Alcohol and Short-Run Mortality: Evidence from a Modern-Day Prohibition

Kai Barron (WZB Berlin)
Debbie Bradshaw (SAMRC & University of Cape Town)
Charles D.H. Parry (SAMRC & Stellenbosch University)
Rob Dorrington (University of Cape Town)
Pam Groenewald (SAMRC)
Ria Laubscher (SAMRC)
Richard Matzopoulos (SAMRC & University of Cape Town)

Discussion Paper No. 273

January 21, 2021

Collaborative Research Center Transregio 190 | www.rationality-and-competition.de
Ludwig-Maximilians-Universität München | Humboldt-Universität zu Berlin
Spokesperson: Prof. Dr. Klaus M. Schmidt, University of Munich, 80539 Munich, Germany
+49 (89) 2180 3405 | info@rationality-and-competition.de

Alcohol and short-run mortality: Evidence from a modern-day prohibition*

Kai Barron[1], Debbie Bradshaw[2,3], Charles D.H. Parry[2,4], Rob Dorrington[3], Pam Groenewald[2], Ria Laubscher[2], and Richard Matzopoulos[2,3]

[1]WZB Berlin
[2]South African Medical Research Council
[3]University of Cape Town
[4]Stellenbosch University

November 14, 2020

Abstract

On July 13, 2020 a complete nation-wide ban was placed on the sale and transport of alcohol in South Africa. This paper evaluates the impact of this sudden and unexpected five-week alcohol prohibition on mortality due to unnatural causes. We find that the policy reduced the number of unnatural deaths by 21 per day, or approximately 740 over the five-week period. This constitutes a 14% decrease in the total number of deaths due to unnatural causes. We argue that this represents a lower bound on the impact of alcohol on short-run mortality, and underscores the severe influence that alcohol has on society—even in the short-run.

JEL Codes: I18, I12, K42

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

*The authors would like to thank Johannes Abeler, Peter Barron, Heather Jacklin, Simas Kucinskas, Jan Marcus, Melissa Newham, Julia Schmieder, Corne van Walbeek and Rocco Zizzamia for helpful comments. The authorship order of this paper follows the conventions from the health sciences with Barron and Matzopoulos occupying the two lead author positions. This is a reflection of the interdisciplinary nature of this research, which straddles economics and the health sciences. Barron gratefully acknowledges financial support from the German Science Foundation via CRC TRR 190.

1 Introduction

Excessive alcohol consumption is common in many developing and developed countries, particularly amongst the poor (Hosseinpoor et al., 2012; 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 and other crimes, 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; Murray et al., 2020). These harms are often borne by other individuals in society, 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, (religious) norms and correct societal regulation of alcohol have been debated in societies around the world for over two 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 on 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 at a societal level.[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 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 at a societal level. This five-week long ban was the second ban on alcohol sales implemented by the South African government in 2020, but unlike the earlier ban it did not occur amid the initial upheaval caused by COVID-19 in which many new regulations were introduced and individuals were rapidly changing their everyday behavior. This policy was implemented in the context of a country in which a minority of individuals drink (31% of South Africans aged 15 years and older, 43.2% of men and 19.4% of women). However, those who do drink, drink heavily: South

[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).

Africa ranks 6th in the world in terms of the average absolute amount of alcohol consumed per day by drinkers (at 64.6 g or 5.4 standard drinks), with six out of ten drinkers (59%) engaging in heavy episodic drinking (WHO, 2019). South Africa is also a country that suffers from a high rate of mortality due to unnatural causes (e.g., interpersonal violence, road traffic collisions, and suicide), with approximately 50 000 injury-rated deaths recorded per year between 1997 and 2012 (Matzopoulos et al., 2015; Pillay-van Wyk et al., 2016), and also between 2015 and 2019 (own calculations).[3] Alcohol consumption has been identified as a major risk factor for injury-related deaths (see, e.g., Rehm et al., 2003, 2017). For example, Probst et al. (2018) use a comparative risk assessment approach to estimate that in 2015 over 12 000 injury-related deaths in South Africa were attributable to alcohol consumption. This analysis suggests that reducing alcohol consumption could lead to a large reduction in injury-related mortality. It is therefore of key importance to test these predictions by assessing how mortality is actually affected when a policy that drastically reduces alcohol consumption is introduced.

This paper uses the exogenous variation generated by the natural experiment provided by the alcohol sales ban to study the causal impact of alcohol on mortality due to unnatural causes at a societal level. This is valuable as it provides policy-makers with robust evidence about whether reducing alcohol consumption is an effective way to save lives in the short-run.[4] It therefore contributes evidence towards the larger discussion regarding the aggregate costs and benefits of alcohol consumption for society.

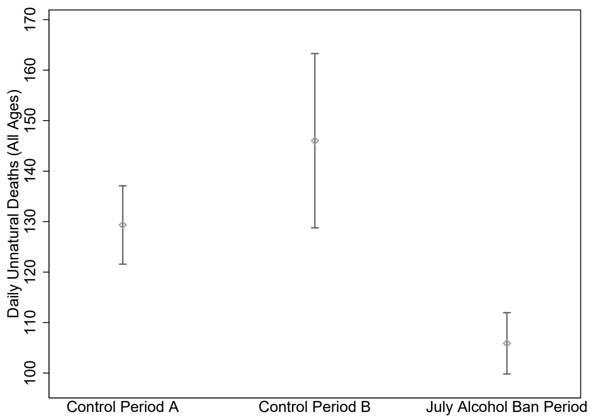
To do this, we use daily mortality data from South Africa for the period between January 1, 2016 and September 13, 2020. This allows us to use data from previous years to carefully control for temporal regularities in mortality observed over the course of the year in our analysis. This is crucial because we show that there are extremely regular, systematic patterns in the number of unnatural deaths observed according to the day-of-the-week and day-of-the-month. Using 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 by the South African government 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

[3]The population of South Africa has grown from 43 million in 1997 to almost 58 million in 2018, implying a gradual reduction in the per capita rate.

