol-related incidents occur at the time when neighborhood bars are open. Our study aims to understand how bartenders could potentially mitigate the negative consequences associated with heavy drinking upon receiving an intervention that motivates responsible alcohol consumption.

The "Good Bartenders" program was designed and implemented by the Secretariat of Security, Coexistence, and Justice and Fundación Bavaria, with technical support provided throughout by the research team. The program provides didactic materials and training to bartenders and offers food and non-alcoholic drinks to patrons. These activities have two primary objectives: i) provide bartenders with standardized practices that promote responsible alcohol consumption among patrons, and ii) give information and strategies to bartenders on how to diffuse conflicts that may escalate to produce alcohol-related incidents within and around neighborhood bars.

The intervention consists of four steps. First, initial contact is made with bars selected into treatment locations. During a first visit, the implementation team introduces the general idea of the program and asks the bartender whether they would like to participate. If the bartender refuses, no further time is taken from these individuals and the team proceeds to visit the next bar selected into treatment. We selected more treatment locations than required to ensure an adequate sample size.

Second, the team hands out and explains the Good Bartenders manual to participating bartenders. This manual provides information on what practices are recommended to avoid brawls and other alcohol-related incidents due to patrons' heavy drinking. On the one hand, the manual suggests that bartenders offer their patrons food and water to control the rapid over-consumption of alcohol. It also recommends that bartenders not promote mixing different kinds of alcohol, encourage dancing, designate a sober friend to look out for others, and promote a calm environment among patrons. On the other hand, the manual proceeds to classify common types of "drunk customers" and how best to approach each kind of person to defuse any potential conflict. The manual ends by reminding bartenders that these practices can help maintain a respectful environment where violence has no place and provides the suggested procedures and contact information for the police. Selected pages of the manual (in Spanish) are shown in Appendix Figure A.2.

After introducing and explaining the Good Bartenders manual, the team also provides a branded kit to participating bars that includes a custom-made welcome mat with the words: "Welcome to an establishment that promotes *Good Drinks*" and the logo of the intervention; a sticker with the slogan "Enjoy calmly – this is an establishment where the night always ends well"; a clock that reminds patrons that 11 PM is last call; and a water pitcher that contains the logo of the intervention and the slogan "If you feel things are burning up [slang for getting drunk], extinguish with water." Pictures of these materials (in Spanish) are shown in Figure A.3 in the Appendix.

Third, on a Thursday, Friday or Saturday night, between the hours of 6 PM and 11 PM, team members arrive at treated bars and "activate" them. They first offer a round of food to all patrons who are consuming alcohol. Meals are standard pub fare in Colombia, including sausages and chips. The team then ensures that customers have access to water, served in pitchers branded with the logo of the intervention. Team members also remind bartenders how to implement the suggested practices in the Good Bartenders manual and provide coasters with the logo of the intervention for use by patrons. A sticker is placed on the wall of the bar that indicates that the bar is participating in the program. Finally, the bartender is told that in the coming weeks a "mystery shopper" and a survey firm will visit the bar to conduct follow up visits.

The fourth and final step is the follow-up and monitoring phase. A mystery shopper visits each treated neighborhood bar to evaluate the extent of compliance with the intervention. Seven criteria are evaluated and the bartender is asked a question on the material from the Good Bartenders manual.[5] The mystery

[5]The seven criteria include: i) having the sticker for participating in the program in a visible location; ii) placing in a prominent location the clock that reminds patrons of last call; iii) locating the welcome mat at the bar's entrance; iv) placing a provided picture

shopper records this information and gives the bartender raffle tickets to win a jukebox according to the level of compliance with the program.[6] A survey firm also visits bars to collect post-treatment data, which are described in detail in the next section.

The intervention was designed during the first semester of 2018. The team chose not to implement over the summer to avoid any potential confounding effects of the 2018 FIFA World Cup, since this event generally coincides with increased alcohol marketing and consumption (Collin and MacKenzie, 2006). Bartenders were first contacted in July and participating bars started receiving treatment in August. Given the scope of the intervention and the size of Bogotá, the intervention was rolled out on a weekly basis. Treatment began in the first week of August and concluded in all participating bars in late September (See Appendix Table A.1). Mystery shopper visits were carried out throughout October and the survey firm collected data during November.

B Randomization

While the natural units of analysis for our field experiment are bars, we focus instead on street segments to avoid contamination issues, since the qualitative evidence shows that alcohol-related brawls in Bogotá often move from bars into streets and because there tends to be more than one bar per street. Streets have been used previously as the unit of analysis in Bogotá to study the effects of hotspot policing on crime (Blattman et al., 2017). Within the four selected localities, we identified eligible street segments: those that do not belong to the most dangerous quadrants in the city (in terms of homicide rates), those with at least one reported brawl during the pre-intervention period (January 2014 to July 2018), and one bar at a distance equal to or less than 100 meters.

Randomized assignment was carried out in two stages, both stratified by locality. In the first stage, we randomly select treatment and control police quadrants. Bogotá is composed of 1,051 police quadrants across 18 urban localities. Quadrants were selected in two phases. First, quadrants with at least two pre-selected street segments (those with at least one reported brawl in the pre-intervention period and one bar at a distance equal to or less than 100 meters) were chosen. Second, using the Statistical, Criminal, Contravention, and

frame with the intervention logo on the wall; v) having and using the provided water pitcher; vi) using the provided coasters; and vii) offering patrons who are consuming alcohol food and/or water. The factual question was randomly selected from a pre-selected bank of quiz questions.

[6]All treated bars were given one ticket for the jukebox raffle upon their acceptance to participate. Up to 7 additional tickets were handed out during the follow-up mystery shopper visits, depending on whether the bartender fulfills some or all of the seven criteria. One jukebox per locality, four in total, were distributed to the winners in December 2018.

Operational Information System (SIEDCO, for its acronym in Spanish) from the National Police, we discarded the most dangerous decile of quadrants in the city (in terms of homicide rates) because brawls in the most violent areas in Bogotá may be explained by other factors besides alcohol consumption.[7] From a universe of 271 police quadrants in the four targeted localities, 221 quadrants were eligible under these criteria. The assignment procedure classified these 221 locations into 109 treatment and 112 control quadrants.[8]

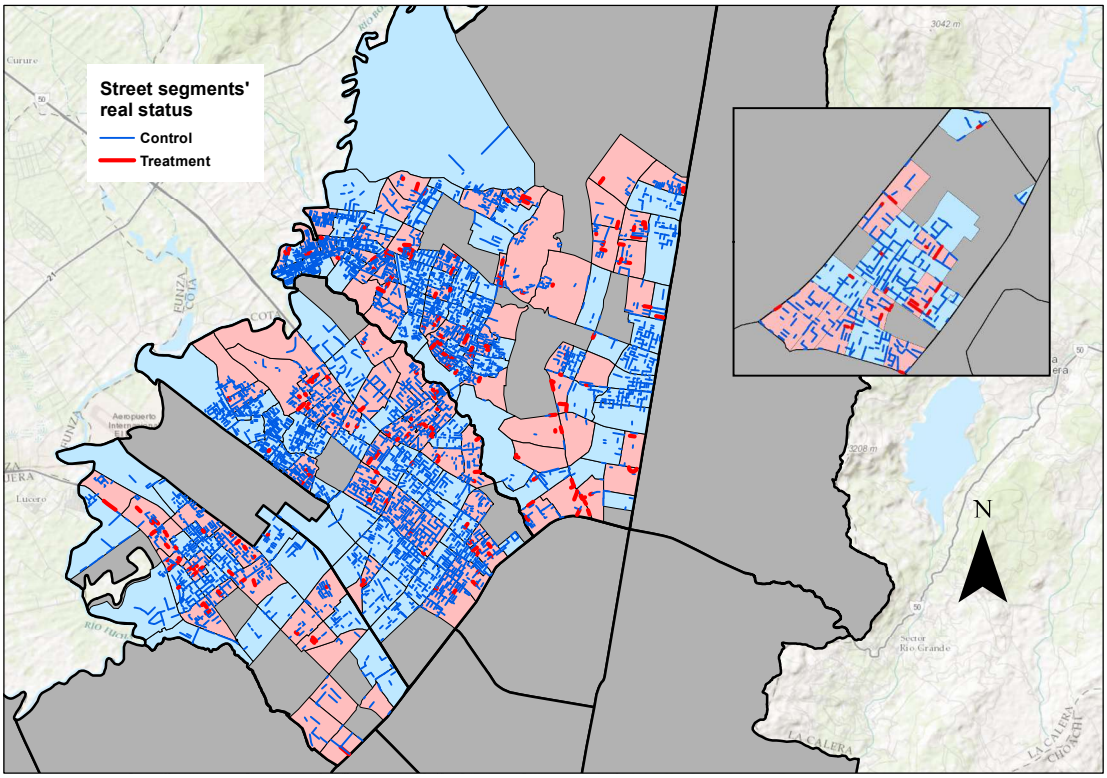

FIGURE 4. RANDOMIZED DESIGN FOR GOOD BARTENDERS PROGRAM

Source: Authors' elaboration from georeferenced administrative data.
Notes: Blue areas denote control quadrants and red areas denote treatment quadrants. Blue lines represent control street segments and red lines depict treated street segments. Due to scale, Los Mártires is shown in the small block on the upper right side.

In the second stage, we randomly select street segments within the 109 treated quadrants that contain bars that will receive the intervention. Given time, budget, and logistical constraints, our power calculations suggested the minimum number of treated bars should be 270 in order to estimate an effect just below 0.30 standard deviations for brawls (Ham et al., 2018). We selected street segments until we reached this number of establishments, with additional replacement streets in case they were required. 209 street segments with at least one bar were originally selected into the treatment group. Figure 4 maps treated and control street segments

[7]We assessed different exclusion criteria, including removing the most dangerous 20%, 10%, and 5% street segments. The mean and standard deviation for violent brawls did not change substantially under these different scenarios, so we opted for the 10% threshold to maximize statistical power and minimize any safety concerns during implementation.
[8]Figure A.4 in the Appendix maps each step of the first-stage randomization procedure.

within police quadrants for each locality. Blue areas represent control quadrants and red areas represent treated quadrants. Ideally, treated street segments should only be seen in red areas. Due to unforeseen circumstances during implementation, 19 street segments originally assigned to control effectively received the intervention (see Figure A.5 in the Appendix for a side-by-side visual comparison). Comparing violent incidents between assigned and effective treatment status shows that this discrepancy can be considered as good as random in terms of brawls and most alcohol-related incidents (See Table A.2 in the Appendix). However, we take this difference into account in our empirical analysis by presenting treatment effect estimates that instrument effective treatment with assigned status.

This two-stage design allows estimating both direct and spillover effects. By comparing outcomes in treated and control street segments *within* treated quadrants, we can estimate the direct impact of the intervention. However, if bar owners in the same police quadrant discuss the intervention among themselves, then our treatment effect estimates would be biased. If the intervention does not reduce brawls but displaces them to nearby streets in the same quadrant, our estimates would also be invalid. To avoid such issues, we also compare outcomes between control street segments *across* treated and control police quadrants. The latter locations constitute a "pure" control group. Our procedure therefore creates three groups from an eligible sample of 5,987 streets: 228 treated street segments located within treated police quadrants (D: directly treated units), 2,730 control street segments located within treated police quadrants (S: indirectly treated units), and 3,029 control street segments located within untreated police quadrants (C: control units).

We highlight that this experiment would not have been possible without collaboration from the Secretariat of Security, Coexistence, and Justice, and Fundación Bavaria. Their shared information and enthusiasm in designing and testing the effectiveness of a program using experimental methods allows us to contribute evidence on the relationship between alcohol consumption and alcohol-attributable violence. Throughout this project, our partners provided information and insightful feedback. Our pre-analysis plan presents statistical power calculations (Ham et al., 2018). Given the sample size, we have sufficient power to detect a change in brawls of 0.27 standard deviations (SD) for direct effects and 0.30 SD for indirect effects assuming an R^2 coefficient of zero.

IV Data and Empirical Strategy

A Data

We employ two sources of data. First, we gather survey data in directly treated and control bars to study if the intervention led to any changes in bartender practices and patron behavior. Second, we use georeferenced administrative data on reported incidents from the Secretariat of Security, Coexistence, and Justice and match it to bar locations from the brewery's client list in order to estimate the direct and indirect effects of the Good Bartenders program on alcohol-related violence.

The survey was carried out by a firm that had previous experience interviewing bartenders, which was hired through a public procurement process. Due to budget constraints, survey data was only collected after the program. We visited all bars located on treated street segments. Out of 270 establishments, 258 provided complete information, a response rate of 96%. The remaining 12 treated bars refused to participate or had changed ownership at the time of the survey. We compare respondents and non-respondents, but find no systematic differences across these establishments or their bartenders (See Appendix Table A.3) We additionally surveyed 320 bars –selected at random– in pure control street segments. Therefore, our survey sample consists of 578 bars in 482 street segments, representative of directly treated and control street segments in the four selected localities. We did not collect survey data for bars on indirectly treated street segments.

The survey instrument was designed to measure potential changes in alcohol consumption patterns such as the value of alcohol sales in the past three months, the value of water sold in the past three months, and the value of food sold in the past three months. It also gathers information on bar attributes (e.g. years of operation, hours of operation, whether it has a restroom, number of tables and chairs, whether private security is provided, the kind of alcohol served, whether non-alcoholic beverage options are available, and the type of food served), bartender characteristics (e.g. whether s/he is the owner, whether s/he lives in the neighborhood, educational attainment, years of experience as a bartender), and characterizes the bar's patrons (regulars or not, gender, age, number of clients during the previous weekend, whether clients pay by round or at the end).

The survey also collects data on the street, including its level of cleanliness, the quality of the pavement, whether the bar is located on a street corner, whether the street segment is primarily residential, whether there are other commercial establishments on the street segment (and if so, of what kind), and whether there are

TABLE 1. DESCRIPTIVE STATISTICS FROM SURVEY DATA

	Control		Directly treated		
	Mean	(SD)	Mean	(SD)	Pr(C)=(D)
A. Bar attributes					
Legal entity	0.11	(0.31)	0.07	(0.25)	0.108
Established by bartender	0.59	(0.49)	0.56	(0.50)	0.466
Age of bar (in years)	10.95	(10.49)	10.53	(9.99)	0.636
Hours open per week	50.39	(27.23)	49.22	(29.85)	0.580
Number of workers	1.85	(1.19)	1.97	(1.14)	0.242
Number of family workers	0.45	(0.64)	0.47	(0.69)	0.736
Bar is part of bartender's house	0.40	(0.49)	0.39	(0.49)	0.882
Bar is in bartender's neighborhood	0.78	(0.41)	0.72	(0.45)	0.090
Bathroom availability	0.93	(0.26)	0.96	(0.20)	0.116
Mixed bathrooms	0.49	(0.50)	0.51	(0.50)	0.716
Number of tables	5.78	(5.43)	6.20	(5.17)	0.403
Number of chairs	21.77	(21.24)	22.71	(16.54)	0.617
Has an entertainment system	0.93	(0.25)	0.97	(0.18)	0.087
Has private security	0.24	(0.43)	0.24	(0.43)	0.871
B. Bartender characteristics					
Owner	0.69	(0.46)	0.68	(0.47)	0.942
Male	0.46	(0.50)	0.45	(0.50)	0.808
Married	0.59	(0.49)	0.62	(0.49)	0.452
Age	48.01	(14.74)	47.31	(15.02)	0.676
Education: incomplete high school	0.39	(0.49)	0.32	(0.47)	0.099
Education: complete high school	0.40	(0.49)	0.43	(0.50)	0.501
Education: post-secondary studies	0.21	(0.41)	0.25	(0.43)	0.272
Experience as bartender (in years)	12.52	(12.00)	11.46	(10.85)	0.304
C. Patron characteristics					
Fraction of regulars	0.92	(0.28)	0.88	(0.33)	0.142
Amount of patrons (last weekend)	42.64	(57.76)	48.47	(61.47)	0.276
Minimum age of patrons	25.50	(8.76)	26.47	(8.78)	0.174
Maximum age of patrons	62.90	(12.62)	61.03	(11.51)	0.105
Provides credit to patrons	0.48	(0.50)	0.50	(0.50)	0.735
D. Street characteristics					
Clean street	0.88	(0.33)	0.87	(0.33)	0.993
Paved street	0.72	(0.45)	0.71	(0.46)	0.752
Corner bar	0.22	(0.42)	0.20	(0.40)	0.542
Residential area	0.65	(0.48)	0.67	(0.47)	0.437
Nearby alcohol-serving establishments	0.73	(0.44)	0.69	(0.46)	0.276
Street vendors present	0.15	(0.36)	0.16	(0.37)	0.793
Bars	320		258		
Street segments	264		218		

Source: Authors' elaboration from survey data.

Notes: The table presents means for each variable and standard deviations in parentheses. The p-values in the final column are obtained by regressing each variable on a treatment group dummy variable and locality fixed-effects with clustered standard errors by street segment, and correspond to the hypothesis that means for Control and Directly treated groups are equal. A joint significance test of all the variables and locality fixed-effects on treatment status yields an F-statistic of 1.38 with a p-value of 0.0814.

street vendors on the street segment. Overall, these self-reported data provide rich and unique insights on bars, bartenders, and patrons that help characterize alcohol consumption patterns in neighborhood bars in the four localities targeted by the program.

Table 1 shows descriptive statistics from the survey data by randomized groups. We present means and standard deviations, as well as the p-value from a test for equality between control and treatment groups means. Neighborhood bars tend to be family-owned and established by the bartender, who often lives in the same property or neighborhood. On average, these bars provide seating for about 20 people (5 tables with 4 chairs), have bathrooms, and entertainment systems (e.g. music, television). Few bars report paying for private security. Bartenders are equally male and female, are usually the owners of the bar, are married, have completed high school or less education, and have accumulated more than a decade of experience on average. Patrons are mostly regular customers, aged between 35 and 69 years, and in some cases receive credit for alcohol consumption. Streets are mostly clean and paved, with few street vendors. Most bars are in residential areas but near other alcohol-serving establishments. About 22% of bars are located on a street corner. Averages are statistically equal between treated and control bars at the 5% level, and a joint test including all variables rejects their shared significance on treatment status at the same confidence level (p-value=0.081). We include all the variables in Table 1 as controls in our survey regressions.

Reports of alcohol-attributable violence are provided in administrative records by the SSCJ. The Unique Number for Security and Emergencies database (NUSE 123, for its acronym in Spanish) collects information on citizens' reports of violent incidents across the entire city. NUSE 123 receives calls made by citizens requesting emergency assistance, classifies them according to the emergency, and assigns them to the relevant agencies to address the situation.[9] When a person calls the emergency services, the location from which the call is made is recorded, so we are able to georeference the latitude and longitude of the report. Additionally, the operator transcribes the conversation and assigns a reference code contained in an institutional incident classification guide.

The NUSE 123 code for violent brawls – our main outcome of interest – is 934, and is defined as "*any incident or altercation that may arise between two or more people causing physical aggression, which may even lead to endangering someone's life*" (SSCJ, 2016). This concept is similar to the definition of aggravated assault used by the FBI's Uniform Crime Reporting program: "an unlawful attack by one person upon another

[9]In Colombia, emergency services corresponds to the phone number "123" rather than "911" as in the United States or "999" in the United Kingdom.

for the purpose of inflicting severe or aggravated bodily injury". The raw data provided by the SSCJ consist of all reported brawls between January 2014 and February 2019. The database contains over 6 million reports for the entire city. However, the same event may have more than one entry since multiple citizens could report the same incident. Once the database is collapsed by event, there are over 2 million unique georeferenced observations for Bogotá, which we then filter for our four localities. We restrict the analysis to brawls that occur between 4 PM and 1 AM, hours when neighborhood bars are open or have recently closed.

NUSE 123 also collects reports on other incidents that may be related to excessive alcohol consumption. We include reports of personal injury, drunk and disorderly behavior, disturbing the peace, and illegal alcohol sales as additional alcohol-related outcomes to examine whether the program had an effect on other incidents that occur within and around neighborhood bars.[10]

We also employ georeferenced records from the Statistical, Criminal, Contravention and Operational Information System (SIEDCO, for its acronym in Spanish) from the National Police. The purpose of these data are to measure potential effects on traditional violence measures: homicides, robberies, and personal injury events. The difference with NUSE is that these data are not citizen reports but actual police records. We use SIEDCO data in our analysis below as controls, additional outcomes since sometimes brawls result in one of these events, and to perform falsification exercises, since homicides for example, should not be affected by the Good Bartenders program.

We merge these geocoded reports with bar locations using an updated client list provided by Fundación Bavaria. This list contains over 40,000 bars, with their respective latitude and longitude. Together, the violence data and client records allow us to jointly measure the relationship between alcohol availability and violence in the four selected localities, something which would not have been possible without this public-private partnership. These merged data contain reports for thousands of alcohol-related incidents within and around 8,909 bars over 62 months. We highlight that these data cover eligible streets and quadrants in the four selected localities using the criteria defined in Section III, and are not necessarily representative of the entire city of Bogotá.

[10]Personal injuries are defined by the SSCJ as "an assault against the life or personal health of an individual that leaves trauma or damages to a person's health", and is identified with code 910. Drunk and disorderly behavior occurs "when a person or group of individuals is under the influence of alcohol and behaving in a disorderly manner that affects others", and is identified with code 924. Disturbances to peace include events "that disturb the order or public tranquility, due to high noise levels, during the evenings until 3 AM", and is identified with code 932. Illegal alcohol sale "includes places that sell bootleg alcohol and establishments with liquor licenses that are open beyond the established time", and is identified with code 926M (SSCJ, 2016).

TABLE 2. DESCRIPTIVE STATISTICS FROM GEOREFERENCED ADMINISTRATIVE DATA

	(C) Control	(S) Indirectly treated	(D) Directly treated	Pr(C)=(S)=(D)
Brawls	0.887	0.889	0.878	0.638
	(1.861)	(1.580)	(1.513)	
Personal injury (reports)	0.250	0.244	0.239	0.221
	(0.864)	(0.736)	(0.678)	
Drunk and disorderly	0.179	0.179	0.206	0.068
	(0.564)	(0.539)	(0.585)	
Disturbing the peace	1.187	1.139	1.204	0.539
	(2.806)	(2.414)	(2.376)	
Illegal alcohol sales	0.098	0.099	0.094	0.669
	(0.447)	(0.455)	(0.417)	
Homicides	0.001	0.001	0.001	0.814
	(0.040)	(0.038)	(0.041)	
Robberies	0.052	0.051	0.073	0.022
	(0.258)	(0.264)	(0.315)	
Personal injury (events)	0.024	0.026	0.029	0.726
	(0.190)	(0.253)	(0.190)	
Street segments	3,029	2,730	228	

Source: Authors' elaboration from georerefenced administrative data.
Notes: The table presents monthly means for each variable and standard deviations in parentheses before the implementation of the Good Bartenders program (January 2014 to July 2018). The p-values in the final column are obtained by regressing each variable on two treatment group dummy variables and locality fixed-effects with two-way clustered standard errors by street segment and police quadrant, and correspond to the hypothesis that coefficients for the Indirectly and Directly treated groups are jointly zero.

Table 2 shows descriptive statistics for alcohol-related violence before implementation of the Good Bartenders program (January 2014 to July 2018). On average, there is less than one reported brawl per month in each street segment, but the standard deviation is between 1.5 and 1.8. Other incidents are less frequent, such as reports of personal injury, drunk and disorderly behavior, and illegal alcohol sales. However, reports for disturbing the peace occur more frequently, with at least 1.2 reports per month in our sample. We test if means across the three randomized groups are equal at baseline. The last column shows we can accept the hypothesis that alcohol-related incidents are balanced across the three randomized groups before the intervention at the 5% level. Other crimes are mostly balanced, with the exception of robberies. To account for these pre-program differences, we include homicides, robberies, and personal injury events as controls in our regressions.

These data are appropriate to answer our research question because they characterize bartender practices, bartender and patron outcomes, and alcohol-related incidents. They allow tracing out our indirect causal pathway which first tests if bartenders that adopt standardized practices in their establishments can promote more responsible alcohol consumption and then examines whether changes in alcohol consumption subsequently reduce reports of alcohol-attributable violence. Tables 1 and 2 lend support to the internal validity of our experiment to explore this causal link.

B Empirical Strategy

We analyze the data using the empirical strategy outlined in our Pre-Analysis Plan (Ham et al., 2018). Given that survey data is only available after the intervention, we estimate the effects of the Good Bartenders program using a cross-section post specification:

$$y_{is} = \alpha + \beta D_s + \gamma X_{is} + \varepsilon_{is} \tag{1}$$

where i refers to a bar and s its street segment. The β coefficient estimates mean differences in outcomes between bars in directly treated and control street segments after the intervention. The estimates from these regressions will provide evidence of changes in bartender practices, as well as any behavioral changes by bartenders and their patrons. For instance, we compare average sales of alcohol, water, and food between treated and control bars, as well as perceptions of alcohol-related violence within and around the bar. We include the variables in Table 1 as controls in X_{is} to improve precision, as well as fixed effects by locality to account for the stratified nature of the program (McKenzie, 2012). We estimate Equation (1) by Ordinary Least Squares (OLS) with clustered standard errors by street segment. Note that we are unable to separate direct and indirect effects for survey outcomes because we did not interview bars in indirectly treated locations.

We then estimate the effects of the intervention on alcohol-attributable violence using a difference-in-difference approach because administrative data are available before and after the program:

$$y_{sqt} = \alpha + \beta_1 D_{st} + \beta_2 S_{qt} + \gamma X_{sqt} + \lambda_s + \delta_t + u_{sqt} \tag{2}$$

where s refers to street segments, q to police quadrants, and t indexes time (month-year cells).[11] Our main dependent variable of interest is the number of brawls in street segment s in police quadrant q during period t, but we also examine other alcohol-related incidents as outcomes. Equation (2) lets us estimate two treatment effects. First, β_1 captures the direct effect between directly and indirectly treated street segments before and after the program. Second, β_2 estimates the spillover effects of the intervention by comparing brawls between indirectly treated and control street segments over time.[12] We include time-varying controls at the street segment level (e.g. homicides, robberies, and personal injury events), the lagged outcome, time-invariant location fixed effects as the data permits (e.g. locality, quadrant, and street segment), as well as time effects to control for secular trends in outcomes. In some specifications, we also control for location-specific linear time trends. Our main specification is estimated by OLS, but we also consider other estimation procedures, including Poisson and event studies. All regressions employ two-way clustered standard errors by police quadrant and street segment due to the multi-stage randomization.

Both specifications estimate the intent-to-treat effects (ITT) of the Good Bartenders program. However, these ITT effects may be biased for two reasons. On the one hand, we need to adjust for compliance with the intervention. If bartenders did not absorb the information nor adopt the suggested practices, then the treatment was not binding. Fortunately, the team visited each treated bar as "*mystery shoppers*" to verify whether bartenders used the provided information and implemented the suggested practices in their establishments. Table A.4 in the Appendix shows the fraction of bars that fulfill each criteria individually. We instrument compliance with random assignment for an "intersection" compliance indicator: bars that fulfill all seven mystery shopper criteria (61% of treated bars fulfill all criteria).[13] On the other hand, we mentioned that effective treatment differed from assigned treatment during implementation, and therefore adjust our estimates to account for this difference. We employ instrumental variable methods to obtain Local Average Treatment Effects (LATE) that control for both these aspects (Angrist et al., 1996).

While we use standard estimation methods to conduct inference on our results, we also conduct additional procedures to ensure that our results are credible and transparent. First, we perform randomization inference on our OLS estimates of direct and spillover effects of the Good Bartenders program on violence, as well

[11] We also present estimates using weekly-level reports for robustness.

[12] The variables on the main coefficients, D_{st} and S_{qt}, are interactions between treatment indicators (T_s^1 and T_q^2) and a variable equal to one after the Good Bartenders program begins in August 2018 ($Post$).

[13] We also estimate results using alternative definitions of compliance. These results are virtually identical to the ones presented in the next section and are therefore omitted here, but are available upon request.

as the probability that these effects are equal (Imbens and Wooldridge, 2009). Randomization inference does not rely on large-sample assumptions, the validity of the model, or the sample used to estimate that model (Heß, 2017). Second, since we assess several outcomes using the same source of exogenous variation, we present q-values that adjust for the false discovery rate (Benjamini and Hochberg, 1995). These procedures increase the transparency of our estimation procedures and lend support that any results we find are due to the intervention and not chance findings. Additionally, we conduct robustness tests and falsification exercises to ensure that our findings are indeed capturing the causal effect of the Good Bartenders program on bartender and patron behavior, as well as alcohol-related brawls and other incidents.

V Results

A Effects on Bartender and Patron Behavior

Table 3 shows treatment effect estimates for survey outcomes. The first specification is an unconditional comparison of means between directly treated and control bars, the second includes all controls in Table 1, and the third estimates a local average treatment effect that adjusts for compliance with mystery shopper criteria. We also present control group means and standard deviations.

The first set of results examines changes in sales practices. The Good Bartenders manual suggests that bartenders could set the pace for heavy drinkers by encouraging water and food consumption between drinks, without necessarily selling more alcohol. The evidence in Table 3 indicates that treated bartenders were not selling more alcohol compared to control bartenders. However, the amount of water and food sold in treated bars increased significantly. On average, treated bars sold 56% more water and 67% more food than untreated bars. When we adjust for compliance, the estimated increase is 87% for water and 107% for food, although these results are more imprecise. These findings are stable across specifications and remain significant at the 1% level after multiple hypothesis adjustments. Since not all bars report selling water and food, as evidenced by the number of observations in Table 3, we explore extensive margin changes in these practices in Appendix Table A.5. Results suggest that some bars begin selling water and food after the Good Bartenders program, but the estimates are marginally insignificant after carrying out multiple hypothesis adjustments. The evidence also suggests that bartenders are selling pre-packaged food instead of prepared meals. Thus, bars that already sold food and water in some capacity report significant increases in sales after the Good Bartenders program when compared to untreated bars.

TABLE 3. EFFECTS OF THE GOOD BARTENDERS PROGRAM ON SURVEY OUTCOMES

	Observations	Mean [SD]	(1) OLS	(2) OLS	(3) IV
A. Sales					
Log alcohol sales	542	14.94	0.192	0.137	0.219
		[1.97]	(0.167)	(0.147)	(0.234)
q-value			0.378	0.353	0.354
F-statistic					396.4
Log water sales	499	11.86	0.509	0.560	0.870
		[2.14]	(0.171)	(0.164)	(0.255)
q-value			0.010	0.003	0.003
F-statistic					420.8
Log food sales	453	12.41	0.638	0.674	1.075
		2.36	(0.201)	(0.183)	(0.296)
q-value			0.010	0.002	0.002
F-statistic					362.3
B. Violence perceptions					
Brawls in bar (past 3 months)	578	0.06	-0.013	-0.018	-0.029
		[0.24]	(0.018)	(0.020)	(0.031)
q-value			0.489	0.353	0.354
F-statistic					427.9
Brawls in street (past 3 months)	578	0.35	-0.029	-0.044	-0.070
		[0.48]	(0.040)	(0.040)	(0.063)
q-value			0.489	0.353	0.354
F-statistic					427.9
Less brawls in past 3 months?	578	0.90	0.036	0.024	0.038
		[0.30]	(0.024)	(0.025)	(0.039)
q-value			0.272	0.353	0.354
F-statistic					427.9
Controls			No	Yes	Yes

Source: Authors' elaboration from survey data..

Notes: Each row presents results from a separate regression. Clustered standard errors by street segment are shown in parentheses. The table reports cross-section estimates of the direct effects of the Good Bartenders program on survey outcomes (see Section IV). Columns (1) and (2) report OLS estimates, while column (3) instruments compliance with all seven mystery shopper criteria with random assignment (See Table A.4). Given that we estimate results for several outcomes using the same source of exogenous variation, we present *q*-values that adjust for multiple hypothesis testing, calculated using the method by Benjamini and Hochberg (1995) that controls for the false discovery rate (FDR) described in Anderson (2008).

The second set of results explores changes in violence perceptions for bartenders within and around the bar. The survey asked bartenders whether a brawl had occurred on their premises or outside their establishment during the past three months. On average, few control bartenders reported brawls inside their establishment (6%) while more reported brawls outside their bar (35%). Treated bars show no statistically significant difference after the Good Bartenders program in these indicators. While the sign of the estimated coefficient is negative, the estimates are imprecise. Finally, we asked bartenders if they believed that brawls had fallen in

the past three months. Almost 90% agreed, but we find no difference in those perceptions between treated and control establishments.

We also explore how bartenders who experienced a violent brawl in the past three months dealt with the situation. The Good Bartenders manual suggests trying to defuse the conflict or calling the police, as opposed to expelling the patrons from the establishment. Table 4 shows results from an OLS regression (specification (2) in Table 3) that interacts treatment status with an indicator variable equal to one if the bartender reported a brawl inside the bar in the past three months. The table shows the coefficient on the main effects and their interaction, with the base category consisting of bars in control streets that did not report a brawl.

TABLE 4. EFFECTS OF THE GOOD BARTENDERS PROGRAM ON BARTENDER STRATEGIES

	Expel the customers	Defuse the conflict	Call the police	Multiple strategies
Brawls in bar (past 3 months)	0.611	0.172	0.371	0.274
	(0.106)	(0.078)	(0.101)	(0.094)
RI: $Pr(D = 0)$	0.212	0.920	0.802	0.786
q-value	0.001	0.080	0.002	0.015
Directly treated	0.003	-0.004	-0.002	0.000
	(0.004)	(0.004)	(0.004)	(0.004)
RI: $Pr(D = 0)$	0.454	0.308	0.642	0.932
q-value	0.602	0.496	0.705	0.916
Brawls in bar (past 3 months) $\times$ Directly treated	-0.158	0.261	0.145	0.168
	(0.160)	(0.156)	(0.162)	(0.168)
RI: $Pr(D = 0)$	0.318	0.102	0.328	0.348
q-value	0.496	0.232	0.496	0.496
Adjusted R^2	0.648	0.426	0.533	0.429
Observations	587	587	587	587
Street segments	482	482	482	482

Source: Authors' elaboration from survey data.

Notes: Each column presents results from a separate regression. Clustered standard errors by street segment are shown in parentheses. The table reports cross-section estimates of the heterogeneous effects of the Good Bartenders program on strategies to deal with brawls in the establishment. We interact treatment status with an indicator variable equal to unity if the bartender reported a brawl inside the bar in the past three months. The table shows the coefficient on both main effects and their interaction, with the base category consisting of bars in control streets that did not report a brawl in the past three months. We also present p-values obtained by randomization inference with 500 replications (Heß, 2017), labeled RI, for the hypothesis that direct treatment effects are zero or $Pr(D) = 0$. Given that we estimate multiple coefficients for several outcomes using the same source of exogenous variation, we present q-values that adjust for multiple hypothesis testing, calculated using the method by Benjamini and Hochberg (1995) that controls for the false discovery rate (FDR) described in Anderson (2008).

Compared to control bars that did not report a brawl during the past three months, untreated bars that did experience a brawl implemented different strategies. Most of these bars expelled customers, called the police, and tried to defuse the conflict. Many also report using several of these strategies. In comparison, treated bars show no significant difference with respect to control bars conditional on no brawl. The last row captures

how treated bartenders reacted when they faced a brawl in their bar. While all the coefficients are statistically indistinguishable from zero, their direction follows the expectations from the program. Bartenders are less likely to expel the patrons and more inclined to defuse the conflict, call the police to intervene, or both. A potential explanation for this result is the fact that few bars report a brawl in the past three months. Only 31 bars report a recent brawl, of which 19 are located in control streets and 12 in treatment streets. These figures –while positive in terms of low levels of brawls– suggest that we have low statistical power to examine how bartenders deal with brawls in their establishment. They do suggest that when facing conflict, bartenders seem to be following the guidelines recommended by the program.

These results show that treated bartenders implemented the practices suggested by the Good Bartenders program. While selling the same amount of alcohol, they increased sales of food and water, which can slow down the negative consequences of rapid alcohol intake. Effects are larger for compliant bars. Therefore, the program does lead bartenders to implement practices that promote more responsible alcohol consumption among their patrons. Few bartenders report a recent brawl in their establishment or street segment, and we find no change in these individuals' perception of alcohol-related violence within and around their bar. While the strategies chosen by bartenders that experienced a brawl follow the recommendations in the Good Bartenders manual, the evidence is suggestive. We now estimate effects on reports of brawls and other incidents to determine whether the significant changes we find result in lower levels of alcohol-related violence.

B Effects on Alcohol-Related Violence

Table 5 shows treatment effect estimates of the Good Bartenders program on brawls. We present six specifications, which vary in the level of fixed effects, controls, and location-specific linear time trends. The results largely indicate that the number of reported brawls was unchanged by the Good Bartenders program in the selected localities. Estimates across all specifications indicate no significant direct effects nor spillovers compared to control street segments, using both conventional and randomization inference p-values. We also test the null hypothesis that coefficients for direct and indirect effects are equal in each specification, which we are unable to reject in all cases.

TABLE 5. EFFECTS OF THE GOOD BARTENDERS PROGRAM ON BRAWLS

	(1)	(2)	(3)	(4)	(5)	(6)
Direct effect (D)	-0.000	0.013	0.013	0.012	0.007	-0.022
	(0.070)	(0.038)	(0.037)	(0.037)	(0.036)	(0.044)
RI: $Pr(D = 0)$	0.704	0.704	0.692	0.726	0.828	0.600
Indirect effect (S)	0.035	0.023	0.023	0.030	0.004	0.008
	(0.043)	(0.020)	(0.020)	(0.019)	(0.017)	(0.018)
RI: $Pr(S = 0)$	0.146	0.146	0.140	0.042	0.758	0.648
RI: $Pr(D = S)$	0.788	0.788	0.790	0.606	0.908	0.516
Adjusted R^2	0.003	0.417	0.418	0.419	0.419	0.426
Mean brawls	0.887	0.887	0.887	0.887	0.887	0.887
SD brawls	1.861	1.861	1.861	1.861	1.861	1.861
Fixed-effects	Locality	Street	Street	Street	Street	Street
Controls	No	No	Yes	Yes	Yes	Yes
Linear time trends	No	No	No	Locality	Quadrant	Street
Observations	371,194	371,194	365,207	365,207	365,207	365,207
Street segments	5,987	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221	221

Source: Authors' elaboration from georerefenced administrative data.
Notes: In all columns, the outcome variable is the number of violent brawls. Each column presents results from a separate regression. Two-way clustered standard errors by street segment and police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of the Good Bartenders program (see Section IV). We also present p-values obtained by randomization inference with 500 replications (Heß, 2017), labeled RI, for three hypotheses: i) direct effects are zero or $Pr(D) = 0$, ii) indirect effects are zero or $Pr(S) = 0$, as well as whether iii) direct and indirect effects are equal or $Pr(D = S)$.

These findings are robust to different treatment definitions and alternative estimation procedures. The Appendix shows results using a street segment's assigned status instead of effective treatment (Table A.6), estimates from Poisson regressions (Table A.7), and LATE estimates that adjust for compliance with mystery shopper criteria and differences between assigned and effective treatment (Panels A and B in Table A.8, respectively). Results that adjust for compliance and treatment differences are insignificant and similar to the results in Table 5 for all specifications. We also estimate the effects of the Good Bartenders program on brawls using weekly- instead of monthly-level reports. The estimates confirm that there are no statistically significant changes in the number of reported brawls after the Good Bartenders program for different specifications, procedures, treatment definitions, compliance and treatment differences (See Tables A.9 to A.12).

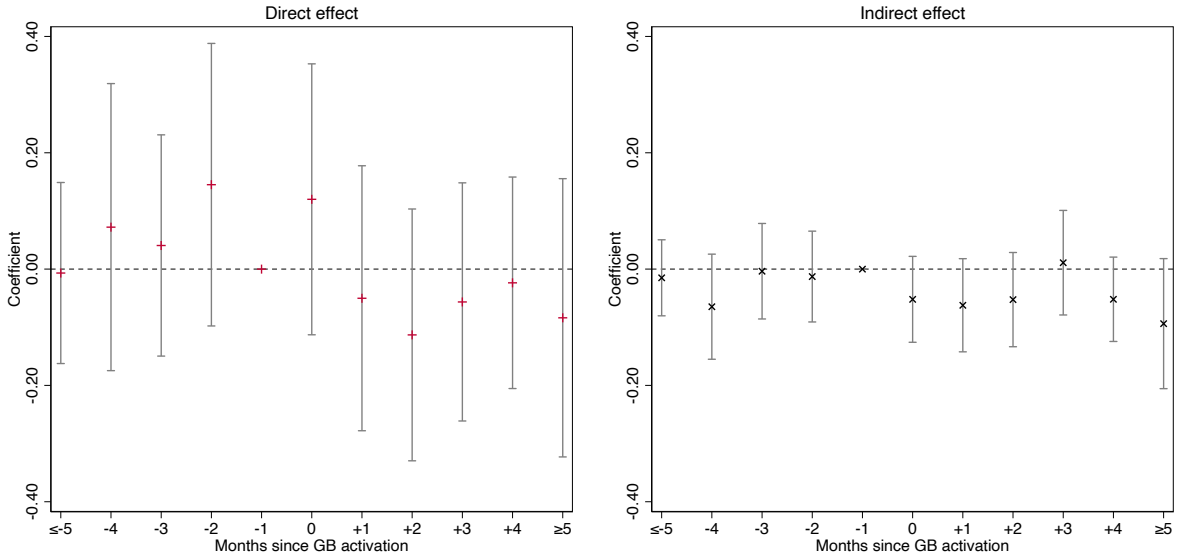

FIGURE 5. EVENT STUDY EFFECTS OF THE GOOD BARTENDERS PROGRAM ON BRAWLS

Source: Authors' elaboration from georerefenced administrative data.
Notes: Direct effects shown as "+" and indirect effects as "x". 95% confidence intervals shown around the point estimates.

Given that program roll-out differed across localities and bars began treatment at different times, we use event study methods to unify exposure to the Good Bartenders program.[14] We estimate linear difference-in-difference regressions that examine differential trends for up to 5 months before and after the program. The results are shown in Figure 5, which presents estimates using the specification in column (3) of Table 5 that includes controls and street-level fixed effects.[15] Trends in reported brawls between treated and control street segments are parallel before the start of the program, and show no statistically significant changes in directly or indirectly treated streets up to 5 months after exposure. The same exercise using weekly reports with a window of 8 weeks before and 16 weeks after provides similar findings (See Appendix Figure A.7).

These results provide evidence that the Good Bartenders program did not have direct or spillover effects on reported brawls within and around bars. Given our sample size, goodness of fit, and standard errors from the regressions, the minimum detectable effect ranges between 0.10 to 0.20 for direct effects and 0.05 to 0.12 for spillovers.[16] The coefficients in Table 5 and the results in the Appendix are below these values and close

[14] In particular, we identify the month (or week) that the bar was "activated" and analyze periods before and after that event. The full estimates are not shown due to space restrictions but are available from the authors upon request.
[15] Event study results for all specifications provide similar results and are shown in Appendix Figure A.6.
[16] This calculation tells us how large the coefficient must be to find a statistically significant effect, and is expressed in the units of each dependent variable. In order to declare a coefficient statistically significant it needs to be 1.96 standard errors away from zero; and to have an 80% chance of finding a coefficient that is at least 1.96 standard errors away from zero 80% of the distribution from which you draw coefficients needs to be to the right of 1.96. Because the inverse normal of 80% is 0.84, that's achieved with a

to zero with tight confidence intervals, which suggests we are estimating precise zero treatment effects of the Good Bartenders program on brawls.

Given that the SSCJ also collects reports on other alcohol-related incidents in NUSE 123, we explore whether other outcomes changed in response to the Good Bartenders program. We estimate the effects of the intervention on reports of personal injury, drunk and disorderly behavior, disturbing the peace, and illegal alcohol sales. Table 6 presents results using our preferred specification with controls and street-level fixed effects.[17]

TABLE 6. EFFECTS OF THE GOOD BARTENDERS PROGRAM ON OTHER ALCOHOL-RELATED INCIDENTS

	Brawls	Personal injury	Drunk and disorderly	Disturbing the peace	Illegal alcohol sales
Direct effect (D)	0.013	0.003	-0.039	-0.129	0.009
	(0.037)	(0.019)	(0.018)	(0.056)	(0.009)
RI: $Pr(D = 0)$	0.692	0.882	0.022	0.092	0.396
q-value	0.882	0.882	0.086	0.086	0.547
Indirect effect (S)	0.023	0.005	0.013	0.015	0.007
	(0.020)	(0.010)	(0.007)	(0.036)	(0.005)
RI: $Pr(S = 0)$	0.126	0.440	0.036	0.688	0.106
q-value	0.418	0.673	0.307	0.673	0.417
RI: $Pr(D = S)$	0.790	0.884	0.006	0.062	0.844
Adjusted R^2	0.418	0.285	0.173	0.377	0.168
Mean outcome	0.887	0.250	0.179	1.187	0.098
SD outcome	1.861	0.864	0.564	2.806	0.447
Observations	365,207	365,207	365,207	365,207	365,207
Street segments	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221

Source: Authors' elaboration from georerefenced administrative data.
Notes: Each column presents results from a separate regression. The reported specification is the same as in Column (3) of Table 4, which includes controls, the lagged outcome, and street-level fixed effects. Two-way clustered standard errors by street segment and police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of the Good Bartenders program (see Section IV). We also present p-values obtained by randomization inference with 500 replications (Heß, 2017), labeled RI, for three hypotheses: i) direct effects are zero or $Pr(D) = 0$, ii) indirect effects are zero or $Pr(S) = 0$, as well as whether iii) direct and indirect effects are equal or $Pr(D = S)$. Given that we estimate results for several outcomes using the same source of exogenous variation, we present q-values that adjust for multiple hypothesis testing, calculated using the method by Benjamini and Hochberg (1995) that controls for the false discovery rate (FDR) described in Anderson (2008).

The first column reproduces the results for brawls, which were unchanged after the program. We also find no evidence of changes in the number of reported incidents of personal injury and illegal alcohol sales. However, there is a statistically significant reduction in reports of drunk and disorderly behavior and incidents

normal centered at 1.96 + 0.84 = 2.8.

[17]The results we discuss in the text are qualitatively similar when using alternative specifications and estimation procedures. These estimates are not shown due to space restrictions but are available from the authors upon request.

of disturbing the peace. Compared to control streets, there is a 22% reduction in reports of drunk and disorderly behavior in directly treated streets where bars participated in the Good Bartenders program. This effect is robust to randomization inference and multiple hypothesis testing. While there is suggestive evidence that drunk and disorderly behavior spills over to indirectly treated streets, this result becomes insignificant after multiple hypothesis adjustments. Reports of disturbing the peace fall by almost 11% in directly treated streets, also a result robust to different adjustments, and show no evidence of significant spillovers. These results indicate that while the Good Bartenders program did not change the number of reported brawls, the program did reduce other alcohol-related incidents, mainly related to coexistence outcomes.

We also look at two outcomes from SIEDCO data that may be affected by the program downstream: robberies and personal injury events. Results are shown in Table A.13 in the Appendix. Overall, we find no consistent effect of the program on both outcomes, since significance varies by specification. The randomization inference p-values also provide no conclusive evidence of any effects. Additionally, differences between the randomized groups were significant in pre-intervention robberies, which limits the extent to which we can extrapolate findings on those outcomes.

Are the results we find on other alcohol-related incidents robust? We conduct placebo regressions to determine whether these findings are attributable to the Good Bartenders program. We change the treatment timing, assuming that the program began in August 2016 and August 2017, respectively. The results are shown in Table A.14. For both cases, none of the coefficients on directly or indirectly treated streets are statistically different compared to control streets. These results are confirmed by randomization inference p-values and false discovery rate adjusted q-values. These placebo regressions support that while the program did not impact brawls, personal injuries, and illegal alcohol sales, it did significantly reduce incidents of drunk and disorderly behavior and disturbances to peace within and around directly treated bars without spillovers onto indirectly treated streets. We also conduct falsification tests on homicides in Table A.15. Results show no changes due to the program. Given that the intervention was not designed to affect this outcome, they further support our results. Therefore, we conclude that while some alcohol-related incidents did change due to the Good Bartenders program, reported brawls were unaffected.

VI Conclusion

This paper studies whether bartenders that adopt standardized practices can promote responsible alcohol consumption and subsequently reduce alcohol-attributable violence. We evaluate the effects of the randomized "Good Bartenders" program, which was designed and implemented by a public-private partnership between the social responsibility unit of Colombia's largest brewery and the Secretariat of Security, Coexistence, and Justice in Bogotá. This intervention was designed to achieve two objectives: i) provide bartenders with standardized practices that promote responsible alcohol consumption among patrons, and ii) give information and strategies to bartenders on how to defuse conflicts that may result in alcohol-related incidents within and around bars. Our two-stage randomized design allows estimating the direct and indirect effects of the program on reported incidents within and around bars. We first study whether the intervention led to changes in consumption practices using survey data for bars. Then, we estimate the effects of the program on alcohol-related violence using a georeferenced administrative panel of reported incidents.

Results indicate that the Good Bartenders program changes bartender practices but these actions have no subsequent effect on alcohol-related brawls. While alcohol sales were unchanged, bartenders sold more water (56% increase) and food (67% increase), thus contributing to more responsible alcohol consumption by patrons. However, we find no direct or spillover effects of the Good Bartenders program on violent brawls using different model specifications, treatment definitions, and alternative estimation procedures. Given the statistical power of our field experiment, these results may be interpreted as zero treatment effects. We do find some direct effects on other alcohol-related incidents, mainly reports of drunk and disorderly behavior (22% decrease) and disturbing the peace (11% reduction), with no significant spillovers to indirectly treated areas. These results are robust to placebo experiments, randomization inference, and multiple hypothesis testing. Overall, the Good Bartenders program encouraged bartenders to promote responsible alcohol consumption among patrons. However, these changes did not result in lower reports of brawls within and around bars, but do lead to some improvements on other alcohol-related incidents.

Our findings that the Good Bartenders program had a significant effect on consumption behavior by patrons but not on brawls, provide new evidence on the causal relationship of alcohol consumption and alcohol-related violence. These results suggest that there may be no relation between more responsible consumption and brawls. However, more responsible alcohol consumption does improve certain coexistence outcomes, lowering reports of drunk and disorderly behavior and disturbances to the peace around treatment locations. Perhaps the

causal mechanism that drives the relationship between alcohol and violence operates directly through changes in alcohol consumption levels, which were unchanged in our field experiment. Future studies may explore whether this hypothesis is true, which would help generate further knowledge of how alcohol and violence interact. Additionally, while we explore effects on certain types of violence, future research may focus on other incidents such as domestic violence, which has also been linked with alcohol.

While our results suggest that bartenders cannot reduce alcohol-attributable violence by themselves, they could be pivotal agents in a broad policy initiative. In Colombia, similar to other countries, bartenders operate similarly to community leaders. We find that bartenders implement practices that promote more responsible alcohol consumption, and interpret this result as evidence that they have a role to play in alcohol-related policies. Community-driven interventions should be considered as one of several actions that may help reduce the short- and long-term consequences of heavy drinking, while also addressing its root causes (Aguirre-Molina and Gorman, 1996). The type of intervention we study is less restrictive than commonly used approaches to regulate alcohol consumption. While prohibition approaches often yield short-term gains, they are not "one-size fits all". Our research suggests that less restrictive, community-driven efforts merit a place in conversations on how to reduce excessive alcohol consumption and its negative consequences.

Some questions remain on how to approach and formulate alcohol policies and regulation for the future. For instance, policymakers need to weigh a policy's restrictiveness, the possibility of implementing multiple complementary strategies, and consider public-private partnerships. Future work may explore answers to these questions and other aspects of the relationship between alcohol consumption and violence. While previous evidence has studied how to get consumers to reduce heavy drinking (Schilbach, 2019) and we have provided evidence on the role of bartenders to achieve the same objective, they are likely to interact, and identifying the extent of that complementarity would be useful. Other agents, such as alcohol producers and distributors, as well as large bars or clubs may also be part of the solution in ways that have not yet been explored. With further knowledge on what works to reduce excessive alcohol consumption and its negative effects, we can better understand these problems and propose effective solutions that improve welfare.

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A Online Appendix

TABLE A.1. ROLLOUT OF GOOD BARTENDERS PROGRAM BY LOCALITY

	August			September			
	8/10/2018	8/30/2018	8/31/2018	9/7/2018	9/22/2018	9/28/2018	Sum
Engativá	13	15	0	7	44	0	79
Fontibón	10	6	0	4	21	0	41
Los Mártires	10	9	0	0	9	0	28
Suba	17	5	25	9	0	66	122
Sum	50	35	25	20	74	66	270

Source: Authors' elaboration.
Notes: The table presents the number of bars treated at each date by locality.

TABLE A.2. DIFFERENCES BETWEEN ASSIGNED AND EFFECTIVE TREATMENT

	Control			Treatment		
	Assigned	Effective	(A)=(E)	Assigned	Effective	(A)=(E)
Brawls	0.887	0.772	0.277	0.889	0.772	0.299
	(1.861)	(1.190)		(1.577)	(1.190)	
Personal injury	0.250	0.204	0.005	0.244	0.204	0.037
	(0.864)	(0.570)		(0.733)	(0.570)	
Drunk and disorderly	0.179	0.213	0.458	0.181	0.213	0.428
	(0.564)	(0.580)		(0.543)	(0.580)	
Disturbing the peace	1.187	1.341	0.660	1.142	1.341	0.492
	(2.806)	(2.476)		(2.410)	(2.476)	
Illegal alcohol sales	0.098	0.047	0.001	0.099	0.047	0.002
	(0.447)	(0.233)		(0.453)	(0.233)	
Homicides	0.001	0.002	0.870	0.001	0.002	0.814
	(0.040)	(0.044)		(0.038)	(0.044)	
Robberies	0.052	0.061	0.962	0.053	0.061	0.750
	(0.258)	(0.277)		(0.268)	(0.277)	
Personal injury (events)	0.024	0.028	0.772	0.026	0.028	0.644
	(0.190)	(0.181)		(0.249)	(0.181)	
Street segments	3,048	3,029	19	2,939	2,958	19

Source: Authors' elaboration.

Notes: The table presents means for each variable and standard deviations in parentheses before implementation of the Good Bartenders program (January 2014 to July 2018). The p-values are obtained by regressing each variable on a dummy variable that identifies differences between assigned and effective randomized status, as well as locality and month-year fixed-effects with two-way clustered standard errors by street segment and police quadrant, and correspond to the hypothesis that means between assigned status and effective treatment are equal.

TABLE A.3. DIFFERENCES BETWEEN SURVEYED AND UNSURVEYED BARS

	Surveyed		Unsurveyed		
	Mean	(SD)	Mean	(SD)	Pr (S)=(U)
A. Bartender characteristics					
Male	0.446	(0.498)	0.500	(0.522)	0.714
Married	0.342	(0.475)	0.250	(0.452)	0.474
Education: high school or less	0.822	(0.384)	0.833	(0.389)	0.916
Education: college or more	0.136	(0.343)	0.167	(0.389)	0.778
B. Bar attributes					
Bathroom availability	0.791	(0.408)	0.833	(0.389)	0.701
Has mixed bathrooms	0.791	(0.408)	0.833	(0.389)	0.701
Number of tables	4.736	(4.106)	4.667	(3.798)	0.949
Number of chairs	17.279	(16.818)	16.833	(15.845)	0.922
Sells local beer	0.981	(0.138)	0.917	(0.289)	0.428
Sells spirits	0.364	(0.482)	0.583	(0.515)	0.135
Bars	258		12		

Source: Authors' elaboration.

Notes: The table presents means for each variable and standard deviations in parentheses before implementation of the Good Bartenders program. The p-values are obtained by regressing each variable on a dummy variable that identifies whether the treated bar was surveyed or not and locality fixed-effects with clustered standard errors by street segment, and correspond to the hypothesis that means for surveyed and unsurveyed bars are equal.

TABLE A.4. COMPLIANCE WITH GOOD BARTENDERS PROGRAM

Criteria	Share compliers
Stickers	0.937
Clock	0.915
Welcome mat	0.930
Picture frame	0.822
Uses water jug	0.933
Uses coasters	0.922
Bartender has offered food or water	0.870
Complies with at least 5 criteria	0.948
Complies with all criteria	0.611
Answered question correctly	0.722

Source: Authors' elaboration from program implementation data.
Notes: The table presents the fraction of locality bars that comply with each of the criteria in follow-up visits by mystery shoppers. Compliance rates are calculated on all 270 treated bars.

TABLE A.5. EFFECTS OF THE GOOD BARTENDERS PROGRAM ON OTHER SURVEY OUTCOMES

	Observations	Mean [SD]	(1) OLS	(2) OLS	(3) IV
Sells Water	578	0.84	0.046	0.054	0.087
		[0.36]	(0.029)	(0.030)	(0.047)
q-value			0.200	0.142	0.138
F-statistic					427.9
Sells non-alcoholic or light beer	578	0.98	-0.005	-0.008	-0.013
		[0.15]	(0.013)	(0.011)	(0.018)
q-value			0.815	0.541	0.543
F-statistic					427.9
Sells Food	578	0.81	0.048	0.049	0.078
		[0.39]	(0.032)	(0.032)	(0.050)
q-value			0.200	0.195	0.195
F-statistic					427.9
Sells packaged food	578	0.77	0.064	0.070	0.112
		[0.42]	(0.035)	(0.035)	(0.056)
q-value			0.200	0.142	0.138
F-statistic					427.9
Sells baked food	578	0.09	0.048	0.053	0.085
		[0.29]	(0.028)	(0.026)	(0.042)
q-value			0.200	0.142	0.138
F-statistic					427.9
Sells refrigerated food	578	0.23	0.055	0.067	0.107
		[0.42]	(0.038)	(0.035)	(0.057)
q-value			0.200	0.142	0.138
F-statistic					427.9
Sells prepared meals	578	0.18	0.060	0.046	0.073
		[0.39]	(0.035)	(0.033)	(0.053)
q-value			0.200	0.220	0.225
F-statistic					427.9
Requires payment by round	578	0.38	-0.001	0.006	0.010
		[0.49]	(0.042)	(0.040)	(0.064)
q-value			0.978	0.873	0.873
F-statistic					427.9
Controls			No	Yes	Yes

Source: Authors' elaboration from survey data.

Notes: Each row presents results from a separate regression. Clustered standard errors by street segment are shown in parentheses. The table reports cross-section estimates of the direct effects of the Good Bartenders program on survey outcomes (see Section IV). Columns (1) and (2) report OLS estimates, while column (3) instruments compliance with all seven mystery shopper criteria with random assignment (See Table A.4). Given that we estimate results for several outcomes using the same source of exogenous variation, we present *q*-values that adjust for multiple hypothesis testing, calculated using the method by Benjamini and Hochberg (1995) that controls for the false discovery rate (FDR) described in Anderson (2008).

TABLE A.6. EFFECTS OF THE GOOD BARTENDERS PROGRAM ON BRAWLS (ASSIGNED)

	(1)	(2)	(3)	(4)	(5)	(6)
Direct effect (D)	0.020	0.022	0.022	0.021	0.020	-0.009
	(0.074)	(0.041)	(0.040)	(0.039)	(0.039)	(0.047)
RI: $Pr(D = 0)$	0.538	0.538	0.534	0.546	0.562	0.848
Indirect effect (S)	0.037	0.024	0.023	0.031	0.006	0.009
	(0.043)	(0.020)	(0.020)	(0.019)	(0.017)	(0.018)
RI: $Pr(S = 0)$	0.140	0.140	0.138	0.034	0.686	0.610
RI: $Pr(D = S)$	0.980	0.980	0.986	0.768	0.676	0.712
Adjusted R^2	0.003	0.417	0.418	0.419	0.419	0.426
Mean brawls	0.886	0.886	0.886	0.886	0.886	0.886
SD brawls	1.857	1.857	1.857	1.857	1.857	1.857
Fixed-effects	Locality	Street	Street	Street	Street	Street
Controls	No	No	Yes	Yes	Yes	Yes
Linear time trends	No	No	No	Locality	Quadrant	Street
Observations	371,194	371,194	365,207	365,207	365,207	365,207
Street segments	5,987	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221	221

Source: Authors' elaboration from georerefenced administrative data.

Notes: In all columns, the outcome variable is the number of violent brawls. Each column presents results from a separate regression. Two-way clustered standard errors by street segment and police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of assignment into the Good Bartenders program (see Section IV). We also present p-values obtained by randomization inference with 500 replications (Heß, 2017), labeled RI, for three hypotheses: i) direct effects are zero or $Pr(D) = 0$, ii) indirect effects are zero or $Pr(S) = 0$, as well as whether iii) direct and indirect effects are equal or $Pr(D = S)$.

TABLE A.7. EFFECTS OF THE GOOD BARTENDERS PROGRAM ON BRAWLS (POISSON)

	(1)	(2)	(3)	(4)	(5)	(6)
Direct effect (D)	0.000	0.015	0.014	0.016	0.007	-0.017
	(0.085)	(0.047)	(0.046)	(0.045)	(0.043)	(0.053)
Indirect effect (S)	0.042	0.028	0.028	0.036	0.007	0.013
	(0.052)	(0.024)	(0.024)	(0.022)	(0.020)	(0.020)
Mean brawls	0.887	0.887	0.887	0.887	0.887	0.887
SD brawls	1.861	1.861	1.861	1.861	1.861	1.861
Fixed-effects	Locality	Street	Street	Street	Street	Street
Controls	No	No	Yes	Yes	Yes	Yes
Linear time trends	No	No	No	Locality	Quadrant	Street
Observations	371,194	371,194	365,207	365,207	365,207	365,207
Street segments	5,987	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221	221

Source: Authors' elaboration from georerefenced administrative data.

Notes: In all columns, the outcome variable is the number of violent brawls. Each column presents results from a separate regression. Two-way clustered standard errors by street segment and police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of the Good Bartenders program using Poisson regression (see Section IV).

TABLE A.8. EFFECTS OF THE GOOD BARTENDERS PROGRAM ON BRAWLS (LATE)

	(1)	(2)	(3)	(4)	(5)	(6)
A. Compliance adjustment						
Direct effect (D)	-0.104	-0.057	-0.055	-0.041	-0.039	-0.046
	(0.079)	(0.039)	(0.038)	(0.038)	(0.038)	(0.048)
Indirect effect (S)	0.028	0.018	0.018	0.026	-0.001	0.006
	(0.043)	(0.020)	(0.020)	(0.019)	(0.017)	(0.018)
F-statistic	38,219.6	37,612.1	37,625.4	39,154.3	14,247.3	37,010.4
B. Assigned & effective status						
Direct effect (D)	0.020	0.022	0.022	0.021	0.020	-0.009
	(0.075)	(0.041)	(0.040)	(0.040)	(0.039)	(0.047)
Indirect effect (S)	0.037	0.024	0.023	0.031	0.006	0.009
	(0.043)	(0.020)	(0.020)	(0.019)	(0.017)	(0.018)
F-statistic	524,699.6	521,385.6	520,173.3	524,926.3	521,265.9	513,017.8
Fixed-effects	Locality	Street	Street	Street	Street	Street
Controls	No	No	Yes	Yes	Yes	Yes
Linear time trends	No	No	No	Locality	Quadrant	Street
Mean brawls	0.886	0.886	0.886	0.886	0.886	0.886
SD brawls	1.857	1.857	1.857	1.857	1.857	1.857
Observations	371,194	371,194	365,207	365,207	365,207	365,207
Street segments	5,987	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221	221

Source: Authors' elaboration from georerefenced administrative data.

Notes: In all columns, the outcome variable is the number of violent brawls. Each column presents results from a separate regression. Two-way clustered standard errors by street segment and police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of the Good Bartenders program using instrumental variables regression (see Section IV), where we instrument compliance with all seven mystery shopper criteria with effective treatment (See Table A.4) in Panel A and instrument effective treatment status with assigned status in Panel B.

TABLE A.9. EFFECTS OF THE GOOD BARTENDERS PROGRAM ON BRAWLS (WEEKLY FREQUENCY)

	(1)	(2)	(3)	(4)	(5)	(6)
Direct effect (D)	-0.005	-0.003	-0.003	-0.003	-0.004	-0.009
	(0.015)	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)
RI: $Pr(D = 0)$	0.820	0.820	0.844	0.812	0.766	0.486
Indirect effect (S)	0.004	0.000	0.000	0.002	-0.004	-0.004
	(0.010)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
RI: $Pr(S = 0)$	0.972	0.972	0.958	0.744	0.486	0.552
RI: $Pr(D = S)$	0.818	0.818	0.832	0.720	0.998	0.702
Adjusted R^2	0.002	0.146	0.147	0.147	0.147	0.149
Controls	0.203	0.203	0.203	0.203	0.203	0.203
SD brawls	0.690	0.690	0.690	0.690	0.690	0.690
Fixed-effects	Locality	Street	Street	Street	Street	Street
Lagged brawls	No	No	Yes	Yes	Yes	Yes
Linear time trends	No	No	No	Locality	Quadrant	Street
Observations	1,616,490	1,616,490	1,610,503	1,610,503	1,610,503	1,610,503
Street segments	5,987	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221	221

Source: Authors' elaboration from georerefenced administrative data.

Notes: In all columns, the outcome variable is the number of violent brawls. Each column presents results from a separate regression. Two-way clustered standard errors by street segment and police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of the Good Bartenders program (see Section IV). We also present p-values obtained by randomization inference with 500 replications (Heß, 2017), labeled RI, for three hypotheses: i) direct effects are zero or $Pr(D) = 0$, ii) indirect effects are zero or $Pr(S) = 0$, as well as whether iii) direct and indirect effects are equal or $Pr(D = S)$.

TABLE A.10. EFFECTS OF THE GOOD BARTENDERS PROGRAM ON BRAWLS (ASSIGNED, WEEKLY FREQUENCY)

	(1)	(2)	(3)	(4)	(5)	(6)
Direct effect (D)	-0.002	-0.003	-0.002	-0.003	-0.003	-0.009
	(0.016)	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)
RI: $Pr(D = 0)$	0.856	0.856	0.876	0.850	0.812	0.522
Indirect effect (S)	0.004	0.000	0.000	0.002	-0.004	-0.004
	(0.010)	(0.006)	(0.006)	(0.007)	(0.006)	(0.007)
RI: $Pr(S = 0)$	0.976	0.976	0.962	0.734	0.486	0.564
RI: $Pr(D = S)$	0.840	0.840	0.848	0.750	0.954	0.724
Adjusted R^2	0.002	0.146	0.147	0.147	0.147	0.149
Controls	0.203	0.203	0.203	0.203	0.203	0.203
SD brawls	0.689	0.689	0.689	0.689	0.689	0.689
Fixed-effects	Locality	Street	Street	Street	Street	Street
Lagged brawls	No	No	Yes	Yes	Yes	Yes
Linear time trends	No	No	No	Locality	Quadrant	Street
Observations	1,616,490	1,616,490	1,610,503	1,610,503	1,610,503	1,610,503
Street segments	5,987	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221	221

Source: Authors' elaboration from georerefenced administrative data.

Notes: In all columns, the outcome variable is the number of violent brawls. Each column presents results from a separate regression. Two-way clustered standard errors by street segment and police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of assignment into the Good Bartenders program (see Section IV). We also present p-values obtained by randomization inference with 500 replications (Heß, 2017), labeled RI, for three hypotheses: i) direct effects are zero or $Pr(D) = 0$, ii) indirect effects are zero or $Pr(S) = 0$, as well as whether iii) direct and indirect effects are equal or $Pr(D = S)$.

TABLE A.11. EFFECTS OF THE GOOD BARTENDERS PROGRAM ON BRAWLS (POISSON, WEEKLY FREQUENCY)

	(1)	(2)	(3)	(4)	(5)	(6)
Direct effect (D)	-0.031	-0.019	-0.018	-0.017	-0.026	-0.044
	(0.091)	(0.071)	(0.070)	(0.071)	(0.073)	(0.071)
Indirect effect (S)	0.019	0.002	0.002	0.010	-0.019	-0.015
	(0.058)	(0.037)	(0.037)	(0.037)	(0.035)	(0.035)
Mean brawls	0.203	0.203	0.203	0.203	0.203	0.203
SD brawls	0.690	0.690	0.690	0.690	0.690	0.690
Fixed-effects	Locality	Street	Street	Street	Street	Street
Controls	No	No	Yes	Yes	Yes	Yes
Linear time trends	No	No	No	Locality	Quadrant	Street
Observations	1,616,490	1,616,490	1,610,503	1,610,503	1,610,503	1,610,503
Street segments	5,987	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221	221

Source: Authors' elaboration from georerefenced administrative data.

Notes: In all columns, the outcome variable is the number of violent brawls. Each column presents results from a separate regression. Two-way clustered standard errors by street segment and police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of the Good Bartenders program using Poisson regression (see Section IV).

TABLE A.12. EFFECTS OF THE GOOD BARTENDERS PROGRAM ON BRAWLS (LATE, WEEKLY FREQUENCY)

	(1)	(2)	(3)	(4)	(5)	(6)
A. Compliance adjustment						
Direct effect (D)	-0.041	-0.029	-0.029	-0.026	-0.025	-0.025
	(0.016)	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)
Indirect effect (S)	0.001	-0.002	-0.002	0.000	-0.006	-0.005
	(0.010)	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)
F-statistic	37,795.2	37,612.2	37,614.8	38,137.1	28,337.9	37,472.6
B. Assigned & effective status						
Direct effect (D)	-0.002	-0.003	-0.002	-0.003	-0.003	-0.009
	(0.016)	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)
Indirect effect (S)	0.004	0.000	0.000	0.002	-0.004	-0.004
	(0.010)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
F-statistic	522,561.2	521,386.7	521,359.6	524,331.6	521,366.7	519,461.7
Fixed-effects	Locality	Street	Street	Street	Street	Street
Controls	No	No	Yes	Yes	Yes	Yes
Linear time trends	No	No	No	Locality	Quadrant	Street
Mean brawls	0.203	0.203	0.203	0.203	0.203	0.203
SD brawls	0.689	0.689	0.689	0.689	0.689	0.689
Observations	1,616,490	1,616,490	1,610,503	1,610,503	1,610,503	1,610,503
Street segments	5,987	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221	221

Source: Authors' elaboration from georerefenced administrative data.

Notes: In all columns, the outcome variable is the number of violent brawls. Each column presents results from a separate regression. Two-way clustered standard errors by street segment and police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of the Good Bartenders program using instrumental variables regression (see Section IV), where we instrument compliance with all seven mystery shopper criteria with effective treatment (See Table A.4) in Panel A and instrument effective treatment status with assigned status in Panel B.

TABLE A.13. EFFECTS OF THE GOOD BARTENDERS PROGRAM ON ROBBERIES AND PERSONAL INJURY EVENTS

	(1)	(2)	(3)	(4)	(5)	(6)
A. Robberies						
Direct effect (D)	0.060	0.042	0.036	0.033	0.004	0.006
	(0.033)	(0.025)	(0.021)	(0.021)	(0.016)	(0.017)
RI: $Pr(D = 0)$	0.044	0.044	0.044	0.048	0.812	0.660
Indirect effect (S)	0.004	0.005	0.005	0.004	0.003	0.004
	(0.018)	(0.013)	(0.011)	(0.011)	(0.007)	(0.007)
RI: $Pr(S = 0)$	0.548	0.548	0.504	0.556	0.610	0.532
RI: $Pr(D = S)$	0.070	0.070	0.076	0.096	0.992	0.864
Adjusted R^2	0.024	0.159	0.177	0.178	0.199	0.252
Mean brawls	0.052	0.052	0.052	0.052	0.052	0.052
SD brawls	0.258	0.258	0.258	0.258	0.258	0.258
B. Personal injury events						
Direct effect (D)	0.010	0.006	0.005	0.004	-0.002	-0.000
	(0.005)	(0.005)	(0.005)	(0.004)	(0.006)	(0.006)
RI: $Pr(D = 0)$	0.314	0.314	0.344	0.366	0.704	0.994
Indirect effect (S)	0.008	0.005	0.004	0.004	0.000	-0.000
	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)
RI: $Pr(S = 0)$	0.024	0.024	0.028	0.040	0.992	0.928
RI: $Pr(D = S)$	0.846	0.846	0.878	0.896	0.702	0.966
Adjusted R^2	0.005	0.040	0.082	0.082	0.083	0.085
Mean brawls	0.024	0.024	0.024	0.024	0.024	0.024
SD brawls	0.190	0.190	0.190	0.190	0.190	0.190
Fixed-effects	Locality	Street	Street	Street	Street	Street
Controls	No	No	Yes	Yes	Yes	Yes
Linear time trends	No	No	No	Locality	Quadrant	Street
Observations	371,194	371,194	365,207	365,207	365,207	365,207
Street segments	5,987	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221	221

Source: Authors' elaboration from georerefenced administrative data.

Notes: In all columns, the outcome variable is the number of violent brawls. Each column presents results from a separate regression. Two-way clustered standard errors by street segment and police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of the Good Bartenders program on robberies and personal injury events from the SIEDCO database collected by the National Police (see Section IV). We also present p-values obtained by randomization inference with 500 replications (Heß, 2017), labeled RI, for three hypotheses: i) direct effects are zero or $Pr(D) = 0$, ii) indirect effects are zero or $Pr(S) = 0$, as well as whether iii) direct and indirect effects are equal or $Pr(D = S)$.

TABLE A.14. PLACEBO EFFECTS OF THE GOOD BARTENDERS PROGRAM ON BRAWLS AND OTHER INCIDENTS

	Brawls	Personal injury	Drunk and disorderly	Disturbing the peace	Illegal alcohol sales
A. Treatment occurs in August 2016					
Direct effect (D)	0.024	-0.002	-0.015	-0.024	0.001
	(0.028)	(0.013)	(0.014)	(0.065)	(0.008)
RI: $Pr(D = 0)$	0.420	0.882	0.216	0.710	0.944
q-value	0.918	0.918	0.918	0.918	0.918
Indirect effect (S)	0.019	-0.000	0.006	-0.035	0.002
	(0.016)	(0.007)	(0.006)	(0.032)	(0.005)
RI: $Pr(S = 0)$	0.118	0.944	0.286	0.238	0.616
q-value	0.572	0.995	0.572	0.572	0.905
RI: $Pr(D = S)$	0.878	0.918	0.098	0.882	0.934
Adjusted R^2	0.418	0.285	0.173	0.377	0.168
Mean outcome	0.902	0.278	0.156	1.003	0.115
SD outcome	1.853	0.936	0.513	2.292	0.464
B. Treatment occurs in August 2017					
Direct effect (D)	0.031	0.005	-0.008	0.019	0.005
	(0.030)	(0.015)	(0.017)	(0.072)	(0.008)
RI: $Pr(D = 0)$	0.284	0.782	0.556	0.800	0.616
q-value	0.800	0.800	0.800	0.800	0.800
Indirect effect (S)	0.022	0.007	0.007	-0.024	0.004
	(0.017)	(0.008)	(0.007)	(0.031)	(0.005)
RI: $Pr(S = 0)$	0.108	0.182	0.246	0.442	0.192
q-value	0.438	0.438	0.438	0.438	0.438
RI: $Pr(D = S)$	0.788	0.894	0.260	0.454	0.974
Adjusted R^2	0.418	0.285	0.173	0.377	0.168
Mean outcome	0.887	0.263	0.164	1.107	0.112
SD outcome	1.854	0.898	0.538	2.596	0.483
Observations	365,207	365,207	365,207	365,207	365,207
Street segments	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221

Source: Authors' elaboration from georerefenced administrative data.

Notes: Each column presents results from a separate regression. The reported specification is the same as in Column (3) of Table 4, which includes controls, the lagged outcome, and street-level fixed effects. Two-way clustered standard errors by street segment and police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of the Good Bartenders program on placebo treatments, assuming that treatment occurs in August 2016 in Panel A and August 2017 in Panel B. We also present p-values obtained by randomization inference with 500 replications (Heß, 2017), labeled RI, for three hypotheses: i) direct effects are zero or $Pr(D) = 0$, ii) indirect effects are zero or $Pr(S) = 0$, as well as whether iii) direct and indirect effects are equal or $Pr(D = S)$. Given that we estimate results for several outcomes using the same source of exogenous variation, we present q-values that adjust for multiple hypothesis testing, calculated using the method by Benjamini and Hochberg (1995) that controls for the false discovery rate (FDR) described in Anderson (2008).

TABLE A.15. FALSIFICATION TESTS OF THE GOOD BARTENDERS PROGRAM ON HOMICIDES

	(1)	(2)	(3)	(4)	(5)	(6)
Direct effect (D)	-0.001	-0.001	-0.001	-0.001	-0.001	-0.000
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
RI: $Pr(D = 0)$	0.436	0.436	0.412	0.422	0.180	0.776
Indirect effect (S)	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
RI: $Pr(S = 0)$	0.256	0.256	0.268	0.264	0.572	0.734
RI: $Pr(D = S)$	0.212	0.212	0.210	0.210	0.124	0.696
Adjusted R^2	0.000	0.003	0.003	0.003	0.003	0.004
Controls	0.001	0.001	0.001	0.001	0.001	0.001
SD brawls	0.040	0.040	0.040	0.040	0.040	0.040
Fixed-effects	Locality	Street	Street	Street	Street	Street
Lagged brawls	No	No	Yes	Yes	Yes	Yes
Linear time trends	No	No	No	Locality	Quadrant	Street
Observations	371,194	371,194	365,207	365,207	365,207	365,207
Street segments	5,987	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221	221

Source: Authors' elaboration from georerefenced administrative data.

Notes: Each column presents results from a separate regression. The reported specification is the same as in Column (3) of Table 4, which includes controls, the lagged outcome, and street-level fixed effects. Two-way clustered standard errors by street segment and police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of the Good Bartenders program on homicides from the SIEDCO database collected by the National Police (see Section IV). We also present p-values obtained by randomization inference with 500 replications (Heß, 2017), labeled RI, for three hypotheses: i) direct effects are zero or $Pr(D) = 0$, ii) indirect effects are zero or $Pr(S) = 0$, as well as whether iii) direct and indirect effects are equal or $Pr(D = S)$.

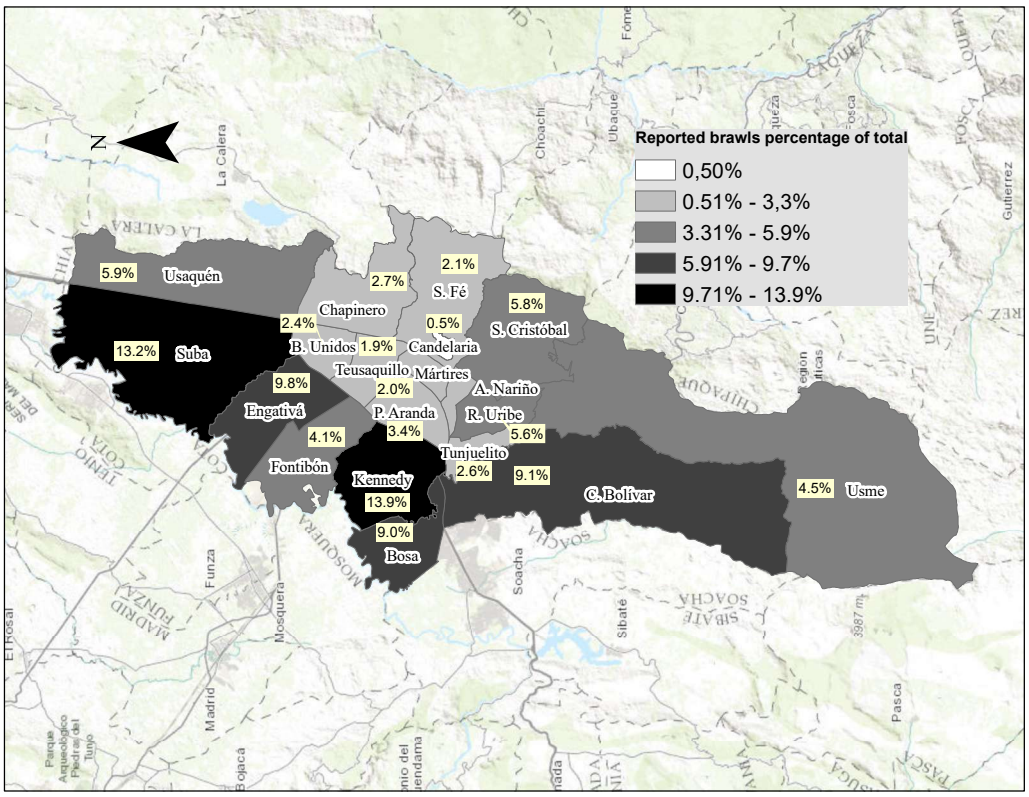


FIGURE A.1. DISTRIBUTION OF REPORTED BRAWLS BY LOCALITY IN BOGOTÁ

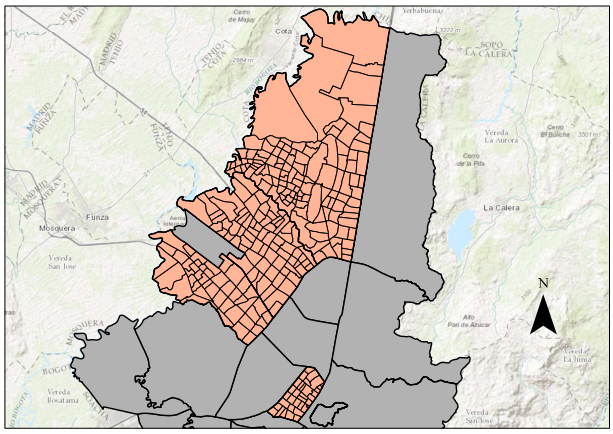
Source: Authors' elaboration from georeferenced administrative data for 2017.

FIGURE A.2. SELECTED PAGES FROM THE GOOD BARTENDERS MANUAL (IN SPANISH)

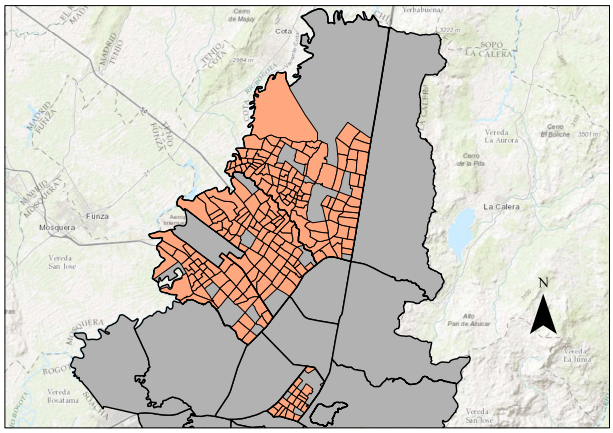
Source: Authors' elaboration from program materials.

FIGURE A.3. MATERIALS GIVEN TO PARTICIPATING BARS (IN SPANISH)

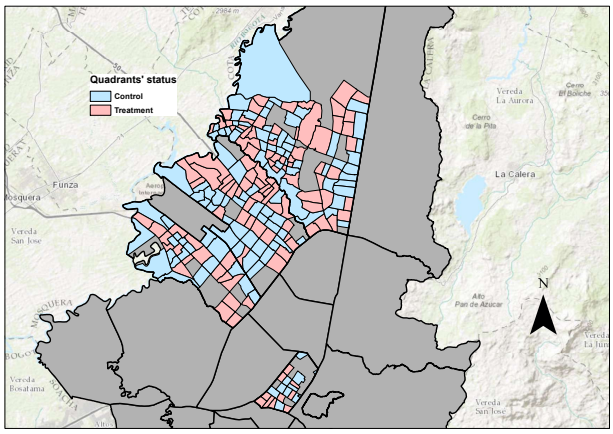
Source: Authors' elaboration from program materials.



(A) ALL POLICE QUADRANTS



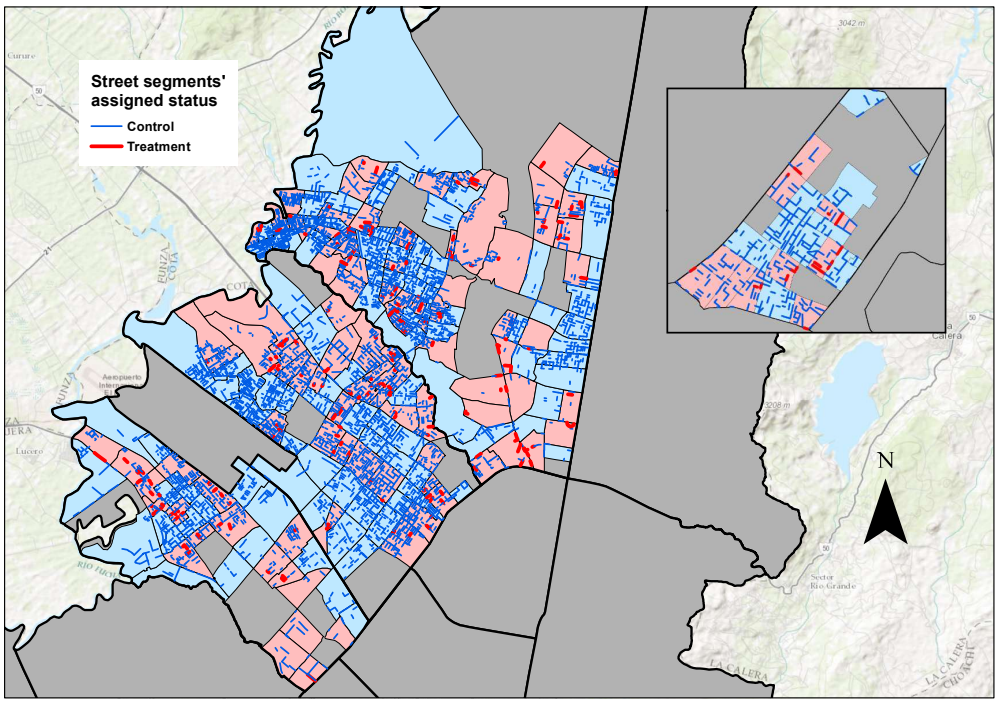
(B) ELIGIBLE POLICE QUADRANTS



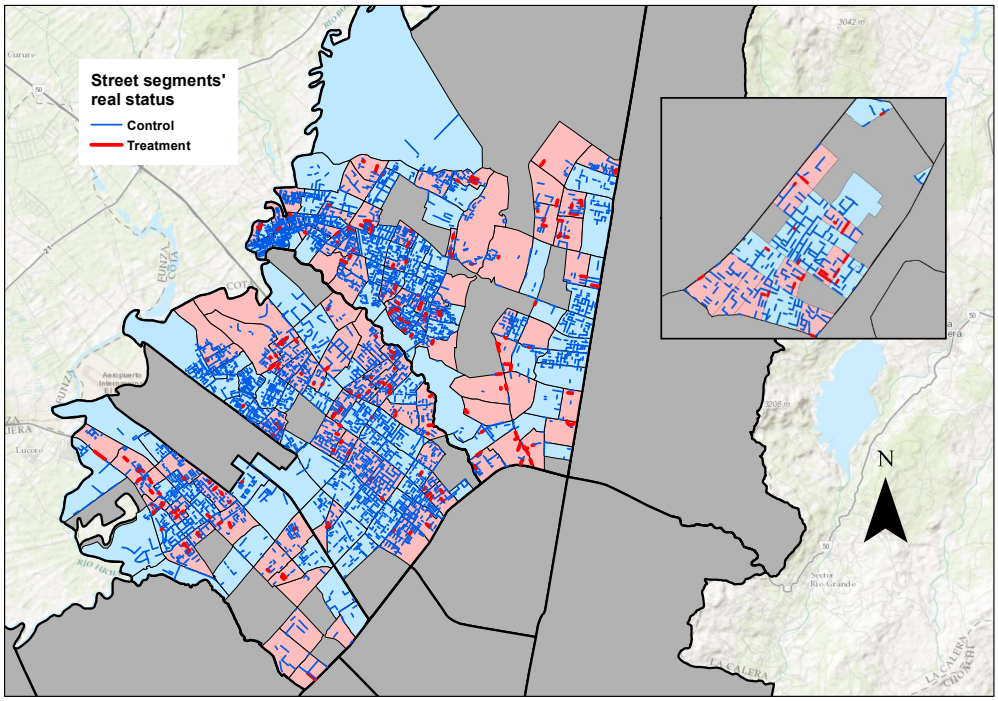
(C) TREATMENT AND CONTROL POLICE QUADRANTS

FIGURE A.4. SELECTION OF QUADRANTS INTO INTERVENTION

Source: Authors' elaboration from georeferenced administrative data.



(A) ASSIGNED TREATMENT STATUS



(B) EFFECTIVE TREATMENT STATUS

FIGURE A.5. DIFFERENCES BETWEEN ASSIGNED STATUS AND EFFECTIVE TREATMENT

Source: Authors' elaboration from georeferenced administrative data.
Notes: Blue areas denote control quadrants and red areas denote treatment quadrants. Blue lines represent control street segment and red lines depict treated control segments. Due to scale, Los Mártires is shown in the small block on the upper right side.

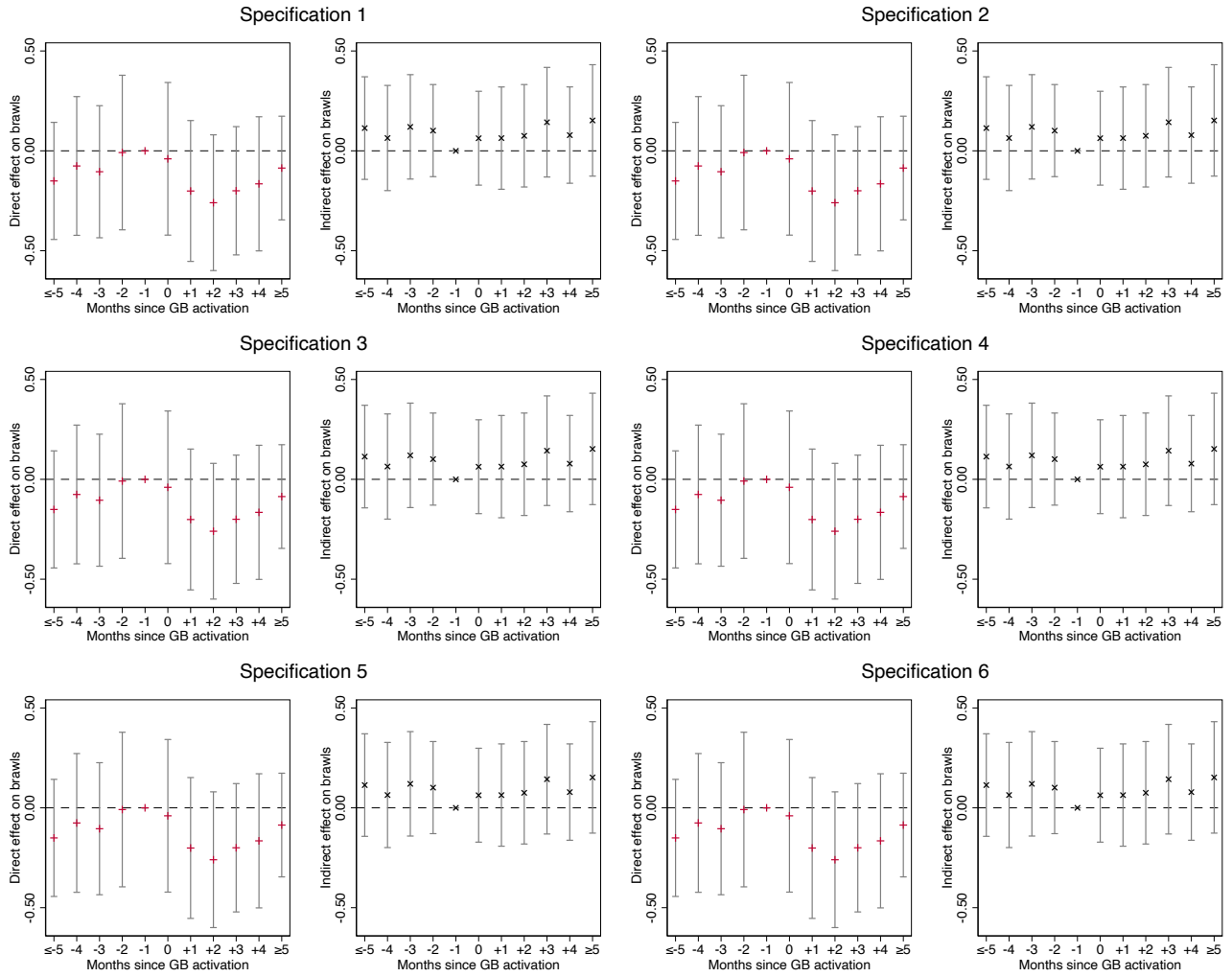


FIGURE A.6. EVENT STUDY EFFECTS OF THE GOOD BARTENDERS PROGRAM ON BRAWLS (ALL SPECIFICATIONS)

Source: Authors' elaboration from georerefenced administrative data.
Notes: Direct effects shown as "+" and indirect effects as "x". 95% confidence intervals shown around the point estimates.

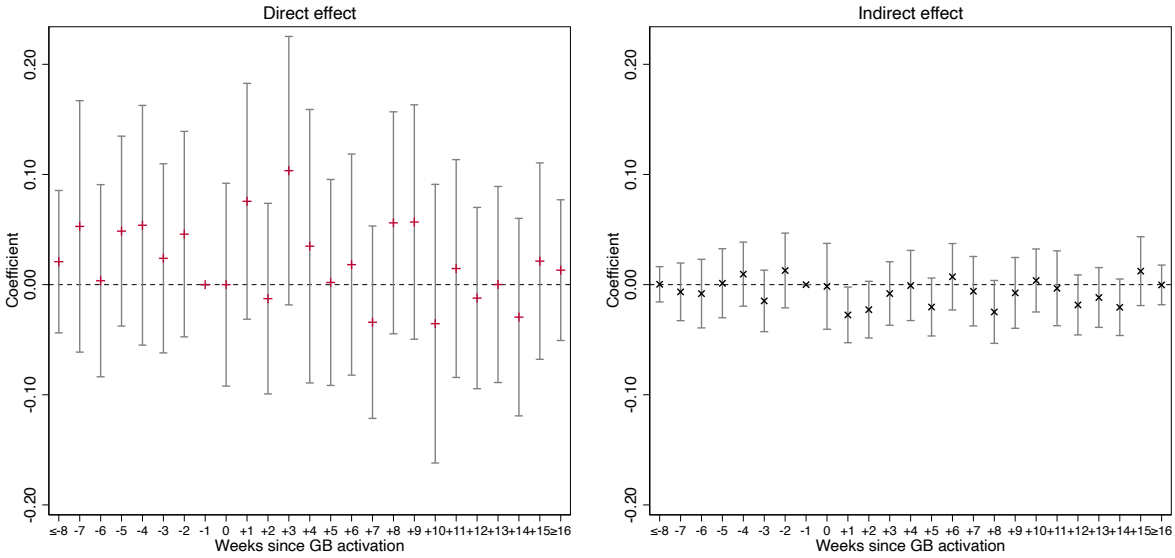


FIGURE A.7. EVENT STUDY EFFECTS OF THE GOOD BARTENDERS PROGRAM ON BRAWLS (WEEKLY)

Source: Authors' elaboration from georerefenced administrative data.
Notes: Direct effects shown as "+" and indirect effects as "x". 95% confidence intervals shown around the point estimates.

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