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Alcohol, violence and injury-induced mortality: Evidence from a modern-day prohibition*

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Abstract

This paper evaluates the impact of a sudden and unexpected nation-wide alcohol sales ban in South Africa. We find that this policy causally reduced injury-induced mortality in the country by at least 14%. We argue that this estimate constitutes a lower bound on the true impact of alcohol on injury-induced mortality. We also document a sharp drop in violent crimes, indicating a tight link between alcohol and aggressive behavior in society. Our results underscore the severe harm that alcohol can cause and point towards a role for policy measures that target the heaviest drinkers in society.

JEL Codes: I18, I12, K42

Keywords: Alcohol, mortality, economics, health, crime, South Africa, COVID-19, violence.

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1 Introduction

Excessive alcohol consumption is common in many developing and developed countries (Allen et al., 2017; Katikireddi et al., 2017; Rehm et al., 2018; WHO, 2019; Probst et al., 2020). It has been associated with numerous social harms, including motor vehicle collisions, violence, risky sexual behavior, long-run adverse health effects, reduced productivity at work, mortality, and morbidity (see, e.g., Carpenter and Dobkin, 2011; Rehm et al., 2017; Griswold et al., 2018; WHO, 2019). These harms are often borne by individuals other than the person consuming alcohol. These externalities may be imposed either directly (as in the case of interpersonal violence) or indirectly (as in the case of public health insurance).[1] Consequently, questions regarding the morality and correct societal regulation of alcohol have been debated in societies around the world for centuries, with virtually all modern and past societies placing legal and religious constraints on alcohol consumption (Phillips, 2014). It is crucial, therefore, to accumulate robust empirical evidence that allows us to construct a clear picture of the true influence of alcohol in society. Despite this, our current understanding of the causal impact that alcohol has at a societal level is largely limited to the estimates of theoretical models (see, e.g., Rehm et al., 2003, 2017; Probst et al., 2018; Shield et al., 2020). There is a scarcity of direct causal evidence.[2] One reason for this is that it is rare to observe an abrupt abatement in alcohol consumption in the entire population of a region or country. Without an exogenous shift of this nature, it is difficult to parse the influence of alcohol consumption on a particular outcome from the influence of the personal characteristics of the individuals who choose to drink heavily.

The sudden and unexpected ban on the sale of alcohol in South Africa on July 13, 2020 provides a rare opportunity to understand how alcohol consumption influences behavior and outcomes

[1]Alcohol consumption may also lead individuals to harm themselves—intoxication can reduce self-control, inducing myopic behavior that the individual would avoid if sober (O'Donoghue and Rabin, 2001; Schilbach, 2019).

[2]The causal evidence that does exist typically focuses on specific segments of society, with evaluations of the impact of changes to the minimum legal drinking age providing the main example of this (Carpenter, 2004; Carpenter and Dobkin, 2011, 2017).

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at a societal level. Since research by health scientists has identified alcohol consumption as a major risk factor for injury-related deaths globally, this points towards the hypothesis that reducing alcohol consumption in an entire country could cause a large reduction in injury-related mortality (see, e.g., Rehm et al., 2003, 2017).[3] It is of key importance to test this hypothesized causal relationship by assessing how mortality is actually affected when a policy that drastically reduces alcohol consumption is introduced.

This paper uses the exogenous variation provided by a natural experiment in the form of a sudden and unexpected alcohol sales ban to study the causal impact of alcohol on mortality due to unnatural causes at a societal level.[4] We also present evidence on one central potential mechanism underpinning this relationship by evaluating the impact that the ban had on aggressive behavior in society (e.g., homicides, assaults, reported rape cases). This analysis is valuable as it provides policy-makers with robust evidence about the harm that alcohol consumption generates in society and informs our understanding of whether reducing alcohol consumption is an effective way to save lives and alleviate interpersonal violence. It therefore contributes evidence towards the wider discussion regarding the aggregate costs and benefits of alcohol consumption to society, paving the way for evidence-based policy making.

Our main analysis uses daily mortality data from South Africa from the period between January 1, 2017 and December 31, 2021. This allows us to use data from previous years to carefully

[3]For example, the WHO (2019) estimates that alcohol was responsible for 0.9 million of the 5.9 million global injury-related deaths in 2016, while Probst et al. (2018) use a comparative risk assessment approach to estimate that over 12 000 of the approximately 50 000 injury-related deaths in South Africa in 2015 were attributable to alcohol consumption.

[4]In the paper we use the terms "injury-induced mortality" and "unnatural mortality" interchangeably. We do this because we find that the former provides a more natural terminology and is therefore more suitable for an interdisciplinary readership, while the latter corresponds to the designation of these deaths in the National Population Register dataset and is used on abbreviated death certificates in South Africa (Dorrington et al., 2020). Deaths due to unnatural causes include deaths with an external cause, such as homicides, traffic injuries and suicides, while natural deaths pertain to conditions resulting from aging and illness.

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control for temporal regularities in mortality observed over the course of the year in our analysis. This is important because we show that there are highly regular, systematic patterns in the number of unnatural deaths observed according to the day-of-the-week and day-of-the-month. Using a difference-in-difference empirical strategy, we evaluate the change in mortality due to unnatural causes that occurred as a result of the alcohol sales ban implemented in July 2020.

This policy shift serves as a good natural experiment for several reasons. First, it was unexpected. The alcohol ban was announced in the evening of Sunday, July 12, 2020, and came into immediate effect from Monday morning on July 13, 2020. Second, it was implemented in the middle of the so-called "Level 3" COVID-19 policy response period during which time other policies and regulations were largely held constant.[5] One important exception to this is that the alcohol ban was introduced together with a curfew, which operated between 9PM and 4AM. However, in our analysis we demonstrate that this curfew had very little influence on mortality. First, we show that when the alcohol ban was lifted, but the curfew remained in place, unnatural mortality jumped back up to the same level observed in previous years, indicating that the curfew alone did not reduce unnatural mortality. Second, we conduct a sensitivity analysis that makes use of a one hour reduction in the length of the curfew (i.e. moving the start time from 9PM to 10PM) which occurred in the middle of the relevant period. This shift on the intensive margin during the alcohol ban period had no effect on unnatural mortality.

Our main result is that the alcohol ban reduced the number of people dying from unnatural causes in South Africa by at least 120 per week. This reflects the lowest estimate of the effect size that we obtain across a range of different empirical specifications. It represents a substantial reduction in mortality due to unnatural causes, since it implies a 14% reduction in all unnatural deaths in the country when compared to the average level during the five weeks immediately preceding the July Alcohol Ban.

The analysis below reveals an extremely strong gender gradient in this effect—the observed reduction in mortality is almost entirely confined to men. This is not entirely surprising for two

[5]The July Alcohol Ban was in force between July 13, 2020 and August 17, 2020. It therefore divides the Level 3 period, which spanned June 1, 2020 to August 17, 2020, neatly in half.

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reasons. First, in South Africa men are far more likely to die of unnatural causes than women (approximately 78% of the over 150 000 deaths from unnatural causes recorded in our dataset between 2017 and 2019 were males).[6] This pattern is not unique to South Africa. For example, Gawryszewski and Rodrigues (2006) describe the gender distribution of injury-related mortality in Brazil in 2003 and show that 84.3% of the people that died from injury-related causes (e.g., homicides, suicides, transport-related deaths) were men. Second, as in many countries around the world (e.g., Brazil, Russia), in South Africa men are far more likely than women to engage in heavy drinking (WHO, 2019). We find that the ban on alcohol reduced the number of men dying due to unnatural causes by at least 120 per week, but find no evidence that it had a statistically significant effect on injury mortality among women in the population as a whole. (Importantly, this does not imply that the absence of alcohol had no impact on other outcomes for women, such as gender-based violence, which often does not result in death. Below, we show that the number of reported rape cases was reduced by the alcohol ban.) Further, we provide evidence that approximately half of the observed reduction in mortality is found amongst young men aged 15-34.

To provide support for the validity of these main results, we conduct several robustness exercises. These include running placebo regressions, varying the window size around the policy change used for our analysis, and relaxing the assumptions made on the error structure (Appendix Section C.2). We also address two key concerns regarding the quality of the natural experiment and the assumptions underlying our ability to use it to identify the impact of alcohol on mortality (Appendix Section C.1).

To better understand what is driving this drop in mortality due to unnatural causes, we augment our main results by conducting an additional analysis that examines police crime data on

[6]While detailed cause-of-death data is not yet available in South Africa for 2020, Matzopoulos et al. (2015) report that for 2009, the three leading causes of unnatural mortality in South Africa were homicides, road-traffic injuries and suicide. Homicides constituted 36% of unnatural deaths, with 86% of these being male deaths. Road-traffic injuries resulted in 33% of unnatural deaths, with 76% of these being male deaths. Suicides made up 12.3% of unnatural deaths, with 82% of these being male deaths.

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homicides, assaults, and reported rape cases during the period of interest. We document evidence indicating that the alcohol ban resulted in a sharp drop in all of these outcomes, with at least 77 fewer homicides, 790 fewer assaults and 105 fewer rape cases reported per week during the alcohol ban period in comparison to the preceding five weeks. This constitutes a drop in each outcome of 21%, 33% and 19% respectively.

To illustrate the dynamic effects of the alcohol ban, we also report the results from event study analyses that examine the evolution in unnatural mortality and the three violent crime outcomes over time. The general pattern that emerges is that the effect of the ban appears to have been strongest in the first few weeks. One speculative potential reason for this is that black market trade and production may have reduced the effectiveness of the ban over time. Overall, the results we document provide compelling evidence that alcohol is causally responsible for inducing aggressive behavior in society at a significant scale, resulting in substantial harm.