[4]Importantly, in this paper we only examine the impact on one extreme short-run outcome (i.e. mortality). This implies that any detected reduction in mortality due to the alcohol ban is likely to be indicative of a reduction in many other less extreme outcomes, such as injury, that typically result from similar behaviors (e.g. violence, road traffic collisions).

in the middle of the 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 implemented together with a curfew between 9PM and 4AM, but we consider this curfew as having had a largely secondary influence on mortality. We provide support for this view by conducting a sensitivity analysis that makes use of a one hour reduction in the length of the curfew which occurred in the middle of the relevant period.

Figure 1: The alcohol ban and unnatural mortality



Notes: (i) The July Alcohol Ban Period refers to July 13, 2020 - August 17, 2020; (ii) Control Period B refers to the same period during the previous year, i.e. July 13, 2019 - August 17, 2019; and (iii) Control Period A refers to the period immediately preceding the alcohol ban in which other policies were largely the same as during the July Alcohol Ban, i.e. June 1, 2020 - July 12, 2020, (iv) the bars represent 95% confidence intervals.

Figure 1 illustrates our main result: Mortality due to unnatural causes was substantially lower during the July Alcohol Ban period than it was during either of the two control periods we consider. Control Period A refers to the 2020 period that immediately preceded the alcohol ban, and Control Period B covers the exact calendar period of the July Alcohol Ban but during the previous year (i.e. 2019). While this figure portrays a simple plot of the raw data, our empirical strategy allows us to control for potential confounding factors. Doing this, we document evidence of several important findings.

Our main result is that the alcohol ban reduced the number of people dying from unnatural causes in South Africa by approximately 21 per day. This corresponds to 740 fewer deaths during the 36 days of the July Alcohol Ban. This represents a substantial reduction in mortality due to unnatural causes. It implies a 14% reduction when compared to the same period in 2019, when

[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.

there were 146 deaths per day due to unnatural causes; or to a 16% reduction when compared to the six-week daily average in the Level 3 period immediately preceding the July Alcohol Ban in 2020, when there were 129 unnatural deaths per day.[6] In the analysis below, we show that this reduction in mortality is entirely confined to men. 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). We find that the ban on alcohol reduced the number of men dying due to unnatural causes by approximately 21 per day, but find no evidence that it had a statistically significant effect on the mortality of women. (Importantly, this does not imply that the absence of alcohol had no impact on other outcomes such as gender-based violence, which often does not result in death.) 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 results, we conduct several robustness exercises. These include running placebo regressions and varying the window size around the policy change used for our analysis (Section B.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 (Section B.1). In addition, using data from previous years (i.e. excluding 2020) we document systematic regularities in the pattern of unnatural deaths observed: (i) a weekly pattern: mortality due to unnatural causes follows a highly predictable weekly pattern, with an increase of over 50% in daily unnatural deaths on Saturdays and Sundays relative to weekdays, (ii) a monthly pattern: unnatural mortality is highest during the last and first few days of the month (over 30% higher), suggesting that this monthly pattern may be related to wage payment schedules. Our data allow us to control for these systematic mortality patterns in our analysis.

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,

[6]There were approximately 106 deaths per day due to unnatural causes during the July Alcohol Ban period. It is worth noting that this implies that the reduction in the raw number of unnatural deaths relative to the two control periods is larger than our causal estimate of 21—i.e. there were 23 fewer daily deaths in comparison to the calendar period that immediately preceded the alcohol ban (Control A), and 40 fewer daily deaths in comparison to the same period in the previous year (Control B). The reason for this is that the larger raw differences include the influence of other factors, e.g., the impact of COVID-19 and related regulations in 2020.

2018). There are two main empirical approaches that have been employed 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).[7] 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 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 struggle 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 of state and federal prohibition statutes enacted in the United States during the early decades of the twentieth century 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.[8]

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

[7] 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.

[8] 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 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.

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 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.[9] Last, our results add to the contemporaneous work studying the impact of COVID-19 policy responses on crime rates (e.g. Asik and Nas Ozen, 2020; Poblete-Cazenave, 2020).

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 results and robustness exercises, and Section 5 concludes.

2 Data and the Policy Landscape

2.1 Policy Timeline

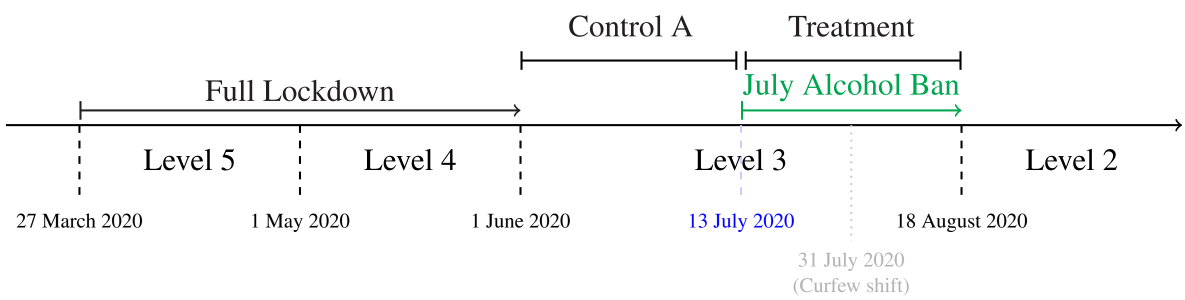
The policy change investigated in this paper is the introduction of a complete ban on all alcohol sales in South Africa. This change was announced on the evening of Sunday, July 12, 2020

[9] 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. An important consideration is that a curfew implemented in isolation is a very different policy tool to a curfew implemented in conjunction with a restriction on alcohol, since complementarities may exist between the two policy tools. While we do not view the curfew implemented on July 13, 2020 in South Africa as a key driver of our results, it is important to keep this previous evidence in mind when interpreting our results.

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 reduce the pressure on the healthcare system due to alcohol related injuries and illnesses in order to free up resources for treating COVID-19 related hospitalizations (Ramaphosa, 2020). 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 the 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 2 provides an overview of the timeline of policy changes.

Figure 2: Timeline of policy events



After the initial period of extremely strict Level 5 measures, there was a slight relaxation of policy measures to Level 4 from May 1, 2020, but for much of the general population, this still involved a continuation of the state of lockdown.[10] 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 with respect to this paper were the following: (i) off-premises and e-commerce alcohol sales were only permitted from Monday to Thursday between 9AM and 5PM,[11] (ii) there was no official curfew, but individuals were only

[10]Section C in the appendices provides a more detailed discussion of the lockdown (i.e. Levels 5 and 4).

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

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.[12]

In the middle of the Level 3 period, on July 13, 2020, the government abruptly introduced a complete ban on the sale of alcohol. The reason for this was that there were reports emerging that the number of alcohol-related trauma cases in hospitals had increased rapidly after the move to Level 3.[13] Along with this alcohol ban, a curfew from 9PM to 4AM was introduced. While curfews are generally viewed as an important policy tool for curtailing the rate of interpersonal violence and road traffic collisions, against the background of the existing Level 3 policy measures already in place in South Africa, the curfew did not represent a substantial change in the de facto legal situation. During the first phase of the Level 3 period, gatherings were already banned and individuals were not permitted to be outside their residence without a valid reason. Therefore, even before the curfew was enacted very few valid reasons existed for leaving one's place of residence in the middle of the night.

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

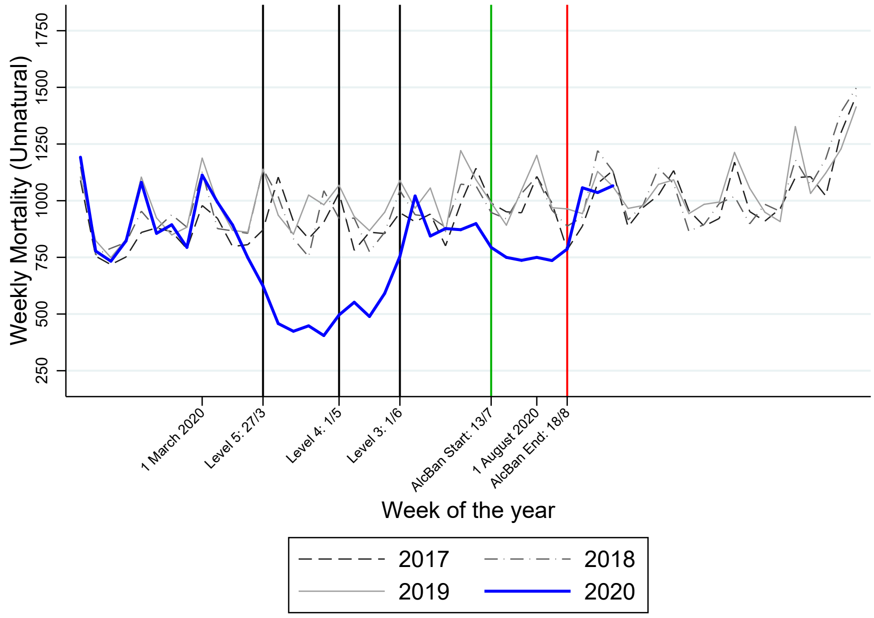
In our main analysis, we use national daily mortality data from January 1, 2017 to September 13, 2020. This dataset is collected by the Department of Home Affairs and curated by the South African Medical Research Council and contains a record of all deaths of persons with a valid South African identity 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. Unnatural deaths, therefore, are often caused by risky behavior with short-run 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.

Figure 3 provides a descriptive illustration of our data. The bold blue line denotes weekly mortality levels due to unnatural causes in 2020, while the grey lines reflect the same measure

[12]Other Level 3 restrictions include: (a) South Africa's borders remained largely closed, (b) movement between provinces within the country was largely prohibited, (c) schools were permitted to open (Government Gazette, 2020c).

[13]For a more detailed historical overview of the evolution of South Africa's relationship with alcohol, see Parry (2005), Mayosi et al. (2009), Parry (2010), Norman et al. (2010), Matzopoulos et al. (2013), Chelwa and van Walbeek (2020) and Matzopoulos et al. (2020).

Figure 3: Weekly mortality (unnatural deaths, all ages)



for each of the previous three years. The vertical lines reflect the changes in regulations in 2020 discussed above, with the green line indicating the start of the July Alcohol Ban, and the red line indicating the end of this alcohol ban. 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 levels back up, closer to the level observed in previous years. However, the figure provides suggestive evidence that the introduction of the July Alcohol Ban then reduced the rate of unnatural mortality again. (It is the objective of the analysis below is to evaluate whether this visual pattern in the raw data persists when subjected to a more rigorous analysis.) Last, the end of the Level 3 period (represented by the red vertical line) brought with it a rise in mortality. The move from Level 3 to Level 2 implied a further relaxation of restrictions. In particular, the alcohol sales ban was rescinded, but the curfew was retained. This increase in mortality at the beginning of Level 2 suggests that the curfew was not a crucial reason for the lower mortality levels observed during the second half of Level 3.

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 A, 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 3 above 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.

Our analysis uses three versions of this data. The first contains a record of daily mortality levels due to unnatural causes in the country as a whole. 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. The main reason for examining this sub-population is that young adults are typically viewed as being the group most prone to risky behavior and therefore potentially the most affected by the short-run negative outcomes associated with alcohol.

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 period that immediately preceded it in the first half of Level 3. However, it is crucial to isolate the effect of the alcohol ban from unrelated seasonal and weekly changes in behavior. Using detailed mortality data from the preceding years (i.e. 2017, 2018, 2019), we do this in two ways: (1) we control for the systematic variation in mortality using day-of-the-week, day-of-the-month and year fixed effects, and (2) we control directly for the baseline mortality level observed during the Level 3 calendar period and alcohol ban calendar period in preceding three years. Doing this removes any weekly, monthly, seasonal or 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 therefore estimate the following model using Ordinary Least Squares:

$$\begin{aligned} Mort_{y,t,g} = & \alpha_0 + \alpha_1 \cdot L3_{y,t} + \alpha_2 \cdot AlcBan_{y,t} + \alpha_3 \cdot L3_{y,t} \times Y_{2020} + \\ & \beta \cdot AlcBan_{y,t} \times Y_{2020} + \lambda_{DoW} + \gamma_{DoM} + \mu_{year} + \epsilon_{y,t,g} \end{aligned} \tag{1}$$

where $Mort_{y,t,g}$ 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). To control for the seasonal mortality level observed during previous years, we include an indicator variable for the entire level 3 calendar period from 1 June to 17 August, $L3_{y,t}$, as well as an indicator variable for the July Alcohol Ban period from 13 July to 17 August, $AlcBan$.[15] 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 alcohol ban.

Our main coefficient of interest is β, which provides an estimate of the impact of the alcohol ban on mortality. In addition, in our preferred specification, we include day-of-the-week, λ_{DoW}, day-of-the-month, γ_{DoM}, and year, μ_{year}, fixed effects. Due to the substantial and systematic weekly and monthly heterogeneity in mortality discussed above, the inclusion of these fixed effects should improve the precision of the estimates.

[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 previous work, e.g. Caliendo and Wrohlich (2010) and Schönberg and Ludsteck (2014), and can be justified when there is strong year-on-year temporal regularity in the outcome of interest.

[15]For clarity, these variables take a value of 1 during the calendar period in question during each of the years in our data (i.e. from 2017 to 2020 in our main estimation).

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 20.57 deaths per day (95% CI: 7.08–34.07). 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. It is also worth noting the large estimated relationship between weekends and mortality, with column (1b) showing that on average 89.43 more individuals die on Saturdays and Sundays in comparison to other days of the week. The interaction term, *Weekend Day* $\times$ *Year=2020*, shows that in 2020, however, this weekend effect was dampened substantially (as can also be seen in Figure 5 in the appendices). However, controlling for this weekend effect—either directly [as in column (1b)] or through day-of-the-week fixed effects [as in column (1c)]—does not substantially affect the estimated impact of the alcohol ban.

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

	(1a)	(1b)	(1c)
Level 3 Period = 1	17.14***	12.66***	8.74***
(1/6-17/8)	(5.20)	(3.07)	(2.43)
Alcohol Ban Period = 1	-0.98	-1.93	0.85
(13/7-17/8)	(6.85)	(3.78)	(3.14)
Level 3 Period x Year=2020	-13.74**	2.47	9.78*
	(6.16)	(4.50)	(5.44)
Alcohol Ban Period x Year=2020	-22.47***	-21.26***	-20.57***
	(8.38)	(5.30)	(6.88)
Weekend Day = 1		89.43***	
		(3.18)	
Weekend Day x Year=2020		-56.74***	
		(7.58)	
Constant	125.93***	104.86***	97.74***
	(1.90)	(1.18)	(4.74)
Day of Week FEs			Y
Day of Month FEs			Y
Year FEs			Y
Observations	972	972	972
Adjusted R^2	0.026	0.573	0.658

Notes: (i) Each observation contains unnatural mortality data for a single day, (ii) Robust standard errors are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between January, 15 and September, 13 of each year, excluding the abnormal period around New Year's Eve and also 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.

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 6 in the appendices). 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 South Africa. For example, the WHO (2019) reports that heavy episodic drinking was five times higher amongst men in comparison to women in 2016 (see, also, Shisana et al., 2013; Probst et al., 2017, for informative descriptions of drinking behavior in South Africa). Together, these factors could lead to a differential effect of the alcohol sales ban by gender.

Table 2: Impact of the alcohol ban on mortality (by gender)

		Men			Women	
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
Level 3 Period = 1	15.05***	11.34***	7.73***	2.91***	2.12***	0.61
(1/6-17/8)	(4.50)	(2.58)	(2.12)	(0.94)	(0.76)	(0.67)
Alcohol Ban Period = 1	-1.98	-2.08	0.80	-0.18	-0.20	0.55
(13/7-17/8)	(5.96)	(3.19)	(2.75)	(1.18)	(0.95)	(0.84)
Level 3 Period x Year=2020	-8.91*	4.76	9.82**	-5.47***	-2.52**	0.75
	(5.33)	(3.82)	(4.57)	(1.23)	(1.15)	(1.32)
Alcohol Ban Period x Year=2020	-20.90***	-20.57***	-20.67***	-0.38	-0.34	-0.40
	(7.27)	(4.60)	(5.99)	(1.71)	(1.49)	(1.55)
Weekend Day = 1		76.47***			13.15***	
		(2.64)			(0.76)	
Weekend Day x Year=2020		-47.86***			-10.31***	
		(6.15)			(1.66)	
Constant	97.25***	79.11***	72.50***	28.51***	25.54***	24.98***
	(1.57)	(0.95)	(3.81)	(0.40)	(0.33)	(1.51)
Day of Week FEs			Y			Y
Day of Month FEs			Y			Y
Year FEs			Y			Y
Observations	968	968	968	968	968	968
Adjusted R^2	0.027	0.596	0.665	0.021	0.311	0.417

Notes: (i) Each observation corresponds to a single day, (ii) Robust standard errors are reported in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between January, 15 and September, 13 of each year, excluding the abnormal period around New Year's Eve and also February, 29, (iv) All columns report estimates for the outcome variable, unnatural mortality, and differ only in their specifications, with column (*a) the simplest specification, (*b) adding controls for weekend days, and (*c) adding fixed effects.

Table 2 reports the estimated impact of the alcohol ban on the 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 20.67 (95% CI: 8.92–32.42). For women, we find no significant impact of the alcohol ban on mortality.

Table 3: Impact of the alcohol ban on mortality (15 to 34 years)

		Men			Women	
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
Level 3 Period = 1	8.33***	6.32***	5.37***	0.21	-0.14	-0.51
(1/6-17/8)	(2.96)	(1.69)	(1.45)	(0.55)	(0.46)	(0.43)
Alcohol Ban Period = 1	-2.71	-2.77	-1.02	0.38	0.37	0.78
(13/7-17/8)	(3.86)	(2.05)	(1.86)	(0.71)	(0.57)	(0.52)
Level 3 Period x Year=2020	-7.04**	0.26	-0.70	-1.25*	0.05	0.26
	(3.51)	(2.56)	(3.30)	(0.73)	(0.71)	(0.89)
Alcohol Ban Period x Year=2020	-11.35**	-11.10***	-11.19***	-1.38	-1.36	-1.40
	(4.66)	(3.03)	(4.03)	(0.99)	(0.90)	(1.00)
Weekend Day = 1		48.87***			7.02***	
		(1.76)			(0.43)	
Weekend Day x Year=2020		-25.53***			-4.56***	
		(4.96)			(1.09)	
Constant	49.02***	37.07***	34.38***	11.19***	9.53***	10.10***
	(1.07)	(0.66)	(2.58)	(0.23)	(0.20)	(0.86)
Day of Week FEs			Y			Y
Day of Month FEs			Y			Y
Year FEs			Y			Y
Observations	968	968	968	968	968	968
Adjusted R^2	0.019	0.552	0.608	0.004	0.257	0.320

Notes: (i) Each observation corresponds to a single day, (ii) Robust standard errors are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between January, 15 and September, 13 of each year, excluding the abnormal period around New Year's Eve and also February, 29, (iv) All columns report estimates for the outcome variable, unnatural mortality, and differ only in their specifications, with column (*a) the simplest specification, (*b) adding controls for weekend days, and (*c) adding fixed effects.

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. This is because 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 3 reports these results. We find that the alcohol ban reduced mortality amongst men in this age-group by approximately 11 deaths per day, with an estimated reduction of 11.19 (95% CI: 3.29–19.09) in column (1c), but did not have a significant 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 11 lives of younger men saved per day by the alcohol ban is only slightly over half the 21 male lives of all ages saved per day. 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. Therefore, 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.

4.4 Robustness exercises

To provide support for the validity of these results, we conduct several robustness exercises. These exercises, and the associated results, are discussed in detail in Section B 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 B.1). To do this, we show that the primary candidate confounding factors were unlikely to have contributed to the observed reduction in unnatural mortality. The two main additional sources of behavioral change in society during the period we are studying were the COVID-19 pandemic and 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 number of daily confirmed COVID-19 cases was dropping rapidly. We also examine the possibility that the main contemporaneous regulatory change, namely the introduction of a curfew, influenced unnatural mortality. To do this, 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 had no impact on unnatural mortality. This result supports our assessment that the curfew was unlikely to be the main driver of the reduction in mortality observed during the alcohol ban period.[16]

The next three exercises check that our results are not driven by the particular empirical strategy that we adopt nor by anomalies in the data (see Section B.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, *Alcohol Ban Period* × *Year=2019*, are no longer statistically significant when considering the full sample (i.e. individuals of all ages). However, for the sub-

[16]An important additional piece of evidence that supports our assessment regarding the curfew is that when the alcohol ban ended on August, 17, the curfew remained in place. However, as show in Figure 3, unnatural mortality increased sharply after this date despite the ongoing curfew, returning to pre-2020 levels. This evidence also suggests that the curfew alone was unlikely to have been a key factor in reducing mortality.

sample of younger adults, we do observe a slight reduction in the mortality level, significant at the 10% level, amongst men. This should be kept in mind as a potential caveat to the subset of our results that focus on younger adults.

Second, we examine the influence of the exact time window used for our estimation. When examining the weekly and daily mortality patterns in Figures 3 and 5, a potential concern is the increase in mortality observed immediately after the relaxation of restrictions on June 1, 2020 (i.e. at the beginning of the Level 3 period). To address this concern, we conduct an additional robustness exercise where we vary the length of the time window around the introduction of the alcohol ban 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 our main results are robust to the exclusion of the potentially problematic first week of the Level 3 period, and the estimated impact of the alcohol ban remains fairly stable when considering windows of 5 weeks, 4 weeks and 3 weeks in length. However, the exception to this is that when we reduce the window to only 2 weeks in length, we no longer observe a significant coefficient estimate for the alcohol ban. In Section B.2.2, we discuss several potential explanations for this, including: (i) the possibility that there could be a lag between the introduction of an alcohol sales ban and a substantial reduction in alcohol consumption as individuals take some time to deplete the stock of alcohol purchased prior to the ban, and (ii) the important consideration that the two week period prior to July, 13 normally includes an payday weekend (with the associated inflated mortality levels), while the two weeks afterwards does not.

Last, we replicate our main results, but restrict the dataset to only contain observations during the Level 3 calendar period. Therefore, we use data for 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:

$$Mort_{y,t,g} = \alpha_0 + \alpha_1 \cdot AlcBan_{y,t} + \beta \cdot AlcBan_{y,t} \times Y_{2020} + \lambda_{DoW} + \gamma_{DoM} + \mu_{year} + \epsilon_{y,t,g} \quad (2)$$

where $Mort_{y,t,g}$ 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). The point estimates from our preferred specification, which includes fixed effects, are very close to those in our main results.[17]

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

5 Concluding discussion

In this paper we have documented evidence that a five-week nationwide ban on the sale of alcohol resulted in a reduction of 21 unnatural deaths per day during that period. This is a large and meaningful number of lives saved. To put the magnitude into context, it corresponds to over 14% of all unnatural deaths. Our results provide unique causal evidence on the impact that a short-term *absence* of alcohol can have on 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. South Africa is a country that has a high baseline rate of unnatural mortality, implying that the impact of alcohol as a magnifying influence is particularly severe.[18] However, the evidence presented above demonstrates that alcohol can substantially increase the rate of behavior-induced harm observed in the population. These empirical results, therefore, support the predictions of earlier modeling studies that estimate that alcohol consumption places a heavy mortality toll on society (see, e.g., Probst et al., 2014, 2018; Mackenbach et al., 2015; Rehm et al., 2017).

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 hypothetical long-term ban, society would shift to a new equilibrium, which may involve legally acquired alcohol being replaced by illegally acquired or homemade alcohol. 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. It illustrates the substantial benefits to society that can be achieved by carefully implementing policies that might

[17]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 B.2.3 for further details.

[18]For a detailed analysis of the breakdown of the cause of death in injury-related mortality in South Africa, see Matzopoulos et al. (2015). The authors provide evidence that over 65% of injury-related deaths in South Africa in 2009 were due to homicides or road-traffic injuries. In addition, it is worth noting that for every alcohol-induced injury death, Matzopoulos et al. (2006) and Norman et al. (2007) show that there are approximately 25 further injuries requiring hospitalization. This implies that the 700 fewer deaths over the five week alcohol ban period could plausibly have been associated with 17 500 fewer hospitalizations.

be successful in curbing 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.[19]

Second, our estimates of the impact of the alcohol sales ban likely constitute a lower bound estimate of the true impact of alcohol on short-run unnatural mortality in society. The reasons for this are: (i) We are essentially comparing mortality levels during the first and second part of the Level 3 policy period. However, during the first part of the Level 3 period, unnatural mortality was already lower than during the same period in previous years. This is especially true for weekends (when more alcohol-related deaths usually occur). One reason for this could be that even during the first part of the Level 3 period, alcohol sales were only legally permitted on Monday to Thursday, between 9AM and 5PM. Therefore, we are estimating the reduction in mortality from an already lower baseline level. (ii) South Africa experienced severe economic hardship as a result of the long and extreme lockdown period during Level 5 and 4. By the time the country reached Level 3, it is likely that many individuals (especially the poor) had less disposable income than they would have had in other years (e.g. Jain et al. (2020) show that there was a 40% decline in active employment, and poor access to social welfare during the lockdown in South Africa). A lower disposable income implies there is less money available for purchasing alcohol. (iii) According to media reports, compliance with the alcohol sales ban was imperfect.[20]

Third, the absence of an estimated impact of alcohol on female mortality does not imply that there was no impact of alcohol on other outcomes for women. For example, our results do not provide evidence regarding the prevalence of gender-based violence resulting in outcomes other than death.[21]

Fourth, this paper focuses exclusively on the relationship between alcohol and short-run mortality. If one wishes to examine the overall influence of alcohol on society, there are numerous other short-run and long-run costs and benefits to consider. The influence of alcohol is ubiquitous in many modern societies and affects the health, wealth and general welfare of the population through a myriad of different channels.

The natural experiment that we study also provides ideal conditions for studying the impact of alcohol on other societal outcomes. This paper examines the impact on one very important outcome: mortality. However, the exogenous variation generated by the abrupt alcohol sales ban

[19]The World Health Organization has proposed five such intervention strategies as part of it's SAFER initiative (WHO, 2018).

[20]Some examples of the media reports include articles in the Guardian (2020), the Economist (2020), and a letter by Prinesha Naidoo published in Bloomberg (2020). In addition, van Walbeek et al. (2020) provide evidence that access to cigarettes (which were also illegal to sell during Levels 5, 4 and 3) was widespread. Further, Onya et al. (2012) and Londani et al. (2019) report that many South Africans are experienced at making homemade alcohol.

[21]See Section D in the appendices for a brief comment on gender-based violence in South Africa during COVID-19.

provides a fruitful opportunity for studying the causal impact of the absence of alcohol on an array of other societal outcomes, such as crime rates, trauma-induced hospitalization rates, savings rates, road traffic injuries, in utero alcohol exposure outcomes, gender-based violence, and the incidence rates of sexually transmitted diseases (e.g. HIV). It also provides an opportunity to examine behavioral questions such as the implications of a short enforced dry period on breaking alcohol addiction. We leave this for future work.

References

Adda, J. (2007). Behavior towards health risks: An empirical study using the "mad cow" crisis as an experiment. *Journal of Risk and Uncertainty 35*(3), 285–305.

Agüero, J. M. (2020). COVID-19 and the rise of intimate partner violence. *World Development (forthcoming).*

Ahituv, A., V. J. Hotz, and T. Philipson (1996). The responsiveness of the demand for condoms to the local prevalence of AIDS. *Journal of Human Resources*, 869–897.

Akesson, J., S. Ashworth-Hayes, R. Hahn, R. D. Metcalfe, and I. Rasooly (2020). Fatalism, beliefs, and behaviors during the covid-19 pandemic. *NBER Working Paper*.

Allen, L., J. Williams, N. Townsend, B. Mikkelsen, N. Roberts, C. Foster, and K. Wickramasinghe (2017). Socioeconomic status and non-communicable disease behavioural risk factors in low-income and lower-middle-income countries: a systematic review. *Lancet Global Health 5*(3), e277–e289.

Anderberg, D., H. Rainer, and F. Siuda (2020). Quantifying domestic violence in times of crisis. *CESifo Working Paper No. 8593*.

Asik, G. A. and E. Nas Ozen (2020). It takes a curfew: The effect of COVID-19 on female homicides. *Mimeo*.

Barron, K., L. F. Gamboa, and P. Rodriguez-Lesmes (2019). Behavioural response to a sudden health risk: Dengue and educational outcomes in colombia. *Journal of Development Studies 55*(4), 620–644.

Bates, G. (1918). The relation of alcohol to the acquisition of venereal diseases. *Public Health Journal 9*(6), 262–267.

Baughman, R., M. Conlin, S. Dickert-Conlin, and J. Pepper (2001). Slippery when wet: the effects of local alcohol access laws on highway safety. *Journal of Health Economics 20*(6), 1089–1096.

Bennett, D., C.-F. Chiang, and A. Malani (2015). Learning during a crisis: The SARS epidemic in taiwan. *Journal of Development Economics 112*, 1–18.

Bhattacharya, J., C. Gathmann, and G. Miller (2013). The gorbachev anti-alcohol campaign and russia's mortality crisis. *American Economic Journal: Applied Economics 5*(2), 232–60.

Biderman, C., J. M. De Mello, and A. Schneider (2010). Dry laws and homicides: evidence from the São Paulo metropolitan area. *The Economic Journal 120*(543), 157–182.

Blocker, J. (2006). Did prohibition really work? alcohol prohibition as a public health innovation. *American Journal of Public Health 96*(2), 233–243.

Bloomberg (2020). South Africa's tobacco, booze ban lit up illegal trade. `https://www.bloomberg.com/news/newsletters/2020-09-15/supply-lines-south-africa-s-tobacco-alcohol-ban-lit-up-illegal-trade`. Online; Letter by Prinesha Naidoo; published 15-September-2020; accessed 28-September-2020.

Boserup, B., M. McKenney, and A. Elkbuli (2020). Alarming trends in us domestic violence during the COVID-19 pandemic. *American Journal of Emergency Medicine*.

Bullinger, L. R., J. B. Carr, and A. Packham (2020). COVID-19 and crime: Effects of stay-at-home orders on domestic violence. *NBER Working Paper*.

Caliendo, M. and K. Wrohlich (2010). Evaluating the German 'mini-job' reform using a natural experiment. *Applied Economics 42*(19), 2475–2489.

Carpenter, C. (2004). Heavy alcohol use and youth suicide: Evidence from tougher drunk driving laws. *Journal of Policy Analysis and Management 23*(4), 831–842.

Carpenter, C. (2005a). Heavy alcohol use and the commission of nuisance crime: Evidence from underage drunk driving laws. *American Economic Review: P & P 95*(2), 267–272.

Carpenter, C. (2005b). Youth alcohol use and risky sexual behavior: evidence from underage drunk driving laws. *Journal of Health Economics 24*(3), 613–628.

Carpenter, C. (2007). Heavy alcohol use and crime: Evidence from underage drunk-driving laws. *Journal of Law and Economics 50*(3), 539–557.

Carpenter, C. and C. Dobkin (2009). The effect of alcohol consumption on mortality: Regression discontinuity evidence from the minimum drinking age. *American Economic Journal: Applied Economics 1*(1), 164–182.

Carpenter, C. and C. Dobkin (2011). The minimum legal drinking age and public health. *Journal of Economic Perspectives 25*(2), 133–156.

Carpenter, C. and C. Dobkin (2017). The minimum legal drinking age and morbidity in the united states. *Review of Economics and Statistics 99*(1), 95–104.

Carr, J. B. and J. L. Doleac (2018). Keep the kids inside? juvenile curfews and urban gun violence. *Review of Economics and Statistics 100*(4), 609–618.

Chelwa, G. and C. van Walbeek (2020). The case for minimum unit prices on alcohol in South Africa. *Mimeo*.

Chikritzhs, T. and T. Stockwell (2006). The impact of later trading hours for hotels on levels of impaired driver road crashes and driver breath alcohol levels. *Addiction 101*(9), 1254–1264.

Dills, A. K. and J. A. Miron (2004). Alcohol prohibition and cirrhosis. *American Law and Economics Review 6*(2), 285–318.

Dorrington, R., D. Bradshaw, R. Laubscher, and N. Nannan (2020). *Rapid mortality surveillance report 2018*. Cape Town: South African Medical Research Council.

Economist (2020). Dry, the beloved country: South Africa bans alcohol sales. `https://www.economist.com/middle-east-and-africa/2020/07/18/south-africa-bans-alcohol-sales`. Online; published 18-July-2020; accessed 28-September-2020.

Emerson, H. (1932). Prohibition and mortality and morbidity. *The ANNALS of the American Academy of Political and Social Science 163*(1), 53–60.

Fetzer, T., L. Hensel, J. Hermle, and C. Roth (2020). Coronavirus perceptions and economic anxiety. *Review of Economics and Statistics*, 1–36.

Gamboa, L. F. and P. R. Lesmes (2019). The fertility-inhibiting effect of mosquitoes: Socio-economic differences in response to the Zika crisis in Colombia. *Economics & Human Biology 35*, 63–72.

Gould, C. (2020). Gender-based violence during lockdown: looking for answers. `https://www.saferspaces.org.za/blog/entry/gender-based-violence-during-lockdown-looking-for-answers`. Online; published 12-May; accessed 28-September-2020.

Government Gazette (2020a). Department of Co-operative Governance and Traditional Affairs, South Africa. Amendment of Regulations issues in terms of Section 27(2) of the Disaster Management Act of 2002, 43148, 398. `https://www.gov.za/sites/default/files/g` cis_document/202003/4314825-3cogta.pdf. Online; published 25-March-2020; accessed 27-September-2020.

Government Gazette (2020b). Department of Co-operative Governance and Traditional Affairs, South Africa. Amendment of Regulations issues in terms of Section 27(2) of the Disaster Management Act of 2002, 43521, 763. https://www.gov.za/sites/default/files/gcis_document/202007/43521gon763.pdf. Online; published 12-July-2020; accessed 27-September-2020.

Government Gazette (2020c). Department of Co-operative Governance and Traditional Affairs, South Africa. Disaster Management Act, 2002: (Act No. 57 of 2002): Determination of Alert Levels and Hotspots, 43364, 608. https://www.gov.za/sites/default/files/gcis_document/202005/43364gon608-translations.pdf. Online; published 28-May-2020; accessed 27-September-2020.

Government Gazette (2020d). Department of Co-operative Governance and Traditional Affairs, South Africa. Regulations issues in terms of Section 27(2) of the Disaster Management Act of 2002, 43258, 480. https://www.gov.za/sites/default/files/gcis_document/202004/43258rg11098gon480s.pdf. Online; published 29-April-2020; accessed 27-September-2020.

Green, C. P., J. S. Heywood, and M. Navarro (2014). Did liberalising bar hours decrease traffic accidents? *Journal of Health Economics 35*, 189–198.

Griswold, M. G., N. Fullman, C. Hawley, N. Arian, S. R. Zimsen, H. D. Tymeson, V. Venkateswaran, A. D. Tapp, M. H. Forouzanfar, J. S. Salama, et al. (2018). Alcohol use and burden for 195 countries and territories, 1990–2016: a systematic analysis for the global burden of disease study 2016. *Lancet 392*(10152), 1015–1035.

Guardian (2020). South Africa's alcohol ban has given 'massive boost' to criminal gangs. https://www.theguardian.com/world/2020/may/31/south-africas-alcohol-ban-has-given-massive-boost-to-criminal-gangs. Online; published 31-May-2020; accessed 28-September-2020.

Hosseinpoor, A. R., N. Bergen, A. Kunst, S. Harper, R. Guthold, D. Rekve, E. T. d'Espaignet, N. Naidoo, and S. Chatterji (2012). Socioeconomic inequalities in risk factors for non communicable diseases in low-income and middle-income countries: results from the world health survey. *BMC Public Health 12*(1), 912.

Jain, R., J. Budlender, R. Zizzamia, and I. Bassier (2020). The labor market and poverty impacts of COVID-19 in South Africa. *Working Paper*.

Katikireddi, S. V., E. Whitley, J. Lewsey, L. Gray, and A. H. Leyland (2017). Socioeconomic status as an effect modifier of alcohol consumption and harm: analysis of linked cohort data. *Lancet Public Health 2*(6), e267–e276.

Kline, P. (2012). The Impact of Juvenile Curfew Laws on Arrests of Youth and Adults. *American Law and Economics Review 14*(1), 44–67.

Lakdawalla, D., N. Sood, and D. Goldman (2006). HIV breakthroughs and risky sexual behavior. *Quarterly Journal of Economics 121*(3), 1063–1102.

Law, M. T. and M. S. Marks (2020). Did early twentieth-century alcohol prohibition affect mortality? *Economic Inquiry 58*(2), 680–697.

Leslie, E. and R. Wilson (2020). Sheltering in place and domestic violence: Evidence from calls for service during COVID-19. *Available at SSRN 3600646*.

Livingston, B. (2016). Murder and the black market: Prohibition's impact on homicide rates in American cities. *International Review of Law and Economics 45*, 33–44.

Londani, M., N. K. Morojele, E. Nel, and C. D. Parry (2019). Manufacturing homemade alcohol in the City of Tshwane, South Africa. *African Journal of Drug and Alcohol Studies 18*(1), 43–54.

Mackenbach, J. P., I. Kulhánová, M. Bopp, C. Borrell, P. Deboosere, K. Kovács, C. W. Looman, M. Leinsalu, P. Mäkelä, P. Martikainen, et al. (2015). Inequalities in alcohol-related mortality in 17 European countries: a retrospective analysis of mortality registers. *PLoS medicine 12*(12), e1001909.

Mahmud, M. and E. Riley (2020). Household response to an extreme shock: Evidence on the immediate impact of the COVID-19 lockdown on economic outcomes and well-being in rural Uganda. *Working Paper*.

Marcus, J. and T. Siedler (2015). Reducing binge drinking? the effect of a ban on late-night off-premise alcohol sales on alcohol-related hospital stays in Germany. *Journal of Public Economics 123*, 55–77.

Matzopoulos, R., M. Peden, D. Bradshaw, and E. Jordaan (2006). Alcohol as a risk factor for unintentional rail injury fatalities during daylight hours. *International Journal of Injury Control and Safety Promotion 13*(2), 81–88.

Matzopoulos, R., M. Prinsloo, V. Pillay-van Wyk, N. Gwebushe, S. Mathews, L. J. Martin, R. Laubscher, N. Abrahams, W. Msemburi, C. Lombard, and D. Bradshaw (2015, May). Injury-related mortality in South Africa: a retrospective descriptive study of postmortem investigations. *Bulletin of the World Health Organization 93*(5), 303–313.

Matzopoulos, R., M. Prinsloo, V. P.-v. Wyk, N. Gwebushe, S. Mathews, L. J. Martin, R. Laubscher, N. Abrahams, W. Msemburi, C. Lombard, et al. (2015). Injury-related mortality in South Africa: a retrospective descriptive study of postmortem investigations. *Bulletin of the World Health Organization 93*, 303–313.

Matzopoulos, R., H. Walls, S. Cook, and L. London (2020). South Africa's COVID-19 Alcohol Sales Ban: The Potential for Better Policy-Making. *International Journal of Health Policy and Management*, 1.

Matzopoulos, R. G., M. Prinsloo, A. Butchart, M. M. Peden, and C. J. Lombard (2006). Estimating the south african trauma caseload. *International Journal of Injury Control and Safety Promotion 13*(1), 49–51.

Matzopoulos, R. G., S. Truen, B. Bowman, and J. Corrigall (2013). The cost of harmful alcohol use in South Africa. *South African Medical Journal 104*(2), 127.

Mayosi, B. M., A. J. Flisher, U. G. Lalloo, F. Sitas, S. M. Tollman, and D. Bradshaw (2009). The burden of non-communicable diseases in South Africa. *Lancet 374*(9693), 934–947.

Miron, J. A. (1999). Violence and the US prohibitions of drugs and alcohol. *American Law and Economics Review 1*(1), 78–114.

Miron, J. A. and J. Zwiebel (1991). Alcohol consumption during prohibition. *American Economic Review: Papers and Proceedings*.

Murray, C. J., A. Y. Aravkin, P. Zheng, C. Abbafati, K. M. Abbas, M. Abbasi-Kangevari, F. Abd-Allah, A. Abdelalim, M. Abdollahi, I. Abdollahpour, et al. (2020). Global burden of 87 risk factors in 204 countries and territories, 1990–2019: a systematic analysis for the global burden of disease study 2019. *Lancet 396*(10258), 1223–1249.

Nakaguma, M. Y. and B. J. Restrepo (2018). Restricting access to alcohol and public health: Evidence from electoral dry laws in Brazil. *Health Economics 27*(1), 141–156.

Norman, R., R. Matzopoulos, P. Groenewald, and D. Bradshaw (2007). The high burden of injuries in South Africa. *Bulletin of the World Health Organization 85*, 695–702.

Norman, R., M. Schneider, D. Bradshaw, R. Jewkes, N. Abrahams, R. Matzopoulos, and T. Vos (2010). Interpersonal violence: an important risk factor for disease and injury in south africa. *Population Health Metrics 8*(1), 32.

O'Donoghue, T. and M. Rabin (2001). Risky behavior among youths: Some issues from behavioral economics. In *Risky behavior among youths: An economic analysis*, pp. 29–68. University of Chicago Press.

Onya, H., A. Tessera, B. Myers, and A. Flisher (2012). Community influences on adolescents' use of home-brewed alcohol in rural South Africa. *BMC Public Health 12*(1), 642.

Oster, E. (2018). Does disease cause vaccination? Disease outbreaks and vaccination response. *Journal of Health Economics 57*, 90–101.

Owens, E. G. (2011). Are underground markets really more violent? Evidence from early 20th century America. *American Law and Economics Review 13*(1), 1–44.

Parry, C. D. (2005). South Africa: alcohol today. *Addiction*.

Parry, C. D. (2010). Alcohol policy in South Africa: a review of policy development processes between 1994 and 2009. *Addiction 105*(8), 1340–1345.

Perez-Vincent, S., E. Carreras, M. Gibbons, T. E. Murphy, and M. Rossi (2020). COVID-19 lockdowns and domestic violence: Evidence from two studies in Argentina. *Working Paper*.

Phillips, R. (2014). *Alcohol: A history*. UNC Press Books.

Pillay-van Wyk, V., W. Msemburi, R. Laubscher, R. E. