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

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Abstract

This paper evaluates the randomized Good Drinks program in four localities of Bogotá, Colombia. The intervention encourages bartenders to adopt standardized practices that promote responsible behavior in terms of alcohol consumption with the goal of reducing alcohol-related violence and was implemented in cooperation with Colombia's largest brewery and the city's Secretariat of Security, Coexistence, and Justice. Tracing out the relationship between alcohol consumption and violence is useful because alcohol-related incidents often lead to more serious crimes. Our experimental 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 behavior by patrons. However, we find no direct or spillover effects of these changes in consumption on brawls five months after the program, but some improvement on other alcohol-related incidents. The experience of the Good Drinks program provides a better understanding of three aspects related to alcohol regulation and policy: (i) the role bartenders can play to curb excessive alcohol consumption and promote good behavior among customers, (ii) a practical experience of using less restrictive interventions for alcohol regulation, and (iii) the value of public-private partnerships. © *2021 by the Association for Public Policy Analysis and Management*

INTRODUCTION

Excessive alcohol consumption is common globally and is generally associated with negative consequences for individuals and society. 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), sexually transmitted diseases (Carpenter, 2005), psychological disorders including suicide and depression (Sher, 2005), as well as criminal activity (Gerson & Preston, 1979; Markowitz, 2005; Zhang, Wieczorek, & Welte, 1997). A recent meta-analysis found that three million deaths in 2016 were directly related to alcohol consumption (Griswold et al., 2018), not including those caused by alcohol-attributable violence. Alcohol-related violence is estimated to account for 248,000 annual deaths globally (Graham & Livingston, 2011), and observational studies frequently find a positive correlation between alcohol consumption and violence such as aggravated assault, domestic violence, and motor vehicle accidents (Card & Dahl, 2011; Carpenter & Dobkin, 2009; Rossow, 2001). Alcohol can make individuals more aggressive—due to loss of inhibitions and heightened emotional reactions—thereby increasing their

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probability of committing interpersonal violence, while alcohol also has sedative effects at high doses, making those who drink heavily easy targets for crime (Carpenter & Dobkin, 2011). Causal evidence on how to circumvent these ills often associated with alcohol is therefore crucial for policymakers.

This paper provides new evidence on the relationship between alcohol consumption and alcohol-attributable violence by evaluating the Good Drinks program in Bogotá, Colombia,[1] 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). The intervention was designed to: (i) provide bartenders with standardized practices that promote responsible alcohol consumption among their patrons, and (ii) furnish information and strategies to bartenders on how to defuse conflicts that may result in alcohol-related violence within and around their bars.

Alcohol-related violence is widespread in Bogotá. Incidents such as aggravated assault or brawls are commonly reported within and around bars. At least 62 percent of brawls result in more serious crimes, such as personal injury, 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 may reduce reported brawls and other alcohol-related incidents. The set of practices provided by the Good Drinks program draws from medical evidence showing that responsible consumption, including consuming water and food while drinking, reduces blood-alcohol content and delays rapid inebriation, with the potential to mitigate alcohol's adverse effects (Carpenter & Dobkin, 2011; Parrott & Eckhardt, 2018; Paton, 2005; Roine et al., 1993; Swift, 2003). On an empty stomach, it takes approximately 30 minutes for alcohol in one standard drink to enter the bloodstream, while it takes 60 minutes for the same to occur on a full stomach (Youngerman et al., 2005).

We test the effectiveness of the Good Drinks program via a randomized experiment in four localities of Bogotá.[2] Our unit of analysis is the street segment, a road that lies between two city block intersections. Street segments were randomly chosen in a two-stage design that allows us to estimate both the direct and indirect effects of the program. This experimental design avoids contamination issues prevalent in location-based interventions and overcomes challenges found in dense urban environments like Bogotá. We compare outcomes for three groups in a sample of 5,987 street segments: 228 that were directly treated; 2,730 indirectly treated; and 3,029 pure control locations. We first study whether the intervention led to any changes in bartender practices and patron outcomes using survey data 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 violent events merged to bar locations.

Results indicate that the Good Drinks program changes bartender practices and promotes more responsible alcohol consumption. While alcohol sales were unchanged in treatment bars, these bartenders sold more food (67 percent increase) and water (56 percent increase) after the intervention, compared to control bars. These changes are driven by intensive margin adjustments: bars that already sold food and water increased their sales. Estimated effects are even larger when we

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

adjust for compliance among treated bars. 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, these effects are not statistically significant. These null effects on perceptions are in line with results from administrative data on violence: we find no direct or spillover effects of the program on violent brawls using different model specifications, treatment definitions, and estimation procedures. Given the *ex ante* statistical power of the experiment and the precision of our estimates, we are confident the program has zero treatment effects on brawls.

We do, however, find some direct effects on alcohol-related incidents related to coexistence outcomes. Reports of drunk and disorderly conduct fall by 22.3 percent and disturbances to the peace by 16.6 percent, with few spillovers to indirectly treated areas. These findings are robust to randomization inference, multiple hypothesis adjustments, and are validated by placebo experiments in which we change treatment timing. Overall, the program did encourage bartenders to promote responsible alcohol consumption. However, these changes did not result in fewer reports of brawls but did lead to reductions for other types of alcohol-related incidents.

These results contribute new evidence on the relationship between alcohol consumption and alcohol-related violence. At the time of writing, we are unaware of similar programs with the same objective, scale, and form of delivery as the Good Drinks program. The experience of this program provides a better understanding of three aspects related to alcohol consumption and alcohol-attributable violence: (i) the role of bartenders in alcohol policies, (ii) the experience of using less restrictive interventions to regulate alcohol, and (iii) the value of public-private partnerships for designing, implementing, and evaluating policy.

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 also on the community (Aguirre-Molina & Gorman, 1996; Hawkins, Catalano, & Arthur, 2002; Holder, 2000). In the Colombian context, bartenders are akin to community leaders because they perform important social control functions, and their customers are usually regulars (Córdoba, 2018). Our results indicate that bartenders may play an important role in shaping alcohol consumption patterns in bars, suggesting that existing harm reduction strategies can be expanded to include different actors and interventions. However, we find suggestive evidence that such interventions may require more time to generate behavioral changes on a larger scale and modify social norms towards more desirable outcomes.

Most alcohol-related policies are traditionally restrictive (Nicholls, 2016).[3] This approach has yielded positive results but infringes upon individual liberties and often generates unintended consequences (Adinoff, 2016; Cook, 2007; Fernandez, Gohmann, & Pinkston, 2018). It may be that these policies do not address the underlying issues motivating abuse of alcohol. Our findings suggest that less restrictive efforts are not a silver bullet to reduce the consequences of excessive alcohol

[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 about the dangers of over-consumption, and encouraged responsible drinking (Anderson, Crost, & Rees, 2018; Carpenter & Dobkin 2011; Grönqvist & Niknami, 2014; Hansen, 2015; Heaton, 2012; Lovenheim & Steefel, 2011; Luca, Owens, & Sharma, 2015; Marcus & Siedler, 2015; Markowitz et al., 2012; Pridemore & Snowden, 2009).

consumption but may represent one path towards more sustainable solutions to heavy drinking.

Partnerships between the state and the private sector, as in the Good Drinks program, are essential to design, implement, and evaluate public policies beyond alcohol regulation. While the effectiveness of these partnerships has been mixed across different areas (Fabre & Straub, 2019), the program we study here would not have been feasible without such collaboration. Promoting cooperation between governments, business, and academics may produce more effective policies.

The remainder of this paper is organized as follows. The following section provides background on alcohol-related violence and alcohol consumption in Bogotá, and their relationship. The third section describes the Good Drinks program and the design of our field experiment to evaluate its effects. The fourth section presents the data sources and empirical strategy that we employ to estimate the impact of the program. The fifth section presents our empirical results and the sixth section explores the potential mechanisms that explain these findings. We conclude in the seventh section.

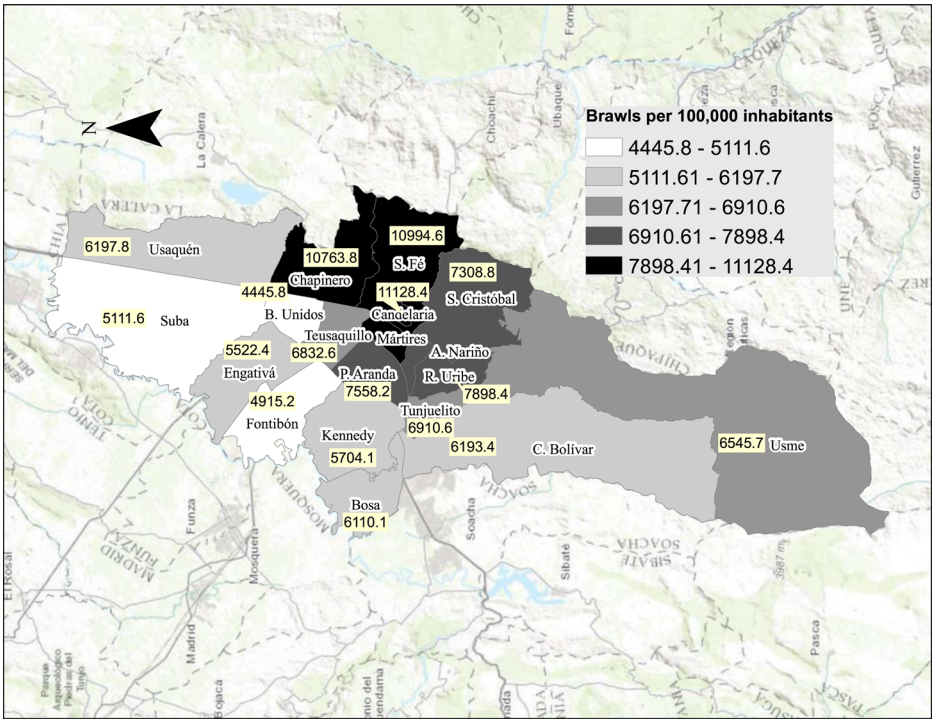
VIOLENCE AND ALCOHOL CONSUMPTION IN BOGOTÁ

Latin America is the world's most dangerous region and Colombia one of its most violent countries (UNODC, 2019). 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 years during 2018, at 12.7 per 100,000 people, a 43 percent reduction compared to 2012. Homicide rates in Bogotá are now the lowest among major Colombian cities and tend to be 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 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.

A core concern for the SSCJ in Bogotá has been the growth in alcohol-attributable violence, including brawls. Data on reported brawls are collected through the emergency services number (NUSE 123, for its acronym in Spanish) and are defined almost identically to aggravated assault in the United States.[4] Figure 1 maps the total number of reported brawls per 100,000 people across Bogotá's 18 urban localities during 2017. The figure shows high rates of reported assaults across the city, as well as considerable heterogeneity across localities. The mean aggravated assault rate for metropolitan areas in the United States was 248.9 per 100,000 people in 2017 (U.S. Department of Justice, 2017). Reports of assault in Bogotá for the same year are 10 times higher than in cities such as Chicago (570.4 per 100,000 people) and New York (345.5 per 100,000 people).

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 (panel B) frequencies. These trends also reveal cyclical behavior. Brawls are more common in March, May, and December, which coincide with Easter, Mother's Day, and Christmas and New Year's, respectively. Analyzing data for 2017, we also identify daily and time-based patterns: brawls tend to occur on weekends (panel C), with a monotonic increase that begins on Friday and ends on Sunday. Brawls are most

[4] 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).



Source: Authors' elaboration from georeferenced administrative data.

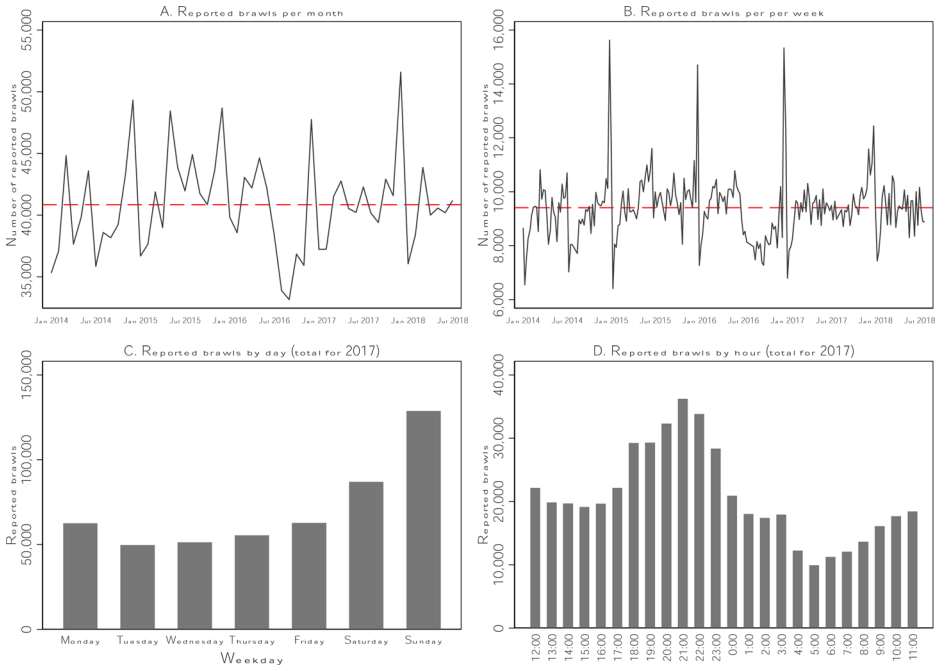
Figure 1. Spatial Distribution of Reported Brawls in Bogotá.
[Color figure can be viewed at wileyonlinelibrary.com]

commonly reported in the evening, between the hours of 8:00 and 11:00 p.m. (panel D).

These statistics suggest that brawls in Bogotá follow systematic patterns. They often occur during times of celebration, on weekends, and early in the evening. The intersection of these attributes likely 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.[5] There is also significant heterogeneity in alcohol consumption across Bogotá's localities (Bogotá Mayor's Office et al., 2016).[6]

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 bars and brawls in the city from 2014 to 2017. There is considerable overlap between areas that report more brawls and the availability of alcohol-serving establishments. This suggestive relationship may be due to several factors, including population density, as well as the concentration of alcohol-serving establishments in the city. This figure

[5] According to data from 2014, there are approximately 2.1 million consumers of alcoholic beverages in the city (Ministry of Justice and Law et al., 2014); 18- to 24-year-olds have the highest rate of alcohol consumption (50.7 percent), followed by 25- to 34-year-olds (45.9 percent).
[6] For instance, localities with the highest prevalence of alcohol consumption in the city are Suba (46.5 percent), Chapinero (43.8 percent), and Usaquén (42.5 percent), followed by the joint zone of Santa Fé, Los Mártires, and Candelaria (34.9 percent).



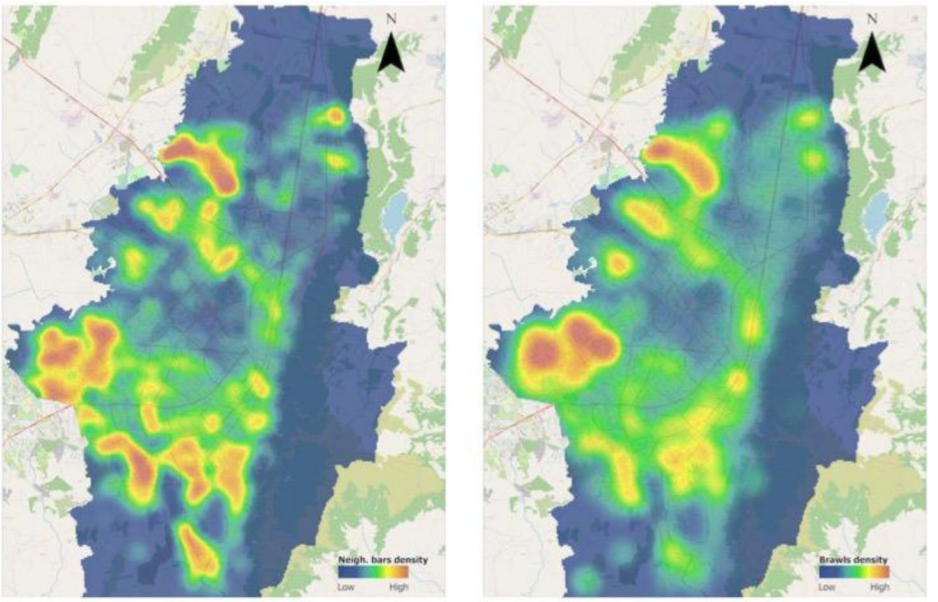
Source: Authors' elaboration from georeferenced administrative data.
Notes: The top panels (A and B) present trends from January 2014 to July 2018. The bottom panels (C and D) present aggregate statistics for 2017.

Figure 2. Attributes of Reported Brawls in Bogotá.
[Color figure can be viewed at wileyonlinelibrary.com]

only captures the correlation between alcohol availability and brawls, a relationship that we attempt to disentangle in the remainder of our study.

To better contextualize these trends in brawls and their correlation to bar locations, the SSCJ carried out an ethnographic study between December 2017 and January 2018 to explore the relationship between alcohol consumption and violence in four localities: Suba, Engativá, Fontibón, and Los Mártires (Córdoba, 2018). The study identified several causes of brawls: intolerance due to excessive alcohol consumption, consumption of drugs along with alcohol, incidents of jealousy and machismo between patrons, and "tribal" behavior between regular customers and strangers. Bartenders in general reported that they felt unequipped to deal with alcohol-related violence within and around their establishments, due to a lack of standardized practices and information on how to react. Most bartenders implemented ad hoc strategies to avoid conflict: denying entry to certain patrons, using price discrimination to drive out unwanted customers, installing security systems, or hiring private security firms. Bartenders were also generally unaware of how to involve the police when incidents occurred within or around their bars.

Bartenders also revealed that most of their clients were regulars, usually neighbors. Several mentioned that if regulars were drinking too much, they would stop serving them or encourage them to go home. In this sense, bartenders in neighborhood bars in Bogotá are akin to community leaders because they engage in social control. Most alcohol-related interventions often focus on consumers (Schilbach, 2019), but this ethnographic evidence suggests that bartenders may have a role to play in curbing the negative consequences of excessive alcohol consumption.



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

Figure 3. Alcohol Availability and Reported Brawl Density in Bogotá.
[Color figure can be viewed at wileyonlinelibrary.com]

EXPERIMENTAL SETTING

We carried out a field experiment in the aforementioned localities: Suba, Engativá, Fontibón, and Los Mártires. These localities are home to 2.6 million people, about one third of the city's total population, and were selected because they account for more than a quarter of all reported brawls in the city (see Figure A1 in the Appendix).[7] The main objectives of our intervention are to test whether bartenders who adopt standardized practices in their establishments can promote more responsible alcohol consumption by patrons, and whether these practices result in lower levels of alcohol-attributable brawls and other incidents within and around bars. If bartenders' actions change alcohol consumption practices among patrons, we will have access to crucial information about the causal relationship between alcohol consumption and alcohol-related violence. We first present details on the program and then proceed to describe its randomized implementation.

The Good Drinks Program

The Good Drinks (*Buenos Tragos* in Spanish) 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 defuse conflicts that may escalate and result in alcohol-related incidents within and around bars.

[7] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

There are three main types of alcohol-serving establishments in Bogotá: neighborhood bars, commercial bars, and clubs.[8] Our study focuses on violence within and around neighborhood bars for two reasons. First, Figure 2 shows that brawls often occur between 8:00 and 11:00 p.m., which coincides with times when these bars are open and serving alcoholic beverages. Second, commercial bars and clubs tend to cluster in areas that may experience different dynamics of violence (Francesconi & 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. To study the relationship between alcohol consumption and alcohol-related violence more broadly, neighborhood bars are better suited than other types of establishments where brawls may be caused by factors besides alcohol (e.g., gangs, drugs, and lethal violence).

The intervention consists of four steps. First, initial contact is made with bars selected into treatment. During an initial visit, the implementation team introduces the program and asks the bartender whether he or she would like to participate. If the bartender refuses, the team proceeds to visit the next bar on the list of randomly selected establishments. To avoid sample selection issues due to bartenders' choice of whether to participate or not, our design randomly selected twice as many bars as necessary to offer involvement in the program. If a bar refuses to participate, we continue down this randomized list until arriving at the total number of treated bars—270, in our case—that was defined *ex ante* in agreement with our implementation partners (Ham et al., 2018).

Second, the team distributes and explains the Good Drinks manual to participating bartenders. This manual provides information on recommended practices to avoid brawls and other alcohol-related incidents due to patrons' heavy drinking. On the one hand, the manual suggests that bartenders offer patrons food and water to control the rapid over-consumption of alcohol. For instance, it states that alcohol dehydrates the body and that consuming water between drinks helps maintain healthy levels of hydration. Similarly, it stresses how eating while drinking helps alcohol enter the body at a slower rate, diminishing its effects. It also recommends that bartenders discourage mixing different kinds of alcohol (such as beer, wine, and spirits), encourage dancing, designate a sober friend to look out for other members of the group, and promote a calm environment among patrons. The manual proceeds to classify common types of "drunk customers" and how best to approach each kind of person to mitigate potential conflicts. The manual ends by reminding bartenders that these practices can help maintain a respectful, non-violent environment and provides suggested procedures for contacting police. Selected pages of this manual (in Spanish) are shown in Figure A2.[9]

After introducing and explaining the Good Drinks 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 11:00 p.m. is last call for drinks; and a water pitcher that contains the logo

[8] Neighborhood bars are small establishments with tables and chairs that are allowed to sell alcohol between 10:00 a.m. and 11:00 p.m. They often also offer cleaning products, fresh produce, and food and, as such, are essentially corner shops with a liquor license and seating. Commercial bars are larger venues that mainly sell alcohol and can remain open until 3:00 a.m., while clubs operate similarly but some may remain open until 5:00 a.m. While some of the latter two types of establishments sell food on their premises, most of them do not.

[9] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

of the intervention and the slogan, "If you feel things are burning up [slang for getting drunk], extinguish with water." Pictures of these materials are shown in Figure A3.

Third, on a Thursday, Friday, or Saturday night, between the hours of 6:00 and 11:00 p.m., team members arrive at treated bars and "activate" them. They offer free food and a pitcher of water to patrons who are consuming alcohol. The food includes standard pub fare in Colombia, specifically sausages and chips. At the same time, team members show bartenders how to implement suggested practices from the manual in their own establishments. Bars also receive coasters with the logo of the intervention for use by patrons, and a sticker is placed in a visible location indicating that the establishment serves "Good Drinks." This process is carried out twice in each treated bar, with approximately 30 days between the activations. Finally, the bartender is told that a "mystery shopper" and a survey firm will visit the bar in a few weeks to follow up.

The fourth and final step is the monitoring phase. A mystery shopper visits each treated bar twice to evaluate progress and compliance with the intervention. The first visit occurs two weeks after the first activation and the second two weeks after the second activation. These mystery shoppers use seven criteria to measure compliance and quiz the bartender on specifics from the Good Drinks manual.[10] The mystery shopper records this information and gives the bartender raffle tickets to win a jukebox, according to each bar's level of compliance with the program.[11] A survey firm also visited all treated bars to collect post-treatment data about one month after the intervention had concluded in all establishments. The survey data are described 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, as this event coincides with increased alcohol consumption (Collin & 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 Table A1).[12] Mystery shopper visits were carried out throughout October and the survey firm collected data during November. The average duration of the program was 90 days, inclusive of both activations and follow-up visits but exclusive of the endline survey.

Randomization

We focus on street segments as our unit of analysis. Since the qualitative evidence shows that alcohol-related brawls in Bogotá often move from bars into streets (Córdoba, 2018), and because there tends to be more than one

[10] The seven criteria include: (i) having the sticker for participating in the program in a visible location, (ii) placing the clock that reminds patrons of last call in a prominent location, (iii) placing the welcome mat at the bar's entrance point, (iv) placing a provided picture frame with the intervention logo on the wall, (v) having and using the provided water pitcher, (vi) using the provided coasters, and (vii) offering food and water to patrons who are consuming alcohol. The factual question was randomly selected from a pre-selected bank of quiz questions.

[11] All treated bars were given one ticket for the jukebox raffle upon their acceptance to participate. Up to 14 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.

[12] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

bar per street, we use a two-stage randomization procedure to avoid contamination issues. 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 not belonging 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.

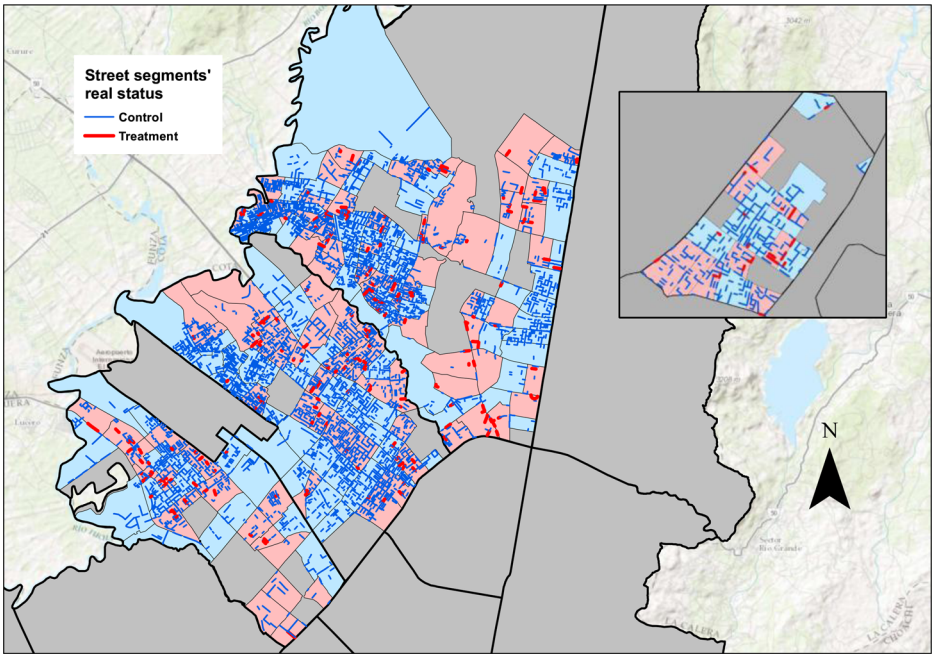
Randomized assignment was carried out in two stages, both stratified by locality. The first stage randomly selects treatment and control police quadrants, geographical units in the city chosen for security management purposes. Bogotá is composed of 1,051 police quadrants across 18 urban localities. Quadrants were selected in two phases. First, quadrants with at least two pre-selected street segments (those with at least one reported brawl in the preintervention period and one bar at a distance equal to or less than 100 meters) were chosen. Second, using the Statistical, Criminal, Contravention, and Operational Information System (SIEDCO, for its acronym in Spanish) from the National Police, we discarded the most dangerous decile of quadrants in the city for homicides given that brawls in these areas may be driven by factors other than alcohol consumption.[13] From a universe of 271 police quadrants in the four targeted localities, 221 quadrants were eligible under these criteria. The assignment procedure classified these locations into 109 treatment and 112 control quadrants.[14]

The second stage randomly selects 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 to be able to detect an effect just below 0.30 standard deviations for brawls (Ham et al., 2018). As noted above, we selected twice the required number of street segments to guarantee reaching our target number of treated units, since bartenders could choose whether to participate or not. Figure 4 maps treated and control street segments within police quadrants for each locality. Blue areas represent control quadrants and red areas are 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 received the intervention (see Figure A5 in the Appendix for a side-by-side visual comparison). Comparisons between assigned and effective treatment status show no systematic differences in terms of brawls and most alcohol-related incidents (see Table A2). We account for this discrepancy in our empirical analysis by presenting treatment effect estimates that instrument effective treatment with the original randomized assignment.

This two-stage design allows us to estimate both the direct and spillover effects of the program. By comparing outcomes in treated and control street segments within treated quadrants, we estimate the direct impact of the intervention. However, if bar owners in the same police quadrant discuss the intervention among themselves, then our estimates would be biased. If the intervention does not reduce brawls but

[13] We assessed different exclusion criteria, including removing the most dangerous 20 percent, 10 percent, and 5 percent street segments. The mean and standard deviation for brawls did not change substantially under these different scenarios, so we opted for the 10 percent threshold to maximize statistical power and minimize any safety concerns during implementation.

[14] Figure A4 maps each step of the first-stage randomization procedure. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.



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.

Figure 4. Randomized Design for Good Drinks Program.
[Color figure can be viewed at wileyonlinelibrary.com]

displaces them to nearby streets in the same quadrant, our estimates would also be biased. To avoid such issues, we compare outcomes between control street segments across treated and control police quadrants, as the latter 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). Our pre-analysis plan presents full 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.[15]

[15] The standard deviation of violent brawls before the program in control areas is 1.861, which implies that we can detect a coefficient of 0.27 x 1.861 ≈ 0.50 for direct effects and 0.3 x 1.861 ≈ 0.55 for indirect effects on violent brawls from a regression with an R^2 coefficient of zero according to our *ex ante* calculations (Ham et al., 2018). This minimum detectable effect increases with the fit of the corresponding regression. We discuss this point further in the section on Results.

DATA AND EMPIRICAL STRATEGY

Data

We employ two sources of data. First, we gather survey data in directly treated and control bars to study whether intervention produced changes in bartender practices and patron behavior. Second, we use georeferenced administrative data on reported incidents from the SSCJ, and matched bar locations from the brewery's client list, to estimate the direct and indirect effects of the Good Drinks program on alcohol-related violence.

The survey was carried out by a private firm, hired through a public procurement process, with prior experience interviewing bartenders. The survey data were collected after the program ended due to budget constraints. We visited all bars located on treated street segments. Of 270 treated establishments in 228 street segments, 258 bars provided complete information, a response rate of 96 percent. The remaining 12 treated bars refused to answer the survey or had changed ownership between the intervention and data collection and were unaware of the Good Drinks program. We compare survey respondents and non-respondents but find no systematic differences between them (see Table A3).[16] Additionally, we surveyed 320 bars—selected at random—located on pure control street segments. We did not collect survey data for bars on indirectly treated street segments. Our survey sample therefore consists of 578 bars on 482 street segments.

The survey instrument provides self-reported data on the value of alcohol, water, and food sales, each over 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 it hires private security, the kind of alcohol served, whether non-alcoholic beverage options are available, and the types of food served), bartender characteristics (e.g., whether he or she is the owner, whether he or she lives in the neighborhood, educational attainment, years of experience as a bartender), and bar 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 characteristics of the street segment, 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 (and if so, what kind), and whether there are informal street vendors. Taken together, these data provide rich and unique insights into bars, bartenders, and patrons in the four selected localities.

Table 1 shows descriptive statistics from the survey data for treatment and control groups. We present means and standard deviations, as well as the p-value from a test for equality between group 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 (distributed across 5 tables with 4 chairs per table), have bathrooms, and entertainment systems (e.g., music, television). Few bars report paying for private security. Bartenders are often female, usually the owners of the bar, married, have at most completed high school, and have accumulated more than a decade of experience tending bars 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

[16] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

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	12.52	(12.00)	11.46	(10.85)	0.304

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Table 1. (Continued).

C. Patron characteristics					
Fraction of regulars	0.92	(0.28)	0.88	(0.33)	0.142
Number 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 F-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.

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alcohol-serving establishments. About 22 percent of bars are located on a street corner. Averages are statistically equal between treated and control bars at the 5 percent level, and a joint F-test including all variables rejects their shared significance on treatment status (p-value = 0.081), confirming balance between treatment and control bars.

Reports of alcohol-attributable violence are provided by the Unique Number for Security and Emergencies database (NUSE 123, for its acronym in Spanish), which collects information on citizens' reports of violent incidents across the city. NUSE 123 receives calls made by citizens requesting emergency assistance, classifies them according to the emergency, and assigns them to the relevant agencies for attention.[17] When a person calls the emergency services, their location is recorded, so we georeference the latitude and longitude of each call. 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 from the FBI's Uniform Crime Reporting program. The raw data consist of all reported brawls between January 2014 and February 2019. Given that the same event may have more than one entry, since multiple citizens could potentially report the same incident, we collapse by event, filter for our four localities, and then restrict the analysis to brawls that occur between 4:00 p.m. and 1:00 a.m., when neighborhood bars are open or may have recently closed to avoid capturing events unrelated to alcohol consumption.

NUSE 123 also collects reports on other alcohol-related incidents. We include street-level reports of personal injury, drunk and disorderly behavior, disturbing the peace, and illegal alcohol sales as additional outcomes to examine whether the program affects other incidents that occur within and around neighborhood bars.[18]

We merge these geocoded reports with bar locations using a client list of over 40,000 bars, with their respective latitude and longitude, provided by Fundación Bavaria. These merged data contain reports for thousands of alcohol-related incidents within and around 8,909 bars, over the course of 62 months. However, because the data cover eligible streets and quadrants in our sample of four localities, they are not necessarily representative of Bogotá as a whole.

Table 2 shows descriptive statistics for alcohol-related violence before implementation of the Good Drinks program (January 2014 to July 2018). On average, there is less than one reported brawl per month on each street segment, with a standard deviation between 1.5 and 1.8. Other incidents are less frequent, such as reports of personal injury, drunk and disorderly conduct, 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 cannot reject the hypothesis that

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

[18] 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 conduct 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	0.879	0.637
	(1.861)	(1.580)	(1.513)	
Personal injury (reports)	0.250	0.244	0.237	0.221
	(0.864)	(0.736)	(0.675)	
Drunk and disorderly	0.179	0.179	0.206	0.068
	(0.564)	(0.539)	(0.582)	
Disturbing the peace	1.187	1.139	1.207	0.540
	(2.806)	(2.414)	(2.378)	
Illegal alcohol sales	0.098	0.099	0.094	0.669
	(0.447)	(0.455)	(0.416)	
Street segments	3,029	2,730	228	

Source: Authors' elaboration from georeferenced administrative data.
Notes: The table presents monthly means for each variable and standard deviations in parentheses before the implementation of the Good Drinks 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 clustered standard errors by police quadrant and correspond to the hypothesis that coefficients for the *Indirectly* and *Directly treated* groups are jointly zero.

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alcohol-related incidents are statistically identical across the three groups before the intervention with 95 percent confidence.[19]

Empirical Strategy

We analyze the data using the strategy outlined in our Pre-Analysis Plan (Ham et al., 2018). Additional empirical exercises that we did not pre-register, which we discuss in the Results section, should be considered exploratory.

Given that survey data are only available after the intervention, we estimate the effects of the Good Drinks program using a cross-section post specification:

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

where i refers to a bar and s its street segment in locality l. The β coefficient estimates mean differences in outcomes between bars in directly treated and control street segments after the intervention. We include the variables in Table 1 as controls in X_{is} mainly as robustness tests, and to improve precision, since observed attributes are balanced across bars. Equation (1) does include fixed effects by locality to account for the stratified nature of the program (McKenzie, 2012). We estimate equation (1) using Ordinary Least Squares (OLS) with clustered standard errors by street segment. Note that we cannot separate direct and indirect effects for survey outcomes because we did not interview bartenders located in indirectly treated locations.

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

$$y_{sqt} = \alpha + \beta_1 D_{st} + \beta_2 S_{qt} + \lambda_{\{l,q,s\}} + \delta_t + u_{sqt}, \tag{2}$$

where s refers to street segments, q to police quadrants, and t indexes time (month-year cells).[20] 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 difference in outcomes between directly and indirectly treated street segments before and after the program. Second, β_2 estimates the spillover effects of the intervention by comparing outcomes between indirectly treated and control street segments over time.[21] We include time-invariant location fixed effects as the data permits (l = locality, q = quadrant, and s = street segment), as well as time effects to control for secular trends. In some specifications, we also control for location-specific linear time trends. We do not include time-varying controls at the street-segment level due to data availability and because existing variables such as other crimes may be "bad controls" that confound our estimates. Our main

[19] We also test the joint validity of alcohol-related incidents normalizing each variable with respect to its mean and standard deviation, then summing the values to obtain a single measure of alcohol-related incidents. In that case, the p-value that all means are equal across treatment groups is 0.8312, indicating that violent events due to alcohol consumption are balanced for all three groups before the intervention.

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

[21] 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 Drinks program begins on the street segment in August, September, or October 2018 (*Post*); see Table A1. All regressions control for the main effects (T_s^1, T_q^2, and *Post*). All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

specification is estimated using OLS, but we also implement other procedures, including Poisson and event study methods. All regressions cluster standard errors by quadrant.[22]

Both specifications estimate the intent-to-treat effects (ITT) of the Good Drinks program. However, these ITT effects capture the effect of being offered the program but may not be of policy interest for two reasons. On the one hand, we need to adjust for compliance: if bartenders did not adopt the suggested practices, then the treatment was not binding. Fortunately, the team visited each treated bar as "mystery shoppers" to verify whether bartenders implemented the suggested practices in their establishments. Table A4 in the Appendix shows the percentage of bars that fulfilled each criterion individually. We instrument compliance with random assignment for an "intersection" compliance indicator. Specifically, we create a binary variable equal to one if a bar fulfills all seven mystery shopper criteria and zero otherwise (61 percent of treated bars fulfill all criteria).[23] Therefore, we calculate a traditional Wald estimate, where the reduced form estimate is divided by the share of compliant bars. On the other hand, given that effective treatment differed from assigned treatment during implementation, we also calculate a Wald estimate to account for this difference. We employ instrumental variable methods that estimate Local Average Treatment Effects (LATE) in these specifications (Angrist, Imbens, & Rubin, 1996).

We also conduct additional procedures to ensure our estimation and inference are credible and transparent. First, we perform randomization inference on our ITT estimates of direct and spillover effects of the Good Drinks program on violence, as well as the probability that these effects are equal (Imbens & Wooldridge, 2009). Randomization inference does not rely on large-sample assumptions, model specification, 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 & Hochberg, 1995). Additionally, we conduct placebo experiments to ensure that our estimates are indeed capturing the causal effects of the Good Drinks program.

RESULTS

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 for robustness, and the third estimates a local average treatment effect that adjusts for compliance using the mystery shopper criteria.

The first set of results examines changes in sales practices. The Good Drinks manual suggests that bartenders should set the pace for heavy drinkers by encouraging water and food consumption between drinks, without necessarily selling more alcoholic beverages. Evidence from Table 3 indicates that treated bartenders were not

[22] Given the two-stage randomization of the program, we also estimate results using two-way clustered standard errors by street segment and police quadrant. The results are identical to those we present in the text. Given that clustering at the higher aggregate level of police quadrants is less likely to over reject the null hypothesis of no program effect, we present those estimates but note that results remain unchanged when adjusting standard errors for both clusters.

[23] 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 in the main text but are available upon request.

Table 3. Effects of the Good Drinks program on survey outcomes.

		Mean	(1)	(2)	(3)
	Observations	[SD]	ITT	ITT	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			0.315	0.400	0.399
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			0.008	0.002	0.002
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			0.008	0.002	0.002
F-statistic					362.3
B. Violence perceptions					
Brawls within or around bar	578	0.37	−0.018	−0.034	−0.055
(past 3 months)		[0.48]	(0.040)	(0.041)	(0.065)
q-value			0.664	0.400	0.399
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			0.227	0.400	0.399
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 Drinks program on survey outcomes (see the previous section on Data). Columns (1) and (2) report ITT estimates, while column (3) instruments compliance with all seven mystery shopper criteria with random assignment (see Table A4 in the Appendix). Given that we estimate results for six 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).
Significance levels: ***$p < 0.01$; **$p < 0.05$; ***$p < 0.1$.

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selling more alcohol compared to control bartenders. However, the treatment significantly increased the quantity of water and food sold. On average, treated bars sold 56 percent more water and 67 percent more food than untreated bars. When we adjust for compliance, the estimated increase is 87 percent for water and 107 percent for food. These findings are stable across specifications and remain significant at the 1 percent level after multiple hypothesis adjustments.[24] Since not all bars report selling water and food (see Table 3), we explore extensive margin changes in these practices in Appendix Table A5. We find that some bars begin selling water and food after the intervention, but the estimates become marginally insignificant after carrying out multiple hypothesis adjustments.[25]

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, over one third of bartenders in the control group reported brawls within and around their bar. There are no statistically significant differences between the treatment and control group, although 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 percent agreed that they had, but we find no statistically significant difference between treatment and control groups.

Effects on Alcohol-Related Violence

Table 4 shows the intent-to-treat effects of the Good Drinks program on reported brawls. We present five specifications, which vary with regard to the included fixed effects and location-specific linear time trends. Our estimates indicate no significant direct effects or spillovers compared to control street segments using both conventional and randomization inference p-values. We also test the null hypothesis that the estimated coefficients for direct and indirect effects are equal in each specification, which we are unable to reject. We note that the coefficients on directly treated streets are negative while the indirect effects (potential spillovers) are positive.

These findings are robust to different treatment definitions and estimation procedures. The Appendix shows results using a street segment's assigned status instead of effective treatment (Table A6) and estimates from Poisson regressions (Table A7).[26] We also estimate the effects of the Good Drinks 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 Drinks program for different specifications, procedures, treatment definitions, or compliance and treatment differences (see Tables A8 to A11). Table 5 shows LATE results that adjust for compliance with "mystery shopper" criteria and differences between assigned and effective treatment (panels A and B, respectively). After these adjustments, we continue to find no statistically significant changes on reported brawls due to the Good Drinks program.

Given that program roll-out differed across localities and bars began treatment at different times (see Table A1), we use event study methods to unify exposure to

[24] FDR adjusted q-values are calculated accounting for five outcomes across each specification in Table 3.
[25] The evidence also reveals that bartenders tend to sell pre-packaged food such as chips, instead of prepared meals.
[26] We also estimated regressions that weight each observation by the number of bars in the street segment. Given that the results are almost identical to those shown in Table 4 and due to space restrictions, we do not report that table, but these results are available upon request. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Table 4. Effects of the Good Drinks program on brawls.

	(1)	(2)	(3)	(4)	(5)
Direct effect (*D*)	−0.001	−0.012	−0.013	−0.013	−0.039
	(0.041)	(0.040)	(0.040)	(0.039)	(0.046)
RI: Pr(D = 0)	0.994	0.724	0.700	0.710	0.378
Indirect effect (*S*)	0.023	0.023	0.031	0.008	0.011
	(0.020)	(0.020)	(0.019)	(0.017)	(0.018)
RI: Pr(S = 0)	0.142	0.146	0.042	0.614	0.530
RI: *Pr(D = S)*	0.474	0.330	0.238	0.538	0.246
Adjusted R^2	0.003	0.417	0.417	0.418	0.424
Mean brawls	0.887	0.887	0.887	0.887	0.887
SD brawls	1.861	1.861	1.861	1.861	1.861
Fixed effects	Locality	Street	Street	Street	Street
Linear time trends	No	No	Locality	Quadrant	Street
Observations	371,194	371,194	371,194	371,194	371,194
Street segments	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221

Source: Authors' elaboration from georeferenced administrative data.
Notes: In all columns, the outcome variable is the number of violent brawls. Each column presents results from a separate regression. Clustered standard errors by police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of the Good Drinks program (see section on Data). 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).
Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{***}p < 0.1$.

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Table 5. Local average treatment effects (LATE) of the Good Drinks program on brawls.

	(1)	(2)	(3)	(4)	(5)
A. Compliance adjustment					
Direct effect (*D*)	−0.001	−0.019	−0.021	−0.021	−0.063
	(0.066)	(0.064)	(0.063)	(0.062)	(0.073)
Indirect effect (*S*)	0.023	0.023	0.031	0.008	0.011
	(0.020)	(0.020)	(0.019)	(0.017)	(0.018)
F-statistic	283.5	283.1	284.6	286.6	277.3
B. Assigned & effective status					
Direct effect (*D*)	0.013	−0.000	−0.002	0.002	−0.024
	(0.045)	(0.043)	(0.043)	(0.043)	(0.049)
Indirect effect (*S*)	0.024	0.024	0.031	0.010	0.012
	(0.020)	(0.020)	(0.019)	(0.018)	(0.018)
F-statistic	629,508.1	590,486.0	591,832.8	572,845.9	325,754.0
Fixed effects	Locality	Street	Street	Street	Street
Linear time trends	No	No	Locality	Quadrant	Street
Mean brawls	0.887	0.887	0.887	0.887	0.887
SD brawls	1.861	1.861	1.861	1.861	1.861
Observations	371,194	371,194	371,194	371,194	371,194
Street segments	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221

Source: Authors' elaboration from georeferenced administrative data.
Notes: In all columns, the outcome variable is the number of violent brawls. Each column presents results from a separate regression. Clustered standard errors by police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of the Good Drinks program using instrumental variables regression (see Data section), where we instrument compliance with all seven mystery shopper criteria with effective treatment (see Table A4 in the Appendix) in panel A and instrument effective treatment status with assigned status in panel B.
Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{***}p < 0.1$.

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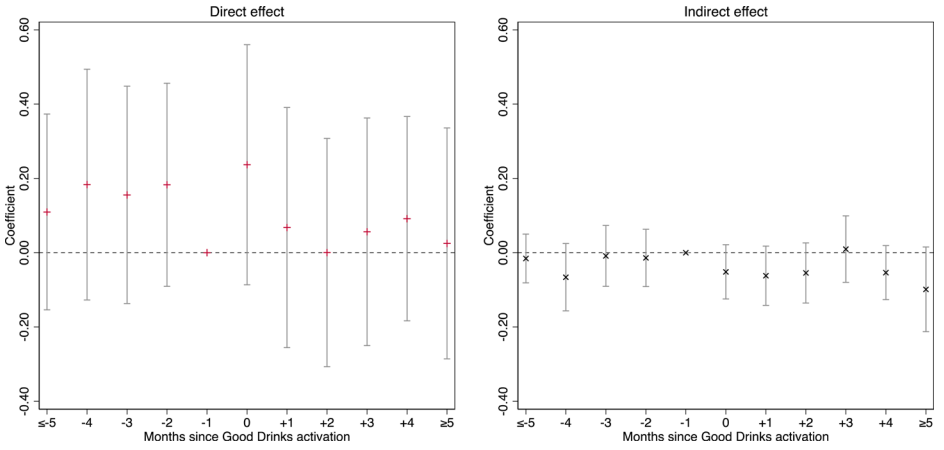

Source: Authors' elaboration from georeferenced administrative data.
Notes: Direct effects shown as "+" and indirect effects as "x"; 95 percent confidence intervals shown around the point estimates.

Figure 5. Event Study Effects of the Good Drinks Program on Brawls.
[Color figure can be viewed at wileyonlinelibrary.com]

the Good Drinks program. We estimate linear difference-in-difference regressions that examine differential trends for up to five months before and after the program. Figure 5 presents estimates using the specification in column (3) of Table 5 that includes street-level fixed effects. 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 five months after exposure. A similar exercise using weekly reports with a window of eight weeks before and 16 weeks after the intervention provides similar findings (see Appendix Figure A7).

Additionally, we perform the Goodman-Bacon (2018) decomposition since bars in some street segments received treatment before others, generating staggered implementation due to practical considerations. In these cases, difference-in-difference estimates are a weighted average between variation in treatment timing and "real" treatment and control comparisons. The results in Table A12 indicate that our estimates are driven by differences between treated and control street segments (weight of 0.97), and therefore variation in treatment timing is not a significant issue in our setting because only a few weeks elapse between the first and last treated bars.

These results provide evidence that the Good Drinks program did not have direct or spillover effects on reported brawls within and around bars up to five months after the intervention. Given our sample size, goodness of fit, and standard errors from the regressions, the minimum detectable effect ranges from 0.11 to 0.13 for direct effects and 0.05 to 0.06 for spillovers.[27] The coefficients in the tables, figures, and the results

[27] 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. To declare a coefficient statistically significant it needs to be 1.96 standard errors away from zero; and to have an 80 percent chance of finding a coefficient that is at least 1.96 standard errors away from zero, 80 percent of the distribution from which you draw coefficients needs to be to the right of 1.96. Because the inverse normal of 80 percent is 0.84, that's achieved with a normal centered at $1.96 + 0.84 = 2.8$.

in the Appendix[28] are significantly below these values and sufficiently close to zero with tight confidence intervals, suggesting precise nulls. That is, despite changes in bartender practices and patron behavior, there is no impact of the intervention on reported brawls within and around bars up to five months later.

Given that NUSE 123 also includes calls for other alcohol-related incidents, we explore whether the program had effects on reports of personal injury, drunk and disorderly conduct, disturbing the peace, and illegal alcohol sales. Given that these additional exercises were not included in our pre-analysis plan, they should be interpreted as exploratory. Table 6 presents results using our preferred difference-in-differences specification with street-level fixed effects.[29]

The first column reproduces the results for brawls, which were unchanged after the program. We also find no evidence of changes in reported incidents of personal injury and illegal alcohol sales. However, there is a statistically significant reduction in reports of drunk and disorderly conduct and incidents of disturbing the peace. Compared to control streets, there is a 22.3 percent reduction in reports of drunk and disorderly conduct on directly treated street segments of which 7.8 percent spills over to nearby streets, both significant with 90 percent confidence. This effect is robust to randomization inference but not multiple hypothesis testing. Reports of disturbing the peace fall by 16.6 percent on directly treated streets, with no evidence of significant spillovers. These results suggest that while the Good Drinks program did not change the number of reported brawls, it did reduce other alcohol-related incidents on balance, mainly related to coexistence outcomes.

Are the results for other alcohol-related incidents robust? We estimate local average treatment effects in Table A13 and find similar results with slightly larger effects (reduction of 35.8 percent in drunk and disorderly conduct with 7.8 percent spillovers, and a decrease of 26.6 percent in disturbances to the peace with no spillovers). We also conduct placebo regressions in Table A14 to conclude whether our findings on coexistence outcomes are indeed attributable to the program. We change the treatment timing: assuming the program begins in August 2016 and August 2017, respectively.[30] None of the coefficients on directly nor indirectly treated streets are significantly different from outcomes in control streets. These results confirm that the Good Drinks program significantly reduced incidents of drunk and disorderly conduct and disturbances to peace within and around directly treated bars, with few spillovers onto indirectly treated streets.

DISCUSSION

The Good Drinks program encouraged bartenders to implement practices that promote responsible alcohol consumption among patrons. However, these actions have no subsequent effect on violent brawls, although they do reduce other alcohol-related incidents. We now discuss potential mechanisms to explain these results, with reference to bartenders, patrons, and other actors.

Bartenders were receptive to the practices suggested by the program. The ethnographic study (conducted before implementation) indicated that most neighborhood bars mainly sold alcoholic beverages but not products such as food and water

[28] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

[29] 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 upon request.

[30] In these estimates, we only use data before the Good Drinks program was implemented in August 2018.

Table 6. Effects of the Good Drinks program on other alcohol-related incidents.

	Brawls	Personal injury	Drunk and disorderly	Disturbing the peace	Illegal alcohol sales
Direct effect (*D*)	−0.012	0.005	−0.040	−0.197	0.015
	(0.040)	(0.023)	(0.023)*	(0.082)**	(0.012)
RI: Pr(D = 0)	0.724	0.828	0.024	0.072	0.232
q-value	0.837	0.837	0.206	0.084	0.335
Indirect effect (*S*)	0.023	0.005	0.014	0.019	0.008
	(0.020)	(0.010)	(0.007)*	(0.050)	(0.006)
RI: Pr(S = 0)	0.146	0.492	0.040	0.728	0.112
q-value	0.430	0.705	0.279	0.705	0.405
RI: *Pr(D = S)*	0.330	0.988	0.006	0.052	0.622
Adjusted R^2	0.417	0.283	0.170	0.318	0.143
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	371,194	371,194	371,194	371,194	371,194
Street segments	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221

Source: Authors' elaboration from georeferenced administrative data.
Notes: Each column presents results from a separate regression. The reported specification is the same as in column (3) of Table 5, which includes street-level fixed effects. Clustered standard errors by police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of the Good Drinks program (see Data section). 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 five 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). Significance levels: ***p < 0.01; **p < 0.05; ***p < 0.1.

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because clients historically did not request them (Córdoba, 2018). Given that the program both informs bartenders about the benefits of water and food to reduce rapid inebriation and reminds customers of these benefits, it increases demand for these products. Given this rise in demand, bartenders adjust and begin stocking and selling more of these items due to changes in patrons' consumption behavior, which is consistent with our findings that sales of food and water increase but alcohol sales remain unchanged.

We also explore how bartenders who experienced a violent brawl in the past three months dealt with the situation. In the ethnographic study, bartenders reported that their main strategy to deal with brawls was to expel patrons (Cordoba, 2018); conversely, the Good Drinks manual suggests that bartenders try to defuse violent conflicts in their bar or call the police. Table A15[31] shows results from a linear regression (employing specification 2 from 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. While all coefficients are statistically indistinguishable from zero, their signs are in line with expectations: bartenders on treated streets are less likely to expel patrons and more inclined to defuse the conflict, call the police to intervene, or both. These estimates are imprecise because only 31 bars reported a brawl in the survey, yet they provide suggestive evidence that bartenders who received the program followed its recommended guidelines for conflict management.

Why does more responsible consumption fail to reduce the number of violent brawls? In the survey, we asked participating bartenders their impressions about the program and how to improve it in the future. Two responses were the most frequent: (i) the program "was too short and required more constant follow-up" and (ii) "while some patrons were receptive, most were indifferent to the advice the program suggests we communicate." The World Health Organization (WHO), in its SAFER initiative, recommends brief community-led interventions similar in nature to the Good Drinks program as one of its five policy areas for alcohol control (WHO, 2019). Our results, together with bartender feedback, suggest that brief interventions to change alcohol consumption patterns may not always be effective, especially when they require profound behavioral changes from heterogeneous agents that take time to solidify (Hummel & Maedche, 2019). Behavioral interventions are promising, but the form and extent of their delivery matters for impact.

While medical evidence shows that consuming food and water while drinking reduces blood-alcohol content in consumers (Carpenter & Dobkin, 2011; Parrott & Eckhardt, 2018), due to lack of data, either at the individual level or in the aggregate, we are unable to detect whether patrons in treated bars showed reduced levels of inebriation compared to streets with untreated establishments. However, our result that drunk and disorderly behavior and disturbances to the peace fall around treated bars lends some support to the idea that the Good Drinks program has effects on inebriation. It is possible that the causal mechanism driving the relationship between alcohol consumption and violence operates through changes in the amount of alcohol consumed, which did not occur in the Good Drinks program. Future studies could conduct follow-ups with patrons after they leave establishments, which would empirically document changes in inebriation and provide evidence of how crime changes when alcohol consumption decreases.

Alcohol consumers are heterogeneous and respond differently to incentives. Many bartenders reported that some customers responded favorably to their recommendations while others "were unreceptive" or "did not take these into account when

[31] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

acting." For instance, consumers who changed their behavior may be less likely to engage in violence. People who drink and then fight may consume alcohol differently than those who do not fight. Encouraging this group to consume more water or food may not change their overall level of intoxication enough to reduce harmful activities. Future programs could pursue different strategies, both behavioral and restrictive, to address differences among consumer profiles, or provide them with tailor-made incentives (Schilbach, 2019).

While bartenders and patrons are key actors in terms of alcohol regulation and policy, they are not isolated from alcohol producers, distributors, and other actors such as law enforcement. The Good Drinks program only targeted bartenders and patrons, but other agents may also be pivotal in curbing excessive alcohol consumption. The available literature and our findings suggest there is not one unique policy that can mitigate all the negative consequences of excessive alcohol consumption. Behavioral interventions such as the Good Drinks program will likely interact with legal restrictions regarding availability, price changes, prevention strategies, greater policing, and others considered by the WHO's SAFER initiative (WHO, 2019). Overall, alcohol control requires multiple and complementary policy measures to provide adequate incentives for consumers, bartenders, producers, and law enforcement to reduce excessive alcohol consumption and its negative consequences.

CONCLUSION

This paper evaluates the Good Drinks program, which encourages bartenders to adopt standardized practices that promote responsible alcohol consumption by consumers to reduce alcohol-attributable violence. We employ a two-stage randomized design that allows us to estimate the direct and indirect impact of this program. We first study whether the intervention led to changes in consumption practices using survey data and then estimate the effects of the program on alcohol-related violence using a georeferenced administrative panel of violent incidents.

Results indicate that the Good Drinks program changed bartender practices, but these actions had no subsequent effect on alcohol-related brawls. While alcohol sales were unchanged, bartenders sold more water (56 percent increase) and food (67 percent increase), contributing to more responsible alcohol consumption. However, we find no direct or spillover effects of the Good Drinks program on brawls using different model specifications, treatment definitions, and alternative estimation procedures. Given the statistical power of our experiment, these results should be interpreted as precisely estimated nulls. We do find some direct effects on other alcohol-related incidents, mainly reports of drunk and disorderly conduct (22.3 percent decrease) and disturbing the peace (16.6 percent reduction), with few significant spillovers to indirectly treated areas. These results are robust to placebo experiments, randomization inference, and multiple hypothesis tests.

Some questions remain regarding how to approach and formulate alcohol policies and implement more effective regulations and harm reduction strategies. For instance, policymakers need to weigh a policy's restrictiveness, the possibility of implementing multiple complementary strategies to target heterogeneous agents, and the possibility of implementing more public-private partnerships. Researchers have a role to play in proposing and rigorously evaluating solutions to determine what works to reduce excessive alcohol consumption and its externalities, as well as how to implement and scale novel and complementary solutions across different contexts

that stand a reasonable chance of improving overall welfare through harm reduction strategies.

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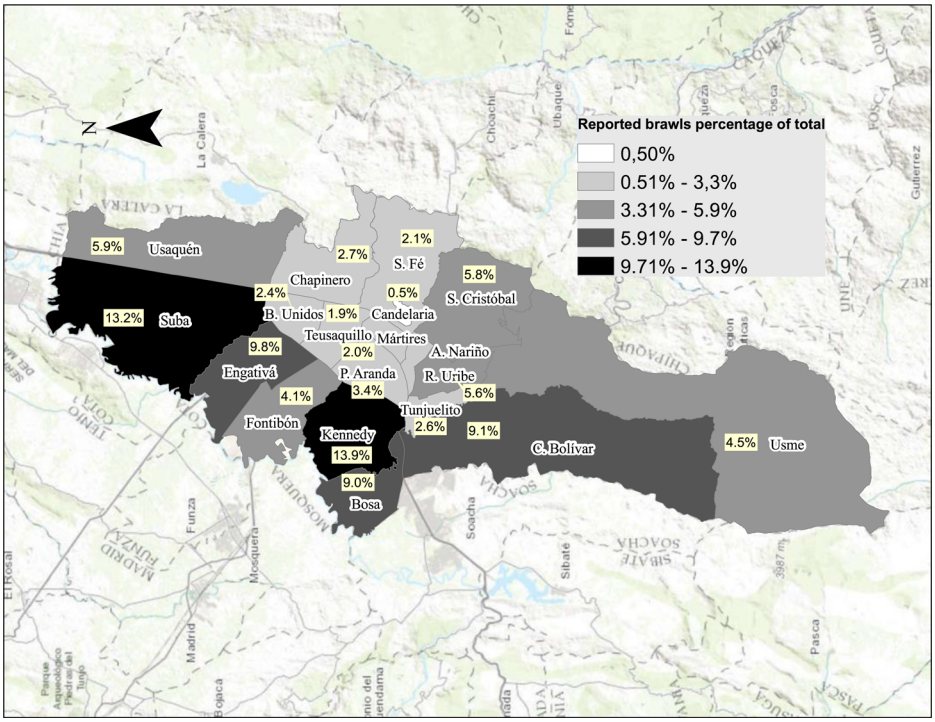
APPENDIX



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

Figure A1. Distribution of Reported Brawls by Locality in Bogotá.

Table A1. Staggered rollout of Good Drinks 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 on each date by locality.

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Table A2. 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 (reports)	0.250	0.204	0.005	0.244	0.204	0.037
	(0.864)	(0.570)		(0.733)	(0.570)	
Drunk and disorderly behavior	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		2,939	2,958	

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 A3. 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.

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Table A4. Compliance with Good Drinks program (mystery shopper).

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 A5. Effects of the Good Drinks program on other survey outcomes.

	Observations	Mean [SD]	(1) ITT	(2) ITT	(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 Drinks program on survey outcomes (see Data section). Columns (1) and (2) report ITT estimates, while column (3) instruments compliance with all seven mystery shopper criteria with random assignment (see Table A4). Given that we estimate results for eight 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).
Significance levels: ***p<0.01; **p<0.05; ***p<0.1.

Table A6. Effects of the Good Drinks program on brawls (assigned).

	(1)	(2)	(3)	(4)	(5)
Direct effect (D)	0.013	−0.000	−0.002	0.002	−0.024
	(0.044)	(0.043)	(0.043)	(0.042)	(0.049)
RI: Pr(D = 0)	0.718	1.000	0.970	0.944	0.620
Indirect effect (S)	0.024	0.024	0.031	0.010	0.012
	(0.020)	(0.020)	(0.019)	(0.017)	(0.018)
RI: Pr(S = 0)	0.138	0.138	0.038	0.540	0.480
RI: Pr(D = S)	0.762	0.492	0.374	0.826	0.450
Adjusted R^2	0.003	0.417	0.417	0.418	0.424
Mean brawls	0.886	0.886	0.886	0.886	0.886
SD brawls	1.857	1.857	1.857	1.857	1.857
Fixed effects	Locality	Street	Street	Street	Street
Linear time trends	No	No	Locality	Quadrant	Street
Observations	371,194	371,194	371,194	371,194	371,194
Street segments	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221

Source: Authors' elaboration from georeferenced administrative data.
Notes: In all columns, the outcome variable is the number of violent brawls. Each column presents results from a separate regression. Clustered standard errors by police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of assignment into the Good Drinks program (see Data section). 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).
Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{***}p < 0.1$.

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Table A7. Effects of the Good Drinks program on brawls (Poisson).

	(1)	(2)	(3)	(4)	(5)
Direct effect (D)	−0.002	−0.015	−0.014	−0.018	−0.037
	(0.051)	(0.050)	(0.049)	(0.047)	(0.056)
Indirect effect (S)	0.028	0.028	0.036	0.011	0.016
	(0.024)	(0.024)	(0.023)	(0.020)	(0.020)
Mean brawls	0.887	0.887	0.887	0.887	0.887
SD brawls	1.861	1.861	1.861	1.861	1.861
Fixed effects	Locality	Street	Street	Street	Street
Linear time trends	No	No	Locality	Quadrant	Street
Observations	371,194	371,194	371,194	371,194	371,111
Street segments	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221

Source: Authors' elaboration from georeferenced administrative data.
Notes: In all columns, the outcome variable is the number of violent brawls. Each column presents results from a separate regression. Clustered standard errors by police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of the Good Drinks program using Poisson regression (see Data section).
Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{***}p < 0.1$.

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Table A8. Effects of the Good Drinks program on brawls (weekly frequency).

	(1)	(2)	(3)	(4)	(5)
Direct effect (D)	0.000	−0.002	−0.002	−0.003	−0.009
	(0.009)	(0.009)	(0.008)	(0.008)	(0.010)
RI: Pr(D = 0)	0.990	0.794	0.778	0.758	0.346
Indirect effect (S)	0.004	0.004	0.006	0.001	0.001
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
RI: Pr(S = 0)	0.270	0.270	0.108	0.854	0.750
RI: Pr(D = S)	0.628	0.482	0.362	0.718	0.298
Adjusted R^2	0.002	0.146	0.146	0.146	0.149
Controls	0.204	0.204	0.204	0.204	0.204
SD brawls	0.692	0.692	0.692	0.692	0.692
Fixed effects	Locality	Street	Street	Street	Street
Linear time trends	No	No	Locality	Quadrant	Street
Observations	1,616,490	1,616,490	1,616,490	1,616,490	1,616,490
Street segments	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221

Source: Authors' elaboration from georeferenced administrative data.
Notes: In all columns, the outcome variable is the number of violent brawls. Each column presents results from a separate regression. Clustered standard errors by police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of the Good Drinks program (see Data section). 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).
Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{***}p < 0.1$.

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Table A9. Effects of the Good Drinks program on brawls (assigned, weekly frequency).

	(1)	(2)	(3)	(4)	(5)
Direct effect (D)	0.003	0.000	0.000	0.000	−0.006
	(0.009)	(0.009)	(0.009)	(0.009)	(0.011)
RI: Pr(D = 0)	0.770	0.966	0.984	0.956	0.566
Indirect effect (S)	0.005	0.005	0.006	0.001	0.002
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
RI: Pr(S = 0)	0.254	0.256	0.102	0.784	0.714
RI: Pr(D = S)	0.868	0.672	0.536	0.970	0.464
Adjusted R^2	0.002	0.146	0.146	0.146	0.149
Controls	0.204	0.204	0.204	0.204	0.204
SD brawls	0.691	0.691	0.691	0.691	0.691
Fixed effects	Locality	Street	Street	Street	Street
Linear time trends	No	No	Locality	Quadrant	Street
Observations	1,616,490	1,616,490	1,616,490	1,616,490	1,616,490
Street segments	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221

Source: Authors' elaboration from georeferenced administrative data.
Notes: In all columns, the outcome variable is the number of violent brawls. Each column presents results from a separate regression. Clustered standard errors by police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of assignment into the Good Drinks program (see Data section). 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).
Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{***}p < 0.1$.

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Table A10. Effects of the Good Drinks program on brawls (LATE, weekly frequency).

	(1)	(2)	(3)	(4)	(5)
A. Compliance adjustment					
Direct effect (D)	0.000	−0.003	−0.004	−0.004	−0.015
	(0.014)	(0.014)	(0.013)	(0.013)	(0.016)
Indirect effect (S)	0.004	0.004	0.006	0.001	0.001
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
F-statistic	285.2	284.4	286.0	287.8	281.4
B. Assigned & effective status					
Direct effect (D)	0.003	0.000	0.000	0.000	−0.006
	(0.009)	(0.009)	(0.009)	(0.009)	(0.011)
Indirect effect (S)	0.005	0.005	0.006	0.001	0.001
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
F-statistic	592,896.3	566,765.6	568,864.4	555,003.2	372,215.3
Fixed effects	Locality	Street	Street	Street	Street
Linear time trends	No	No	Locality	Quadrant	Street
Mean brawls	0.204	0.204	0.204	0.204	0.204
SD brawls	0.691	0.691	0.691	0.691	0.691
Observations	1,616,490	1,616,490	1,616,490	1,616,490	1,616,490
Street segments	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221

Source: Authors' elaboration from georeferenced administrative data.
Notes: In all columns, the outcome variable is the number of violent brawls. Each column presents results from a separate regression. Clustered standard errors by police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of the Good Drinks program using instrumental variables regression (see Data section), where we instrument compliance with all seven mystery shopper criteria with effective treatment (see Table A4) in panel A and instrument effective treatment status with assigned status in panel B.
*Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{***}p < 0.1$.

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Table A11. Effects of the Good Drinks program on brawls (Poisson, weekly frequency).

	(1)	(2)	(3)	(4)	(5)
Direct effect (D)	−0.000	−0.011	−0.010	−0.016	−0.039
	(0.047)	(0.047)	(0.046)	(0.045)	(0.054)
Indirect effect (S)	0.023	0.023	0.032	0.005	0.010
	(0.024)	(0.024)	(0.023)	(0.020)	(0.020)
Mean brawls	0.204	0.204	0.204	0.204	0.204
SD brawls	0.692	0.692	0.692	0.692	0.692
Fixed effects	Locality	Street	Street	Street	Street
Linear time trends	No	No	Locality	Quadrant	Street
Observations	1,616,490	1,616,490	1,616,490	1,616,490	1,616,490
Street segments	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221

Source: Authors' elaboration from georeferenced administrative data.
Notes: In all columns, the outcome variable is the number of violent brawls. Each column presents results from a separate regression. Clustered standard errors by police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of the Good Drinks program using Poisson regression (see Data section).
Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{***}p < 0.1$.

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Table A12. Goodman-Bacon decomposition.

Dependent variable	Coefficient	Weight
Brawls		
Timing groups	−0.083	0.002
Never treated vs. timing	−0.023	0.970
Within	0.395	0.028
Personal injury		
Timing groups	−0.023	0.002
Never treated vs. timing	0.003	0.970
Within	0.085	0.028
Drunk and disorderly		
Timing groups	0.005	0.002
Never treated vs. timing	−0.048	0.970
Within	0.240	0.028
Disturbing the peace		
Timing groups	−0.222	0.002
Never treated vs. timing	−0.212	0.970
Within	0.325	0.028
Illegal alcohol sales		
Timing groups	0.073	0.002
Never treated vs. timing	0.011	0.970
Within	0.141	0.028

Source: Authors' elaboration from georeferenced administrative data.
Notes: For each outcome, we estimate the weights of the different comparisons of the difference-in-difference estimates as suggested in Goodman-Bacon (2018). *Timing groups* refers to differences between early and late treated units, *Never treated vs. timing* refers to comparisons between treated and control units, and *Within* captures the weights of differences in covariates across units.

Table A13. Effects of the Good Drinks program on other alcohol-related incidents (LATE).

	Brawls	Personal injury	Drunk and disorderly	Disturbing the peace	Illegal alcohol sales
Direct effect (*D*)	−0.019	0.008	−0.064	−0.316	0.024
	(0.064)	(0.038)	(0.036)*	(0.131)**	(0.019)
q-value	0.838	0.838	0.202	0.084	0.341
Indirect effect (*S*)	0.023	0.005	0.014	0.019	0.008
	(0.020)	(0.010)	(0.007)*	(0.050)	(0.006)
q-value	0.430	0.705	0.279	0.705	0.405
F-statistic	283.1	283.1	283.1	283.1	283.1
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	371,194	371,194	371,194	371,194	371,194
Street segments	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221

Source: Authors' elaboration from georeferenced administrative data.
Notes: Each column presents results from a separate regression. The reported specification is the same as in column (3) of Table 5, which includes street-level fixed effects. Clustered standard errors by police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of the Good Drinks program using instrumental variables regression (see Data section), where we instrument compliance with all seven mystery shopper criteria with effective treatment (see Table A4). Given that we estimate results for five 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).
Significance levels: ***$p < 0.01$; **$p < 0.05$; ***$p < 0.1$.

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Table A14. Placebo effects of the Good Drinks 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)	0.022	−0.003	−0.008	0.006	−0.000
	(0.031)	(0.013)	(0.016)	(0.097)	(0.010)
RI: Pr(D = 0)	0.462	0.854	0.564	0.948	0.994
q-value	0.990	0.990	0.990	0.990	0.990
Indirect effect (S)	0.015	−0.002	0.004	−0.065	0.000
	(0.016)	(0.007)	(0.007)	(0.048)	(0.006)
RI: Pr(S = 0)	0.280	0.696	0.492	0.130	0.978
q-value	0.859	0.948	0.907	0.859	0.983
Adjusted R^2	0.417	0.282	0.173	0.323	0.148
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)	0.035	0.005	0.008	0.119	0.003
	(0.035)	(0.016)	(0.022)	(0.123)	(0.009)
RI: Pr(D = 0)	0.330	0.772	0.600	0.212	0.800
q-value	0.761	0.761	0.761	0.761	0.761
Indirect effect (S)	0.019	0.007	0.004	−0.068	0.003
	(0.017)	(0.008)	(0.008)	(0.048)	(0.006)
RI: Pr(S = 0)	0.246	0.178	0.600	0.124	0.484
q-value	0.580	0.580	0.628	0.580	0.628
Adjusted R^2	0.417	0.282	0.173	0.323	0.148
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	329,588	329,588	329,588	329,588	329,588
Street segments	5,987	5,987	5,987	5,987	5,987
Quadrants	221	221	221	221	221

Source: Authors' elaboration from georeferenced administrative data.
Notes: Each column presents results from a separate regression. The reported specification is the same as in column (3) of Table 5, which includes street-level fixed effects. Clustered standard errors by police quadrant are shown in parentheses. The table reports difference-in-difference estimates of the direct and spillover effects of the Good Drinks 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 five 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).
Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{***}p < 0.1$.

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Table A15. Effects of the Good Drinks program on bartender strategies to defuse conflict.

	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) x 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	578	578	578	578
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 Drinks 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 four 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). Significance levels: ***p < 0.01; **p < 0.05; ***p < 0.1.

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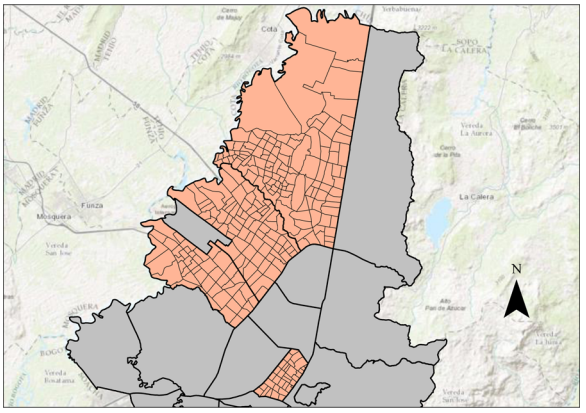
Source: Authors' elaboration from program materials.

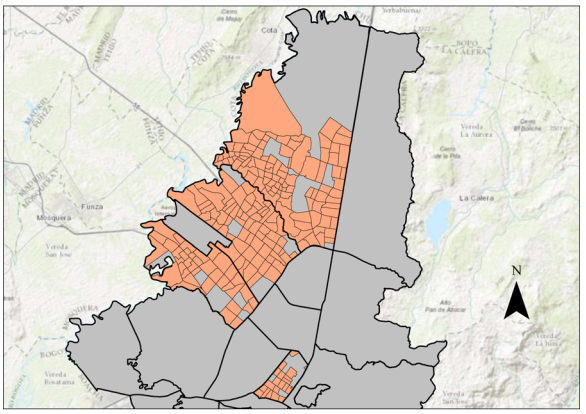
Figure A2. Selected Pages from the Good Bartender Manual (in Spanish).

Source: Authors' elaboration from program materials.

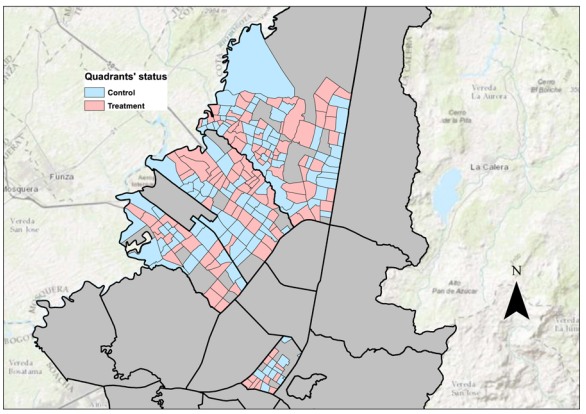
Figure A3. Materials Given to Participating Bars (in Spanish).



(a) All police quadrants



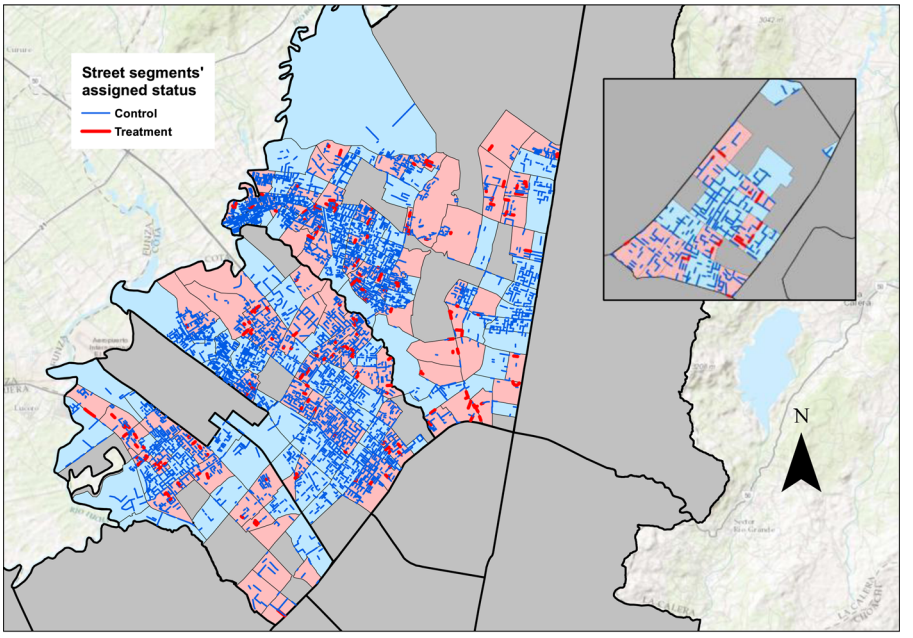
(b) Eligible police quadrants



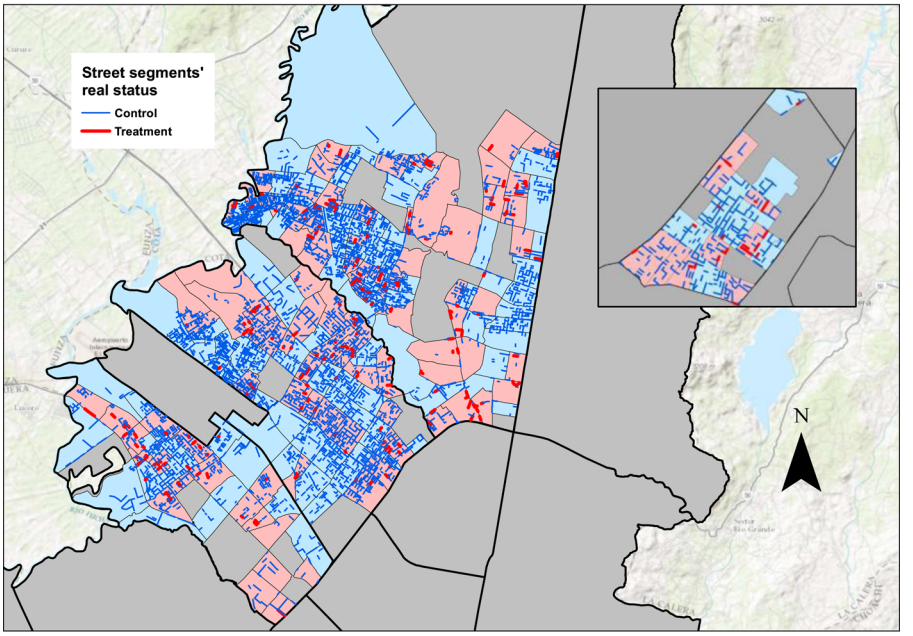
(c) Treatment and control police quadrants

Source: Authors' elaboration from georeferenced administrative data.

Figure A4. Selection of Quadrants into Intervention.



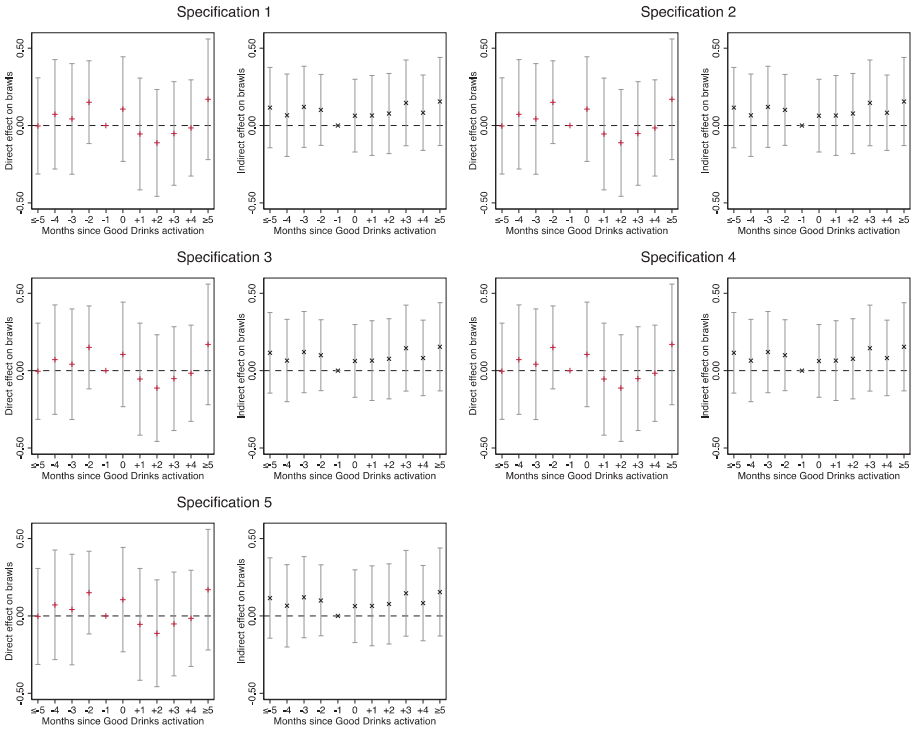
(a) Assigned treatment status



(b) Effective treatment status

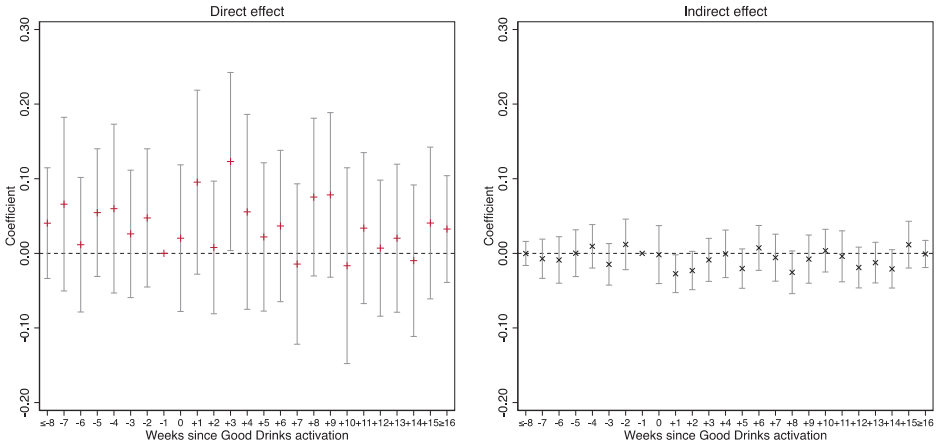
Source: Authors' elaboration from georeferenced administrative data.
Notes: Blue areas denote control quadrants and red areas denote treatment quadrants. Blue lines represent control street segment and red lines depict treated control segments. Due to scale, Los Mártires is shown in the small block on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

Figure A5. Differences Between Assigned Status and Effective Treatment.



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

Figure A6. Event Study Effects of the Good Drinks Program on Brawls (All Specifications).



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

Figure A7. Event Study Effects of the Good Drinks Program on Brawls (Weekly).