What lessons can be drawn from these results? First, these findings are highly informative for policy discussions within South Africa as they provide clear causal evidence of a strong relationship between alcohol consumption and both interpersonal violence and unnatural mortality. This evidence therefore helps to support the conclusions drawn from comparative risk assessment (CRA) analyses by health scientists (see, e.g., Probst et al., 2018; Matzopoulos et al., 2021).

Second, this paper provides a valuable contribution to the collective global effort to better understand the relationship between alcohol, violence and injury-related outcomes more generally. This is an extremely important endeavour, since alcohol is estimated to have been responsible for 5.3% of all deaths worldwide in 2016 (3 million), with 0.9 million of those being injury-related deaths (WHO, 2019; Shield et al., 2020). While it is essential to acknowledge that any evidence collected within a single country relates to behavior that occurs within a particular societal context, collecting rigorous evidence across a range of contexts makes it possible to aggregate the evidence and identify which alcohol-driven relationships occur systematically across contexts, and which are context-specific (i.e., mediated by an interaction between alcohol consumption and other societal factors). The evidence presented here is particularly useful for this exercise since South Africa is part of a class of countries that make up a large part of the world's population, but tend to be

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underrepresented as the focus of academic research relative to more developed nations due partially to constraints on the availability of highly detailed data. This evidence from South Africa provides an informative benchmark for countries characterised by high levels of injury-related deaths, a sizable fraction of the population that drinks excessively, a strong asymmetry in drinking patterns between men and women, and high levels of poverty and inequality. This set of characteristics is reflective of several countries in Eastern Europe and South America, such as Brazil (where 32.6% of men and 6.9% of women were heavy episodic drinkers [HEDs] in 2016, which is nearly identical to the pattern in South Africa, where 30.6% of men and 6.5% of women were HEDs, according to WHO (2019)) and Russia (where 48.4% of men and 24.2% of women were HEDs in 2016). Both Brazil and Russia also share many other structural similarities with South Africa that could interact with alcohol consumption in influencing behavior, such as suffering from social issues including poverty, inequality and high levels of violence. Both countries are also characterized by a strong gender asymmetry in unnatural deaths, similar to South Africa (see, e.g., Starodubov et al., 2018; Gawryszewski and Rodrigues, 2006).

Third, our results provide a society-level demonstration of the way in which alcohol can act as a catalyst in inducing violence. While contextual factors in different countries may shape the way in which excessive alcohol consumption manifests in behavior, the growing body of evidence of a deep link between excessive alcohol consumption and aggressive behavior is important for all countries. The evidence discussed in this paper complements a large body of existing work showing that there is a strong association between alcohol consumption and aggressive behavior across a range of domains (for a review of this evidence, see Tomlinson et al., 2016). More specifically, our causal evidence contributes to the existing literature that documents a strong association between homicides and alcohol, finding that a high fraction of homicide offenders (and victims) were under the influence of alcohol at the time of the offence (for systematic reviews, see Darke, 2010; Kuhns et al., 2011, 2014).[7]

[7]For example, Kuhns et al. (2011) reports that from over 70 000 toxicology test results from 13 countries (predominantly from the United States), 48% of homicide victims tested positive for alcohol, while Kuhns et al. (2014) reports that from almost 30 000 homicide offenders across nine

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Therefore, in summary, the evidence discussed in this paper is highly informative for local policy discussions, but also helps to advance the wider global effort of constructing a clear evidence-based understanding of the relationship between alcohol, aggressive behaviour and harmful outcomes.

Relation to the literature. This paper contributes to several strands of the literature. It relates most closely to the body of work that studies the short-run relationship between alcohol and harmful behavior, such as violence, suicide and crime (Carpenter, 2004, 2005a, 2007; Biderman et al., 2010; Rossow and Norström, 2012; Wilkinson et al., 2016), road traffic collisions (Baughman et al., 2001; Chikritzhs and Stockwell, 2006), risky sexual behavior (Carpenter, 2005b), and outcomes such as mortality and morbidity (Matzopoulos et al., 2006; Carpenter and Dobkin, 2009; Marcus and Siedler, 2015; Carpenter and Dobkin, 2017; Sanchez-Ramirez and Voaklander, 2018; Nakaguma and Restrepo, 2018). There are two main empirical approaches that have been employed in this literature to provide this type of causal evidence: (i) using changes in underage drunk driving laws or minimum drinking age laws (see, e.g., Wagenaar and Toomey, 2002; Carpenter and Dobkin, 2009, 2011, 2017), or (ii) using changes in the alcohol trading hour regulations (see, e.g., Biderman et al., 2010; Green et al., 2014; Marcus and Siedler, 2015; Wilkinson et al., 2016; Sanchez-Ramirez and Voaklander, 2018).[8] Each of these approaches generates valuable insights regarding the influence of an important alcohol control policy margin (i.e., restrictions on young adults on the verge of legal adulthood, or restrictions on late-night on-premise drinking or late-night purchases). Collectively, this evidence points towards alcohol control policies being effective in reducing short-run social harms on these margins.

To the best of our knowledge, we are the first to document causal evidence of the short-run

countries (mostly Australia, the United States and Europe), 48% were reported to be under the influence of alcohol. In recent work that explores the causal role of alcohol in victimization more broadly, Bindler et al. (2021) show that obtaining increased access to alcohol at ages 16 and 18 in the Netherlands results in sharp discontinuous increases in the risk of being a crime victim.

[8] An exception to this is Nakaguma and Restrepo (2018), who study the impact of a single-day alcohol sales ban during the 2012 municipal elections in Brazil and find that motor vehicle collisions and traffic-related hospitalizations were reduced by 19% and 17% respectively.

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impact that alcohol consumption has at a societal level in contemporary times. In this, our paper joins a long history of research trying to understand the relationship between alcohol and mortality and morbidity more broadly (see, e.g., Bates, 1918; Emerson, 1932; Warburton et al., 1932, for some early contributions). This work emanates from the contentious social debates of the late nineteenth and early twentieth century in many Western societies, including the United States, about whether allowing alcohol consumption is good for society (Blocker, 2006). A set of more recent studies have tried to estimate the effect that state and federal prohibition statutes enacted in the United States during the early decades of the twentieth century had on mortality and morbidity (Miron and Zwiebel, 1991; Miron, 1999; Dills and Miron, 2004; Owens, 2011; Livingston, 2016; Law and Marks, 2020). This literature portrays a highly ambiguous picture regarding the health and safety impacts of alcohol prohibition. However, in a recent contribution, Law and Marks (2020) argue that they overcome several empirical challenges faced by the prior work and conclude that early prohibition laws enacted between 1900 and 1920 significantly reduced mortality rates in the United States.[9]

Our results are in line with the conclusions of Law and Marks (2020). However, our study differs from the research examining the United States Prohibition era in several important ways. The Prohibition research typically considers a substantially longer time horizon, often using yearly data. This implies that it is evaluating the composite effect of prohibition laws, along with all the social changes that occur as society shifts to a new equilibrium. Additionally, the following considerations suggest that these evaluations are likely to be measuring the influence of alcohol together with other social changes: (i) endogenous community characteristics influenced where dry laws were passed prior to 1920, and the degree to which they were enforced after National

[9]Bhattacharya et al. (2013) reach a similar conclusion in their insightful analysis of the 1985-1988 Gorbachev Anti-Alcohol campaign, showing that the campaign was associated with a marked reduction in mortality during the late 1980s, while the demise of the campaign saw increased mortality in the early 1990s. Interestingly, much of this effect was lagged due to the delayed effect of alcoholism on several health outcomes leading to mortality, e.g. liver cirrhosis and heart disease. Our paper complements their work by providing an analysis of the short-term behavioral impact.

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Prohibition came into force in 1920, (ii) the first decades of the twentieth century constituted a period of substantial turbulence in the prevailing social norms regarding alcohol, and (iii) the gap between prohibition laws being enacted and becoming effective was up to two years (Blocker, 2006; Law and Marks, 2020). In contrast, we use daily mortality data to study the impact of an immediate and unanticipated five-week drop in alcohol consumption. Therefore, the interpretation of our results is complementary but different: our results examine the short-run influence of alcohol on mortality in society as it currently is, rather than the influence of alcohol prohibition policies on medium and long-run mortality after adjusting to the new equilibrium. In addition, society has changed in the last hundred years, which makes it useful to document modern evidence.

This paper also relates to the small body of literature that studies the impact of curfews on crime, which documents mixed results.[10] Last, our results add to the recent work studying the impact of COVID-19 policy responses on crime, violence, morbidity and mortality in South Africa and other countries (e.g. Poblete-Cazenave, 2020; Leslie and Wilson, 2020; Abrams, 2021; Bullinger et al., 2021; Nivette et al., 2021; Navsaria et al., 2021; Moultrie et al., 2021; Chu et al., 2022).[11]

The remainder of the paper is organized as follows: Section 2 describes the data and policy background, Section 3 outlines the empirical strategy we adopt, Section 4 reports the main results and robustness exercises, Section 5 presents an event-study analysis, Section 6 describes the additional results on interpersonal violence, and Section 7 concludes.

[10]Kline (2012) shows that the introduction of a juvenile curfew in Dallas reduced the arrest rate of individuals below the statutory curfew age for both violent and property crimes. In contrast, Carr and Doleac (2018) use variation in the timing of the onset curfews in Washington DC to provide evidence that gunfire *increased* by 150% during the marginal hour (i.e., the first hour of the curfew). Therefore, the existing evidence regarding the effectiveness of curfews is ambiguous—it is not well established whether they increase or decrease crime rates.

[11]In an earlier version of this paper, we also devoted more space to describing the evolution of unnatural mortality during other phases of the policy response to COVID-19 in South Africa (see, e.g., Appendix C in Barron et al., 2020).

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2 Data and the Policy Landscape

2.1 Policy Timeline

The policy change studied in this paper is the introduction of a complete ban on all alcohol sales in South Africa. This change was announced on the evening of Sunday, July 12, 2020 and came into force immediately the following morning on Monday, July 13, 2020 (Government Gazette, 2020b). The explanation provided by the South African government for implementing this policy was to try to free up hospital resources in order to be prepared for potential COVID-19 related hospitalizations (Ramaphosa, 2020). The underlying idea circulating amongst medical professionals was that alcohol-related injuries are responsible for a substantial number of hospital admissions every week in South Africa and, therefore, making alcohol unavailable would reduce the number of such injuries, thereby freeing up hospital resources in the short-run. The ban was unexpected and represented a deviation from the South African government's carefully constructed COVID-19 response plan, which involved a cautious step-by-step scaling back of restrictions from the most extreme policy bundle (Level 5) to the least extreme (Level 1). The alcohol ban was implemented in the middle of the Level 3 period.

To properly interpret the results below, it is important to fully understand the context and policy background. During 2020, South Africa, like the rest of the world, faced the challenge of having to rapidly develop a policy response to try to ameliorate the impact of the COVID-19 pandemic. The South African government's initial response was swift and decisive: on March 27, 2020, South Africa entered a stringent lockdown period that included strict stay-at-home orders (Government Gazette, 2020a). After an initial period of high uncertainty, the government developed a policy response plan that involved a gradual step-by-step relaxation of the strict policy response measures from Level 5 to Level 1. Figure 1 provides an overview of the timeline of policy changes during the period of interest for this paper and Table 15 in the Appendices summarizes the main regulatory changes during each period.

After the initial period of extremely strict Level 5 measures, there was a slight relaxation of policy measures to Level 4 on May 1, 2020, but for much of the general population, this still

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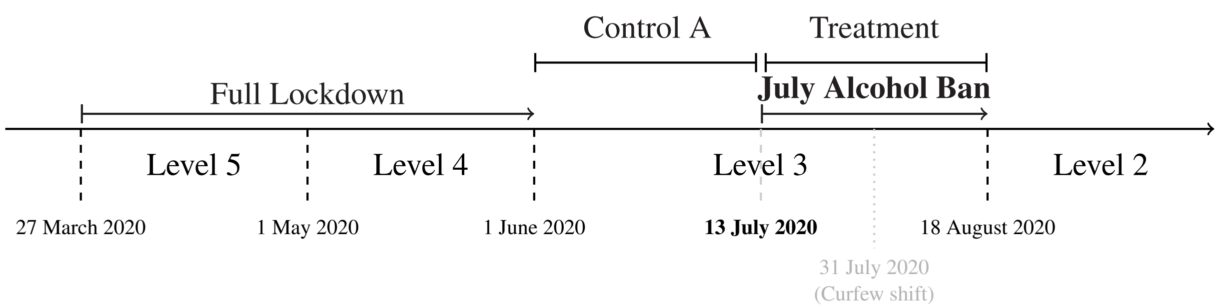
involved a continuation of the state of lockdown. On June 1, 2020, the country entered Level 3, which is the key period of interest for this paper. Level 3 involved a further relaxation of policy restrictions on daily life. The key restrictions in place during Level 3 were the following: (i) off-premises and e-commerce alcohol sales were only permitted from Monday to Thursday between 9AM and 5PM,[12] (ii) there was no official curfew, but individuals were only permitted to leave their house when they had a valid reason (e.g. exercise between 6AM and 6PM, going to work), (iii) gathering in groups was still forbidden, with some exemptions for work or specific religious events. In practise, these restrictions were not easy to regulate and enforce, particularly in areas with informal housing and high-density living conditions.

Figure 1: Timeline of policy events



In the middle of the Level 3 period, on July 13, 2020, the government abruptly introduced a complete ban on the sale of alcohol. Along with this alcohol ban, a curfew from 9PM to 4AM was introduced. Below we provide evidence showing that the curfew is unlikely to have been a key determinant of any changes observed in mortality due to unnatural causes during this period.

We therefore consider the July Alcohol Ban period as our treatment period and evaluate how the level of unnatural mortality was shifted by the introduction of the alcohol ban.

2.2 Data

This paper uses outcome data from two sources. First, in our main analysis, we use national daily mortality data from January 1, 2017 to September 13, 2020. This dataset is collected by the De-

[12]These sales were permitted for businesses holding either an on-premises or off-premises consumption liquor license.

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partment of Home Affairs and curated by the South African Medical Research Council. It contains a record of all deaths of persons with a valid South African identity document (Dorrington et al., 2020). We focus on mortality due to *unnatural* causes. This includes deaths precipitated by road traffic injuries, interpersonal violence, and suicide, but excludes all deaths due to natural causes, such as illness (e.g., COVID-19). Unnatural deaths, therefore, are often caused by risky behavior with immediate consequences. As such, the data allow us to examine how policy changes implemented during 2020 influenced short-run mortality through changes in behavior. In the remainder of the article, all references to mortality refer to mortality due to unnatural causes unless otherwise specified.

Second, in Section 6, we augment our main analysis by investigating interpersonal violence as a potential mechanism driving our unnatural mortality results. We do this using data obtained from the South African Police Services (SAPS) on homicides, assaults and reported rape cases. This data is described in more detail in Section 6.

Figure 2 provides a descriptive illustration of our unnatural mortality data. The bold black line denotes weekly mortality levels due to unnatural causes between March 2020 and February 2021, while the grey lines reflect the same measure for each of the previous three years. The shaded vertical bars denote the three periods during which alcohol bans were implemented, with the July Alcohol Ban the second of these three alcohol bans. The thick dark bar at the bottom of the figure indicates when a curfew was in place (see Table 15 for details regarding the curfew).

The figure reveals several interesting features in the data. First, it is striking how regular mortality patterns are from year to year (prior to 2020). The three grey lines (reflecting 2017, 2018 and 2019) all appear to follow a similar trajectory. Second, the strong Level 5 and Level 4 policy responses, which included a full lockdown as well as an alcohol ban, were associated with a large drop in unnatural mortality in 2020 relative to previous years. Third, a visual inspection of the graph suggests that the introduction of the Level 3 period brought mortality numbers back up to a level slightly below that observed in previous years. The figure also provides suggestive evidence that the introduction of the July Alcohol Ban then reduced the rate of unnatural mortality again. (The aim of the analysis below is to evaluate whether this visual pattern in the raw data reflects a

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robust statistical relationship when subjected to a more rigorous analysis.) Last, the figure suggests that during the first week following each of the alcohol bans, there was a sharp rise in unnatural mortality. For example, this appears to have occurred at the end of the Level 3 period—mortality jumped upwards despite the curfew remaining in place when the alcohol ban was rescinded. This increase in mortality during the first part of the Level 2 period suggests that the curfew was not a crucial reason for the lower mortality levels observed during the July Alcohol Ban.

Our main analysis uses three versions of this unnatural mortality data. The first contains a record of daily mortality levels in the country as a whole.[13] The second dataset is similar, except that it is disaggregated by gender: it contains two observations for every day—one for men and one for women. The third dataset contains unnatural mortality data for the sub-population of individuals aged 15-34 (for completeness, we also report the corresponding results for other age groups). The main reason for examining this sub-population is that young adults are typically viewed as being the group that is most prone to risky behavior and therefore potentially the most affected by the short-run negative outcomes associated with alcohol.

[13]To facilitate the interpretation of the analysis below, it is important to take note of some other empirical regularities observed in the data. In Appendix D, we show that unnatural mortality displays the following patterns. First, the number of daily deaths due to unnatural causes is markedly different for men and women. Between 2017 and 2019, the daily average number of deaths due to unnatural causes was 31 for women and 109 for men. Second, unnatural mortality in South Africa follows a strong and systematic weekly pattern: Mortality is at least 50% higher on Saturdays and Sundays in comparison to weekdays for men, and at least 25% higher for women. Third, there is also variation in mortality according to the day of the month, with higher mortality levels observed at the beginning and end of the month. One potential explanation for these monthly peaks is that they are associated with wage payment days. This monthly cycle is the reason why Figure 2 displays a zigzag pattern in weekly mortality. Fourth, there is some heterogeneity in mortality observed across different months of the year, with the main outlier being December, where higher levels of mortality are observed. In our analysis below, the detailed data that we have from previous years allows us to control for these systematic patterns in mortality.

3 Empirical Strategy

Our empirical strategy utilizes the sudden implementation of the July Alcohol Ban as a natural experiment. In combination with the observation that unnatural mortality follows a highly regular temporal pattern, this allows us to employ a difference-in-difference style estimation approach. Essentially, our main analysis conducts a comparison of the number of unnatural deaths observed during the alcohol ban period (in the second half of Level 3) with the number observed during the period that immediately preceded it (in the first half of Level 3).

In doing this, it is important to isolate the effect of the alcohol ban from unrelated weekly and seasonal changes in behavior. Using detailed mortality data from four years (i.e. 2017, 2018, 2019 and 2020), we do this in three main ways: (1) we control for the systematic variation in mortality using day-of-the-week, day-of-the-month and year fixed effects, (2) we control directly for the baseline mortality level observed during the Level 3 calendar period and alcohol ban calendar period in the preceding three years, and (3) to account for the role played by weekends and the systematic way in which the first and last weekends of the month are characterized by higher levels of unnatural mortality, we flexibly control for weekend effects. In addition, to allow for the fact that the systematic patterns in behavior may have changed in 2020, we interact these weekend effects with 2020 indicator variables in our main analysis. Below, we conduct several robustness exercises to ensure that our results are not driven by our empirical specification.

Our main specification, therefore, removes weekly, monthly, seasonal and yearly time trends that may play a role, allowing us to focus on the difference in mortality observed within the Level 3 period in 2020 before and after the implementation of the alcohol ban.[14] We estimate the following

[14]Difference-in-difference studies typically use a control group that follows the same time trajectory as the treatment group, but that are not affected by the intervention or natural experiment (often due to being in a different geographical location). Here, we instead use detailed information on outcomes observed in previous years in the same geographical location as our control. This approach has also been used in other previous (e.g. Caliendo and Wrohlich, 2010; Schönberg and Ludsteck, 2014) and contemporary (e.g. Leslie and Wilson, 2020; Abrams, 2021) work.

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model using Ordinary Least Squares:

$$M_{y,t,g} = \alpha_0 + \alpha_1 \cdot L3_{y,t} + \alpha_2 \cdot T_{y,t} + \alpha_3 \cdot L3_{y,t} \times Y_{2020} + \beta \cdot T_{y,t} \times Y_{2020} + \boldsymbol{\lambda}_{y,t} + \epsilon_{y,t,g} \quad (1)$$

where $M_{y,t,g}$ refers to the number of daily unnatural deaths in year y on day-of-the-year t in group g (i.e. for a specific gender or age group) and $\boldsymbol{\lambda}_{y,t}$ is a vector of time-related fixed effects that vary across specifications.[15] To control for seasonal mortality, we include two calendar period indicator variables: $L3_{y,t}$, which corresponds to the Level 3 calendar period, and $T_{y,t}$, which corresponds to the July Alcohol Ban calendar period. Importantly, both these variables take a value of 1 for the relevant calendar periods in *all* years in our data (i.e., in the years between 2017 and 2020). We then interact each of these two variables with an indicator variable that takes a value of 1 if the year is 2020. The first interaction variable, $L3_{y,t} \times Y_{2020}$, is crucial for our identification as it controls for the influence of the basket of Level 3 policies that were in place throughout the Level 3 period (which encompasses the July Alcohol Ban period). Controlling for the baseline level of mortality during the Level 3 period in 2020 allows us to use the second interaction variable, $T_{y,t} \times Y_{2020}$, to examine the shift in unnatural mortality that occurred when the July Alcohol Ban was introduced.

Our main coefficient of interest, β, provides an estimate of the impact of the alcohol ban on

[15]The vector $\boldsymbol{\lambda}_{y,t}$ varies across the empirical specifications that we use. In the specification associated with columns (*a) of our results, it is an empty vector. In columns (*b), it includes a set of weekend controls that contains an indicator variable for being a weekend day, the first weekend of the month, the last weekend of the month, and also interactions of each of these weekend variables with a 2020 indicator variable. In our preferred specification, usually reported in columns (*c) of our results tables, $\boldsymbol{\lambda}_{y,t}$ includes day-of-the-week, day-of-the-month and year fixed effects in addition to the weekend controls. Due to the substantial systematic weekly and monthly heterogeneity in mortality described in Appendix Section D, the inclusion of these fixed effects should improve the precision of the estimates. As an illustration, Table 5 in the Appendices provides an example of the results that report the coefficients for the weekend variables in full. Note, in column (*c), the reason that the weekend day variable is omitted is to avoid collinearity with the day-of-the-week fixed effects.

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mortality by estimating the shift in unnatural mortality that occurred when the alcohol ban was introduced. Specifically, β reports the difference between the first and second halves of the Level 3 period in 2020, controlling for the corresponding difference observed in pre-2020 years.[16]

4 Results

4.1 The impact of the alcohol ban on the population as a whole

Table 1 reports our main results. The main coefficient of interest, β, is associated with the interaction variable, *Alcohol Ban Period* $\times$ *Year=2020*, and is reported in bold in the table. Our preferred specification is reported in column (1c) and includes the full set of fixed effects. The results indicate that the alcohol ban reduced unnatural mortality by 21.99 deaths per day (95% CI: 11.39–32.58). Our estimates of the magnitude of the impact of the alcohol ban are similar across the different specifications, but the inclusion of fixed effects substantially improves the precision.

When interpreting these results, there are two additional important considerations to keep in mind. First, it is also worth noting that there is a large estimated relationship between weekends and mortality. Table 5 in the Appendices reports the coefficients for the full set of weekend controls. These results show that: (i) substantially more individuals die from unnatural causes on Saturdays and Sundays in comparison to other days of the week, (ii) this weekend effect is even more pronounced on the first and last weekend of the month, and (iii) these weekend effects were dampened during 2020 (as can also be seen in Figure 15 in the appendices). However, controlling for this weekend effect does not affect the estimated impact of the alcohol ban much. Second,

[16]The interpretation of the other coefficients is as follows: α_1 reports the average difference in unnatural mortality between the first half of the Level 3 calendar period and the rest of the year in pre-2020 years, while α_2 reports the average difference in unnatural mortality between the first and second halves of the Level 3 calendar period in pre-2020 years. The corresponding interaction coefficients for 2020, α_3 and β, report the change in the corresponding objects for 2020 relative to pre-2020 years: α_3 is the additional difference in unnatural mortality between the first half of the Level 3 period and the rest of the year in 2020 relative to pre-2020 years.

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Figure 2 showed that there was a spike in unnatural mortality during the first week of the Level 3 period (i.e., the first week of June 2020). This week forms part of our control period and since this one-week spike in unnatural mortality may have been a reaction to the end of the previous alcohol ban, it could be argued that it is appropriate to also consider an empirical specification that omits this week from the control period. For this reason, we also conduct this exercise, replicating Table 1, but essentially reducing the Level 3 period by one week in our estimation by omitting the first week of June 2020 from the Level 3 period variable. These results are reported in Table 6 in the Appendices. As expected, this slightly reduces the magnitude of our estimate for β, with these results indicating that the reduction in unnatural mortality due to the alcohol ban was 17.96 deaths per day (95% CI: 7.60–28.33). We view it as reassuring that the results are not very sensitive to the inclusion or exclusion of this week and also yield highly consistent estimates across the range of different exercises we conduct (discussed below).

4.2 Heterogeneity by gender

Next, we consider heterogeneity by gender. There are two reasons for this. First, unnatural mortality levels of men and women are very different, with approximately 3.5 men dying from unnatural causes for every 1 woman (see, e.g., Figure 16 in the Online Appendix). Second, the cause-of-death distribution is different for men and women. For example, the ratio of men to women dying from homicides is higher than the ratio of men to women dying from road-traffic injuries (see, e.g., Matzopoulos et al., 2015). Third, we know from the existing literature that men and women display markedly different patterns of drinking behavior in many countries, including South Africa. For example, the WHO (2019) reports that heavy episodic drinking was five times higher amongst men in comparison to women in South Africa in 2016. Together, these factors could lead to a differential effect of the alcohol sales ban by gender.

Table 3 in the Online Appendix reports the estimated impact of the alcohol ban on the unnatural mortality of men and women. For men, the pattern is similar to that observed in the population as a whole, with the estimates indicating that the alcohol ban reduced mortality by approximately 21 deaths per day—our preferred specification in column (1c) reports a reduction of 21.43 (95% CI:

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12.13–30.74). For women, we find no significant impact of the alcohol ban on mortality. As above, we also replicate the results when omitting the first week of June 2020 from the control period (see Table 7) and under this specification estimate that the alcohol ban reduced mortality amongst men by 18.05 per day (95% CI: 8.95–27.15).

4.3 Focusing on younger adults

Young adults comprise a group that is of particular interest when studying the impact of alcohol on short-run outcomes. The reasons for this is that they are typically more likely to engage in risky behavior (e.g. risky drinking). We therefore estimate the impact of the alcohol ban on the sub-population of younger adults between the ages of 15 and 34 years. Table 4 in the Online Appendix reports these results. We find that the alcohol ban reduced mortality amongst men in this age-group by approximately 12 deaths per day, with an estimated reduction of 11.78 (95% CI: 5.49–18.07) in column (1c), and may have had a small impact on the mortality of younger women. An important implication of these results is that the reduction in mortality observed for men of all ages does not seem to be completely due to a reduction in risky behavior by young adults. The 12 lives of younger men saved per day by the alcohol ban is only slightly over half of the 21 male lives of all ages saved per day.[17] When we omit the first week of Level 3 from our control period (see Table 8 in the Appendices), we obtain an estimated reduction in the mortality of young men of 10.24 (95% CI: 3.90–16.58), and no significant impact for young women.

To investigate the relationship between age and the impact of the alcohol ban further, in Figure

[17]However, an important caveat to keep in mind is that the victims of alcohol-related deaths are often not the users themselves (as in the case of interpersonal violence and motor vehicle collisions). Therefore, the demographic characteristics of the individuals engaging in the risky behavior may not always correspond to the demographic characteristics of the individuals who are affected by the behaviour. Examining the change in mortality amongst young adults may not reflect the true aggregate impact of any change in the behaviour of young adults. This externality of alcohol consumption illustrates the importance of examining the impact of changes in alcohol consumption on society as a whole, as opposed to focusing on the particular sub-population.

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6 in the Appendices, we compare the age distribution of unnatural mortality during the 5 weeks preceding the alcohol ban with the age distribution during the 5 weeks of the alcohol ban. This figure suggests that the majority of the decrease in unnatural mortality due to the alcohol ban was observed for individuals between 18 and 35 years of age. However, it appears to show that there was also a decrease in unnatural mortality for individuals over 35. Tables 9, 10 and 11 in the Appendices provide further evidence by replicating the empirical analysis used in Table 4 for other age groups. These results are largely in line with the suggestive evidence from Figure 6: (i) for individuals aged 14 or younger, we find no effect of the alcohol ban on mortality, (ii) for individuals between 35 and 54 years, we estimate that the alcohol ban resulted in 6.7 fewer male deaths per day, with a possible smaller reduction in the mortality of women by 1.3 deaths per day, (iii) for individuals over 55 years, we estimate that there were 2.5 fewer male deaths per day, and 1.6 additional female deaths per day during the alcohol ban period.[18] Overall, the results indicate that most of the effect of the alcohol ban on unnatural mortality was concentrated amongst younger male adults, with a smaller, but still sizable, impact also observed amongst middle aged men.

4.4 Robustness exercises

To provide support for the validity of these findings, we conduct several robustness exercises. These exercises, and the associated results, are discussed in detail in Section C of the appendices.

The first two exercises address concerns regarding the suitability of the natural experiment for providing causal evidence on the impact of reducing alcohol consumption (see Section C.1). To do this, we show that the primary candidate confounding factors were unlikely to have contributed to the observed reduction in unnatural mortality. Aside from the alcohol ban, the two main sources of behavioral change in society during the period we are studying were: (i) the COVID-19 pandemic and (ii) the associated changes in regulation. We reason that fear of COVID-19 was unlikely to have caused a reduction in unnatural mortality during the period of the July Alcohol Ban since the

[18]It is unclear to us why the alcohol ban may have resulted in an increase in mortality due to unnatural causes amongst women over 55 years of age. One speculative potential explanation is that some COVID-19 related deaths were misclassified as unnatural deaths.

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number of daily confirmed COVID-19 cases was dropping rapidly. We also evaluate the possibility that the main contemporaneous regulatory change, namely the introduction of a curfew, influenced unnatural mortality. To do this, we make two observations. First, we note that when the July Alcohol Ban ended, the curfew remained in place. Figure 2 shows that unnatural mortality increased sharply at this point in time and remained at pre-2020 levels despite the ongoing curfew. Second, we estimate the impact of a one hour reduction in the curfew length which occurred in the middle of the July Alcohol Ban period. We show that it did not have a statistically significant impact on unnatural mortality. These observations on both the extensive and intensive margin support our assessment that the curfew was unlikely to have been a key factor in reducing mortality during the July Alcohol Ban.

The next four exercises check that our results are not driven by the particular empirical strategy that we adopt nor by anomalies in the data (see Section C.2). First, we run a set of placebo regressions. Essentially, this involves replicating our main analysis, but replacing 2020 with 2019 as our treatment year and using 2016 to 2018 as our comparison years. As expected, the coefficients associated with the interaction term of interest, *Alcohol Ban Period* $\times$ *Year=2019*, are no longer statistically significant.

Second, we examine whether our results are sensitive to the precise choice of time window used for our estimation. To do this, we conduct an additional robustness exercise where we vary the length of the treatment-control time window around the introduction of the alcohol ban used in our analysis. Instead of including a indicator variable for the entire Level 3 period, we consider time windows of between 2 weeks and 5 weeks in length. We find that the estimated impact of the alcohol ban remains fairly stable when considering windows of 5 weeks, 4 weeks and 3 weeks in length. The only exception to this is that when we use a very narrow window of only 2 weeks in length, we no longer observe a significant coefficient estimate.[19]

[19] In Section C.2.2, we discuss several potential explanations for this, including the important consideration that the two week period prior to July 13 normally includes a payday weekend (with the associated inflated mortality levels), while the two week period afterwards does not. Given the very short time window being considered, this imbalance between the treatment and control period

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Third, we consider alternative approaches to calculating the standard errors and drawing inference from our regressions. Specifically, we conduct a series of exercises that reproduce the results from Table 3 but relax the assumptions on the error structure to allow for serial correlation in the error term. The results from these exercises are discussed in detail in Section C.2.3 and report (i) standard errors estimated by clustering at the calendar-week level, (ii) standard errors calculated using the Newey-West (1987) variance estimator that allows for autocorrelation up to a pre-specified lag length, and (iii) p-values for the main coefficient of interest, calculated using the wild cluster bootstrap to correct for the small number of clusters when clustering at the year level (Cameron and Miller, 2015; Roodman et al., 2019). These exercises yield estimates that are all consistent with the findings reported above.

Last, we replicate our main results, but restrict the dataset to only contain observations during the Level 3 calendar period. Therefore, we use data from the years 2017 to 2020, between 1 June and 17 August of each year, and estimate the following simplified version of our main estimation equation:

$$M_{y,t,g} = \alpha_0 + \alpha_1 \cdot T_{y,t} + \beta \cdot T_{y,t} \times Y_{2020} + \boldsymbol{\lambda}_{y,t} + \epsilon_{y,t,g} \tag{2}$$

The point estimates from our preferred specification, which includes fixed effects, are very close to those in our main results.[20]

Collectively, we view these six exercises as providing strong support for the validity of the results discussed above.

could provide an explanation for absence of a significant estimate if our weekend and fixed effect controls are not perfectly accounting for this imbalance.

[20]The results from the other specifications are also largely in line with our main results, but the point estimates are less stable across specifications. The main difference between these results and the results from our main estimation approach is that we observe a significant impact of the alcohol ban on female mortality under specifications that don't include fixed effects. For the reasons discussed above, we view the results with fixed effects as being more trustworthy. See Section C.2.4 for further details.

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5 Event study analysis

In this section, we augment our main analysis by using an event-study design to examine how unnatural mortality levels evolved week-by-week in the period before and after the implementation of the July Alcohol Ban. This generalization of the difference-in-difference style empirical approach used above involves using indicator variables for each of the lag and lead weeks around the event of interest (see, e.g., Schmidheiny and Siegloch, 2019; Clarke and Tapia-Schythe, 2021). One benefit of the event-study design is that it can help to provide an informative illustration of where the estimated aggregate effect is coming from. For example, it can show whether the effect is driven by a large shift in mortality in a single week or a smaller shift spread across several weeks. It can also provide an indication of whether any dynamic effects are present. Below, we provide estimates for both the *transition into* the July Alcohol Ban on July 13, 2020, and also the *transition out* of the July Alcohol Ban on August 18, 2020.

For the transition into the alcohol ban, which is our primary focus, our event-study analysis considers the eleven weeks of the Level 3 period, comprising the six weeks before the July Alcohol Ban and the five weeks in which the July Alcohol Ban was in force. Following Schmidheiny and Siegloch (2019), we refer to this eleven week period as the *effect window*. The central idea is to fix one of the weeks as the benchmark, and to compare the level of the outcome of interest (unnatural mortality, $M_{y,t,g}$) in each of the other nearby weeks to the benchmark level. To estimate our event-study, we therefore adjust the empirical strategy described above in equation 1 by including indicator variables for each of the weeks in the effect window to arrive at the following specification:

$$M_{y,t,g} = \alpha_0 + \sum_{j=\underline{j}}^{\overline{j}} \beta_j b^j_{y,t} + \boldsymbol{\mu}_{y,t} + \boldsymbol{\lambda}_{y,t} + \epsilon_{y,t,g} \tag{3}$$

where $\boldsymbol{\lambda}_{y,t}$ is a vector of time-related fixed effects (as in equation 1 above), $\boldsymbol{\mu}_{y,t}$ contains indicator variables for the Level 3 calendar period ($L3_{y,t}$) and the July Alcohol Ban calendar period ($T_{y,t}$) in all years (i.e., not only 2020), and $b^j_{y,t}$ is a treatment indicator that takes a value of one when the

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event of interest, e, namely the start of the July Alcohol Ban, takes place $j \in [\underline{j}, \overline{j}]$ weeks away from $w(t)$, where $w(t)$ refers to the week in which day t occurs.

$$b^j_{y,t} = \begin{cases} \mathbb{1} & \text{if } y \leq 2019 \ \& \ j = \underline{j} \\ \mathbb{1}[w(t) \leq e + j] & \text{if } y = 2020 \ \& \ j = \underline{j} \\ \mathbb{1}[w(t) = e + j] & \text{if } y = 2020 \ \& \ \underline{j} < j < \overline{j} \\ \mathbb{1}[w(t) \geq e + j] & \text{if } y = 2020 \ \& \ j = \overline{j} \end{cases} \tag{4}$$

Following the standard event-study approach, the indicator variables at the endpoints of the effect window serve to bin together all observations that occur outside the effect window (see, e.g., Schmidheiny and Siegloch, 2019). Put simply, in our setting, all observations that occurred seven weeks or more before the July Alcohol Ban (including all observations from 2019 or earlier) are binned together, with $b^{\underline{j}}_{y,t} = b^{-7}_{y,t} = 1$ for these observations. Similarly, all observations that occurred six weeks or more after the July Alcohol Ban are binned together and $b^{\overline{j}}_{y,t} = b^{6}_{y,t} = 1$ for these observations. For the weeks within the eleven week effect window, $b^j_{y,t} = 1$ when an observation occurs in a week that is j weeks away from the onset of the July Alcohol Ban (with negative values denoting weeks before the ban, and positive values denoting weeks after the ban). Importantly, since the transition into the July Alcohol Ban occurred between two weeks (i.e., between Sunday and Monday), we define $j = \{-7, ..., -1, 1, ..., 6\}$, with $j = -1$ denoting the last week before the ban and $j = 1$ the first week after the onset of the ban, so there is no period $j = 0$. In our analysis of the transition into the ban, the benchmark week is $j = -1$.[21]

[21]For all the observations in 2020, using the normalization that the event of interest occurs in week 0 (i.e., $e = 0$), we can rewrite equation 4 more simply as follows:

$$b^j_{y,t} = \begin{cases} \mathbb{1}[w(t) \leq -7] & \text{if } j = -7 \\ \mathbb{1}[w(t) = j] & \text{if } -7 < j < 6 \\ \mathbb{1}[w(t) \geq 6] & \text{if } j = 6 \end{cases}$$

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The results from our event study analysis for the transition into the July Alcohol Ban are displayed in the left-hand panels of Figures 4 and 5 in the Online Appendices.[22] Figure 4 reports the results corresponding to specification (*b) of our main regressions, including a range of weekend controls. Figure 5 corresponds to specification (*c), which additionally includes a full set of calendar fixed effects (see footnote 15 for details). In both figures, the top-left panel reports the results for men, while the bottom-left panel reports the results for women.

These results for the transition into the alcohol ban reveal several insights. First, the high coefficient estimate for men in week $j = -6$ is consistent with the observation that there was a jump in unnatural mortality during the first week of June 2020 (recall that $j = -6$ is the first week after Alcohol Ban 1, which lasted over two months). This jump in unnatural mortality can also be seen clearly in the raw data in Figure 2. Second, for men, all the point estimates in weeks during the July Alcohol Ban (i.e., $j > 0$) in both specifications are negative.[23] Therefore, these results are consistent with the aggregate level results above that pool the weeks together. Third, the lower left-hand panels of both figures show that the coefficient estimates for women are almost completely flat and close to 0 (only β_{-2} in Figure 5 is statistically different from zero at the 10% level). This

[22]Note, the figures report the coefficient estimates for the weeks within the event window. We exclude the coefficients for the two endpoints ($j = -7$ and $j = 6$) from the figures as they are not very informative for evaluating the event-study transition. The interpretation of the binned coefficient at the start of the event window is the difference in unnatural mortality between the benchmark week ($j = -1$) and the average level in *all* weeks prior to the event window. The interpretation of the binned coefficient at the end is similar.

[23]It is worth noting that in specification (*c), but not specification (*b), the point estimate for β_{-2} also appears to be negative (although 0 still lies within the 95% CI). One potential reason for this negative point estimate two weeks prior to the July Alcohol Ban is that this week includes the first weekend of the month. If our calendar fixed effects do not perfectly account for all the variation in unnatural mortality due to payday effects, this could make the estimates for payday weekends more sensitive to the choice of specification. This is perhaps supported by the observation that for women in the lower-left panel of Figure 5 the only coefficient with a 95% confidence interval lying fully below zero is also week $j = -2$.

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null result for women is highly informative as it shows that there was no change in the causes of unnatural mortality that affected women when the July Alcohol Ban was introduced. This implies that the results that we observe for men must be driven by some change in the causes of unnatural mortality that affect men differently from women. Since alcohol consumption patterns in South Africa are highly gendered, this provides further support for the idea that the reduction in alcohol consumption is the likely explanation for the effect we observe.

The right-hand panels of Figures 4 and 5 repeat the event-study analysis for the *transition out* of the July Alcohol Ban. Here, we define the effect window as the five weeks of the July Alcohol Ban plus the five weeks following the alcohol ban. We now assign the benchmark period to be the first week after the end of the alcohol ban ($j = 1$), so that it is again the closest week lying outside the alcohol ban period. Before discussing these results, it is important to re-iterate that our main source of identification in this paper comes from the transition into the alcohol ban, since this was unexpected and occurred in the absence of other substantial policy changes. By contrast, the transition out coincided with other changes to regulation (i.e., the relaxation from Level 3 to Level 2: see Table 15 for details). Nevertheless, we view this exercise of examining the transition out of the ban as being useful for the following reasons. First, even though other regulations changed when the alcohol ban was lifted, it still provides a useful check of whether unnatural mortality increased when alcohol became available again. This is informative as it indicates that any unknown change in society influencing mortality that may have occurred when the alcohol ban was initiated would have also needed to be reversed at the same time as the alcohol ban ended to generate the down-up pattern in unnatural mortality that we observe. Second, the curfew that was initiated at the same time as the July Alcohol Ban remained in place when the alcohol ban was lifted, so the transition out provides a test of whether the curfew alone reduced unnatural mortality. If it did, then one would expect unnatural mortality to remain low when the alcohol ban was lifted and the curfew remained in place.

Examining the top right-hand panels of Figures 4 and 5 shows that for men the transition out of the July Alcohol Ban was associated with a step-up in unnatural mortality. These results are consistent with our main results, and also provide evidence that the curfew alone was not the main

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driver of the reduction in unnatural mortality observed during the July Alcohol Ban. Turning to the bottom-right panels of the two figures, we see that for women in contrast to the transition into the alcohol ban, which was not associated with a change in mortality, the transition out was associated with an increase in unnatural mortality. This is consistent with the idea that the change in unnatural mortality when transitioning out of the alcohol ban was driven by both the re-availability of alcohol and also by the relaxation of other regulations that occurred when moving from Level 3 to Level 2. This is also consistent with the observation that the event-study coefficient estimates for men in Figures 4 and 5 are larger in magnitude (more negative) during the transition out in comparison to the transition in. Examining the raw data in Figure 2 shows this even more clearly.

Overall, we interpret the results of this event-study analysis to be highly supportive of our main results. However, as a caveat, it is important not to place too much weight on the estimates for each individual week in our event-study analysis because: (i) the coefficient for each week is estimated from a small number of observations, and (ii) as we see from comparing Figures 4 and 5, there is some sensitivity in the estimates to the precise choice of empirical specification.

6 Crime: interpersonal violence as a mediator

One of the primary causes of injury-induced mortality is interpersonal violence. Therefore, to shed light on one potential mechanism that could be driving our main results, in this section we examine the impact of the July Alcohol Ban on three outcomes related to interpersonal violence. To do this, we use data collected by the South African Police Service (SAPS). This data contains the daily number of reported contact crimes in South Africa during the three month period between June 1, 2020 and August 31, 2020 in three categories: homicides, assault with intent to inflict grievous bodily harm (GBH), and rape. Figure 7 in the Online Appendices displays the raw data. This figure reveals that each of these three outcomes followed a very similar week-by-week trajectory around the July Alcohol Ban to that observed for unnatural mortality. Specifically, in the very first week of June, which is also the first week following Alcohol Ban 1, we observe an elevated level of each of the three outcomes. Thereafter, in the next five weeks (which immediately precede the July

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Alcohol Ban) each of the outcomes stays relatively flat, with a slight increase around the change of month between June and July.[24] At the onset of the July Alcohol Ban, all three outcomes drop to a lower level for the duration of the ban, and when the ban is lifted, we again see a large jump upwards in all three outcomes. Therefore, the raw data suggests that the July Alcohol Ban affected each of these three outcomes similarly to how it affected injury-induced mortality.

To assess whether the pattern observed in the raw data reflects a statistically significant shift in each of the outcomes at the onset of the July Alcohol Ban, we use a simplified version of our empirical strategy from above (simplified due to the reduced data availability).[25] Using only the eleven weeks and one day of the Level 3 period between June 1, 2020 and August 17, 2020, we estimate the following specification for each of our three contact crime outcomes and also for unnatural mortality:

$$Z_t = \alpha_0 + \beta \cdot T_t + \boldsymbol{\kappa}_t + \epsilon_t \tag{5}$$

where Z_t refers to outcome Z at time t, T_t is an indicator variable that takes a value of one during the July Alcohol Ban, and $\boldsymbol{\kappa}_t$ is a vector of indicator variables that control for the effect of weekends on each of the outcomes.[26]

[24]To check more formally for the presence of a statistically meaningful pre-trend, we conduct an additional analysis using only data from this five-week period before the July Alcohol Ban in which we regress each of the outcomes on a week counter that takes values from -5 (the fifth week before the ban) to -1 (the week before the ban). These results are reported in Table 12 of the Appendices. If the drop in the outcomes observed at the start of the July Alcohol Ban were part of an existing pre-trend, we would expect to see a negative coefficient on the *Week Counter* variables in Table 12. However, we do not observe a statistically significant negative coefficient for any of the four outcomes. This evidence suggests that the estimated drop in all four outcomes at the start of the July Alcohol Ban is not a continuation or exacerbation of an existing pre-trend.

[25]The reason for not including the full set of fixed effects considered above is to avoid overfitting, since here we use a smaller sample consisting of 78 observations (days).

[26]In specification (*2) in Table 2, $\boldsymbol{\kappa}_t$ contains three indicator variables, *Weekend Day*, which takes a value of one for Saturdays and Sundays, *First Weekend of Month* and *Last Weekend of*

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The results are reported in Table 2. They show that there was a statistically significant drop in all of our outcomes of interest when comparing the July Alcohol Ban period to the preceding six weeks. However, as discussed in the main results above, one important caveat to these results is that this six week comparison period includes the first week of June, which saw a jump in all of our outcomes of interest. Therefore, we also replicate the analysis reported in Table 2, excluding this first week of June. These replication results are reported in Table 13 in the Online Appendices and also show a statistically significant drop in all four outcomes, but of a slightly smaller magnitude. The first two columns of Tables 2 and 13, labelled (H*), estimate that the number of homicides was reduced by 11 [15] per day, depending on the specification, during the July Alcohol Ban period in comparison to the five [six] week preceding period. This reduction represents a substantial fraction of the 54 [58] daily homicides that occurred on average during the five [six] weeks leading up to the July Alcohol Ban. It also suggests that a large proportion of the 18 [22] fewer daily unnatural deaths during the July Alcohol Ban period can be attributed to this reduction in interpersonal violence.

The next four columns, (A*) and (R*), show that there was also a sharp drop in assaults (with intent to cause GBH) of 113 [142] per day, and in reported rape cases of 15 [19] per day. The final two columns, (U*), provide another robustness check for unnatural mortality, since the specification here is the simplest one used in the paper, making use of data from only a 10 [11] week period during Level 3 in 2020. It is therefore reassuring that the results we observe from this simple specification are highly consistent with the main results reported above.

To illustrate the dynamic effects in each of these four outcomes around the onset of the July Alcohol Ban, we adjust the specification in equation 5 by replacing T_t with indicator variables for each of the eleven weeks in the Level 3 period. This allows us to conduct a simple version of the event-study analysis that we used above.[27] Figure 3 displays the coefficients from this analysis for

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Month, which indicate whether it is the first or last weekend in the month. In specification (*1), $\boldsymbol{\kappa}_t$ is an empty vector.

[27]Note, in contrast to the specification used in our event study in Section 5, here we only use data from the eleven weeks, so there are no binned categories at the end points. We, therefore, include the indicators for weeks $j = -6$ to $j = -2$, and $j = 1$ to $j = 5$, with week $j = -1$ serving as the

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each of the four outcomes. Overall, the figure shows a relatively sharp drop for all four outcomes at the onset of the July Alcohol Ban, providing support for our main findings.

7 Concluding discussion

In this paper we have documented evidence that a five-week-long nationwide ban on the sale and transport of alcohol resulted in a reduction of at least 14% of all unnatural deaths during that period. This is a large and meaningful number of lives saved. We have also shown that the alcohol ban led to a sharp drop in violent crimes, suggesting that the relationship between alcohol and aggressive behavior is one of the key mechanisms driving our mortality results. Our findings provide unique causal evidence on the impact that a short-term *absence* of alcohol can have in a society; or rather, perhaps much more importantly, they provide a clear illustration of the impact that the *presence* of alcohol has on society every day. They demonstrate that alcohol can substantially increase the amount of behavior-induced harm observed in the population.

There are several important considerations that should be kept in mind when interpreting our results. First, it is important not to extrapolate from these results to try to infer the impact that a longer ban on alcohol would have on mortality. The alcohol ban that we evaluate lasted only five weeks. In the presence of a longer ban, society would shift to a new equilibrium, which may involve legally acquired alcohol being replaced by illegally acquired or homemade alcohol, or a substitution to other recreational drugs. Therefore, our results should not be taken as evidence that prohibition works well, but rather as evidence of the magnitude of harm generated by alcohol in society. They illuminate the substantial benefits to society that can be achieved by carefully implementing policies that will successfully curb alcohol consumption in the long-run—policies other than a complete prohibition on alcohol sales may well be more effective avenues for pursuing this objective.[28]

omitted benchmark week (i.e., we include indicator variables for 10 weeks, and omit an indicator for one week to serve as the benchmark).

[28]The World Health Organization has proposed five such intervention strategies as part of its SAFER initiative (WHO, 2018).

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Second, our estimates of the impact of the alcohol sales ban likely constitute a lower bound on the true impact of alcohol on short-run unnatural mortality in South Africa. The main reason for this is that the July Alcohol Ban occurred against the backdrop of COVID-19 which implies that during our control period (the first half of the Level 3 period), people were more likely to be at home and less likely to be going out to bars and restaurants in comparison to the same period in previous years (e.g., see the Google mobility trends in Figure 10 in the Appendices, which also shows that there was not a sharp change in these trends at the onset of the alcohol ban). This depressed level of social activity translated into a lower benchmark level of unnatural mortality in our main control period in comparison to the same period in previous years (see Figure 2). Therefore, we are comparing the outcomes observed during the July Alcohol Ban period to an already lowered base level of these outcomes, which suggests that our estimated effect sizes are likely to be smaller than they would be if an alcohol ban were implemented during a year with more social activity.[29] Importantly, since the effect sizes that we observe are still so large, these lower bound estimates provide valuable information, pointing towards an even larger influence of alcohol when social activity is at normal pre-COVID levels.[30]

[29] An additional reason why our estimates likely constitute a lower bound on the true effect size of the presence of alcohol in society is that, according to media reports, compliance with the alcohol sales ban was imperfect. Some examples of the media reports include articles in the Guardian (2020) and the Economist (2020).

[30] One concern that can be raised is that alcohol consumption may have been higher-than-normal during the COVID-19 pandemic outside of the alcohol bans, which would potentially increase the effectiveness of an alcohol ban relative to other years. There are two pieces of evidence that suggest that this was not the case. First, Figures 8 and 9 in the Appendices show that monthly alcohol production and sales were at approximately the same level in 2020 as in 2019 during the months when the alcohol bans were not in place and dropped during the bans. This is suggestive evidence that alcohol consumption was roughly normal during 2020 outside of the ban periods. Second, if alcohol consumption were inflated during 2020 then we would expect to observe inflated levels of unnatural mortality during our control period relative to previous years. Instead we observe the reverse (see Figure 2).

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Third, the absence of an estimated impact of alcohol on female mortality should not be taken as evidence that women are less affected by the presence of alcohol in society than men. While women drink substantially less than men in many societies around the world (including South Africa) they are often the victims of alcohol-related harms. Our results showing that the number of reported rape cases dropped during the July Alcohol Ban illustrate this. It should also be kept in mind that there are many other forms of gender-based violence that do not result in either death or a reported rape case that fall outside the scope of this paper.

Fourth, while the evidence that we report on homicides and assaults suggests that a large part of the influence that alcohol has on mortality due to unnatural causes is mediated by alcohol-induced aggressive behavior, it is important not to neglect road-traffic collisions as another important potential channel through which alcohol can induce injury and death. Unfortunately, at the time of writing this paper, it was not possible for us to obtain data that would allow us to study this channel directly. However, in the future more detailed cause-of-death data may become available and could be used retrospectively to provide direct evidence on the effect that the ban had on road-traffic collision mortality. Given the background pandemic context, an important consideration to keep in mind when thinking about road-traffic collision deaths during this period is that it is not entirely clear a priori whether the lower-than-usual level of traffic volume and road congestion would increase or decrease the number of road-traffic collision fatalities. It is plausible that emptier roads can result in more speeding and more deaths (for example, in the United States there were more motor vehicle deaths and lower traffic volumes during the period from June 2020 to December 2020 in comparison to the same period in 2019, according to preliminary estimates by the National Safety Council).[31] However, in the South African context there are two reasons to believe that road-traffic fatalities were lower than usual during our control period. First, Navsaria

[31]For further details, see Bolotnikova (2022) and National Safety Council (2022). However, it is also worth noting that this pattern of behavior in the United States is not representative of the evidence from around the world during the pandemic. Yasin et al. (2021) review the evidence on traffic volumes and fatalities during April 2020 from 36 countries and find that 32 of those countries experienced a reduction in road deaths in comparison to 2019.

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et al. (2021) show that during June 2020 (the first four weeks of our control period) the number of trauma patients admitted due to a road traffic collisions was 32% lower than pre-COVID levels in a large tertiary urban trauma centre in Cape Town, South Africa. Second, Figure 2 shows that the level of unnatural mortality was lower-than-usual during our control period (relative to previous years), indicating that if road-traffic fatalities were higher-than-usual, there would have needed to be a much larger drop in some other cause of unnatural mortality. Taken together, the available evidence suggests that road-traffic fatalities were likely lower-than-usual in our control period, which implies that there was less scope for reducing road-traffic fatalities by banning alcohol than would normally be the case. This further supports the idea that we are estimating a lower bound on the impact of alcohol on unnatural mortality.

The results discussed above raise important questions regarding the optimal design of alcohol control policy. In particular, since several of the social harms we study (e.g., homicide, assault, rape) involve the cost of an action being borne by another individual in society, there seems to be a potential mandate for policy intervention. However, policy discussions surrounding optimal alcohol control are complex as they involve taking a global perspective and balancing all of the social benefits of alcohol consumption (which are non-trivial to measure) against the large set of potential short-term and long-term social costs (which are also typically difficult to causally estimate).[32] Welfare analysis is further complicated by the fact that some of the costs of alcohol consumption are borne by the individual themselves, such as the influence on their long-term health and cognitive functioning, and on the quality of their short-term decision-making. A fully rational model would ascribe a limited role for policy intervention if these were the only costs, since the individual consuming alcohol is assumed to be factoring in these costs when they maximize their own

[32]For example, the range of potential social harms includes outcomes such as the emotional abuse of family-members and the loss of utility due to poor decision-making (which are difficult to fully measure) as well as long-run outcomes, such as liver cirrhosis, cardiovascular diseases, cancers and mental health outcomes (for which it is challenging to construct a precise causal attribution to alcohol consumption). This makes it very difficult to fully evaluate the aggregate social cost of alcohol consumption.

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lifetime utility and decide to consume alcohol. However, a behavioral model that allows for hyperbolic discounting, (non-rational) addiction, bounded rationality or imperfect foresight regarding the future costs of consuming alcohol would permit a role for welfare-enhancing intervention that assists individuals in overcoming their own behavioral biases (see, e.g., Simon, 1984; Ainslie, 1991; Orphanides and Zervos, 1995; Laibson, 1997; Rubinstein, 2003). Consequently, there is an entire literature devoted to the design of optimal alcohol control (see the Handbook chapter by Cawley and Ruhm, 2011, for a discussion of traditional and behavioral economics models of risky behavior, and their implications for the design of policy interventions). In relation to the current paper, the discussion in Carpenter and Dobkin (2011) provides the most useful benchmark case of welfare analysis and their discussion highlights the numerous challenges faced in making progress when taking a general perspective that weighs up all the costs and benefits in an exercise of this nature.

Here, we have opted not to try to take a general perspective that considers all the costs and benefits, but rather highlight what we view as the main policy lessons of our results. One of the key take-aways from our results is that alcohol consumption can play a pivotal role in inducing aggressive behavior in society at a significant scale, resulting in substantial harm.[33] When thinking about the policy implications of this, it is important to consider that the evidence from the existing literature, along with the pattern of drinking observed in South Africa (where heavy drinking is

[33]One way to think about the magnitude is in terms of the value of a statistical life (VSL) as discussed by Viscusi and Aldy (2003) (bearing in mind all the substantial caveats that are implicit in assigning a monetary value to a human life). Converting the estimates from Viscusi and Aldy (2003) of the VSL for the United States to 2020 US$ gives $10.52 million. Since the per capita average income is approximately 11 times lower in South Africa, one perspective is to view $1 million as the appropriate value for thinking about the monetary VSL in our paper. This implies a cost to society, in South Africa, of at least $115 million per week due to deaths resulting from alcohol consumption. This estimate does not account for the costs due to any of the other harms not resulting in death (e.g., assaults, rape, long-run deleterious health outcomes). In addition, it is important to keep in mind that short-run alcohol-related deaths tend to be amongst younger individuals.

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common amongst those who drink), points towards heavy drinking, as opposed to social drinking, as a key driver of this aggressive behavior (Duke et al., 2011; Kuhns et al., 2014; Tomlinson et al., 2016; Matzopoulos et al., 2021). This implies that a society that wishes to alleviate these short-run harms from alcohol consumption should start by targeting a reduction in the alcohol consumption of the heaviest drinkers in society. This provides a clear principle to guide which policy levers to prioritize. For example, one candidate policy lever that has been proposed to reduce heavy drinking is the use of minimum unit pricing (MUP). The rationale for this is that the heaviest drinkers typically spend the least per unit of pure alcohol and will be the group that reduces their consumption most in response to an increase in the floor price of a standard drink (Holmes et al., 2014; O'Donnell et al., 2019; van Walbeek and Chelwa, 2021; Gibbs et al., 2021). As noted in the introduction, South Africa is not at all unusual in terms of the proportion of the adult population that engage in heavy episodic drinking, with many countries around the world observing a higher proportion of the population engaging in 'binge-drinking'. While other background social factors may influence the way that intoxication manifests in behavior, potentially making South Africa a better model for countries facing similar social issues (e.g., Brazil, Russia), the results in this paper should serve as a warning for all countries with substantial levels of heavy drinking.

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TABLES AND FIGURES IN PAPER

Figure 2: Weekly mortality (unnatural deaths, all ages)

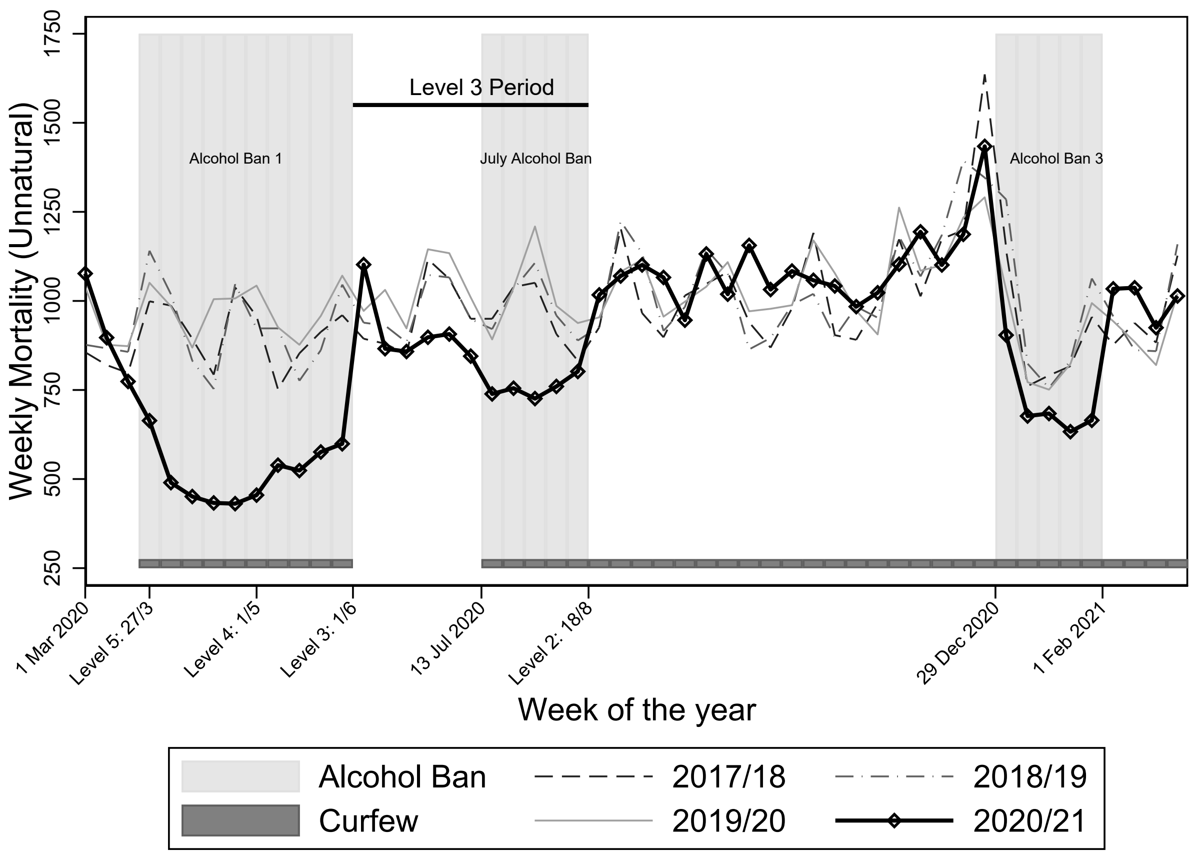

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Table 1: Impact of the alcohol ban on mortality (entire population)

	(1a)	(1b)	(1c)
Level 3 Period = 1	6.88	3.00	1.75
(1/6-17/8)	(5.14)	(2.80)	(2.20)
Alcohol Ban Period = 1	-2.17	-0.28	0.91
(13/7-17/8)	(6.88)	(3.31)	(2.73)
Level 3 Period x Year=2020	-13.46**	-2.29	-1.78
	(6.19)	(4.32)	(4.51)
Alcohol Ban Period x Year=2020	-20.93**	-21.55***	-21.99***
	(8.41)	(5.30)	(5.40)
Constant	136.96***	115.14***	130.76***
	(1.56)	(1.13)	(4.42)
Weekend Controls		Y	Y
Day of Week FEs			Y
Day of Month FEs			Y
Year FEs			Y
Observations	1460	1460	1460
Adjusted R^2	0.008	0.524	0.599

Notes: (i) Each observation contains unnatural mortality data for a single day, (ii) Heteroskedasticity robust standard errors (Huber, 1967; White, 1980, 1982) are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between January, 1 and December, 31 of each year, excluding February, 29, (iv) All three columns report estimates of the impact on unnatural mortality, and differ only in their specifications, with column (*a) the simplest specification, (*b) adding controls for the weekend, and (*c) adding fixed effects. The weekend controls include an indicator variable for a weekend day, an indicator for the first weekend of the month, and one for the last weekend of the month. They also include all three variables interacted with a 2020 indicator variable. In column (*c), the weekend day indicator is excluded due to the Day of Week FEs.

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Table 2: Impact of the alcohol ban on criminal offences and mortality

	Homicide		Assault (GBH)		Reported Rape		Unnatural Mortality	
	(H1)	(H2)	(A1)	(A2)	(R1)	(R2)	(U1)	(U2)
Alcohol Ban Period = 1	-15.59***	-15.06***	-148.94***	-142.15***	-20.23***	-19.28***	-23.10***	-22.28***
	(3.12)	(2.45)	(27.38)	(18.29)	(5.15)	(4.01)	(4.88)	(3.68)
Weekend Day = 1		11.17***		146.74***		26.83***		23.14***
		(2.54)		(17.37)		(5.07)		(4.17)
First Weekend of Month = 1		14.41***		149.39***		17.87*		19.04***
		(3.72)		(47.31)		(9.52)		(7.11)
Last Weekend of Month = 1		16.92**		37.00		-3.08		14.75**
		(6.44)		(45.47)		(9.23)		(7.09)
Constant	57.81***	52.44***	373.50***	315.59***	83.12***	73.90***	130.38***	121.26***
	(2.71)	(2.45)	(24.62)	(19.16)	(3.93)	(3.77)	(3.83)	(3.33)
Observations	78	78	78	78	78	78	78	78
Adjusted R^2	0.221	0.528	0.250	0.657	0.153	0.484	0.209	0.554

Notes: (i) Each observation contains the total number of cases for the relevant outcome for a single day, (ii) Heteroskedasticity robust standard errors (Huber, 1967; White, 1980, 1982) are reported in parentheses $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$, (iii) The estimation uses data from 2020, between June, 1 and August, 17 (iv) The outcome of interest is described in the column header, with two specifications estimated for each of the four outcomes.

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Figure 3: Event study: Dynamics of crime and unnatural mortality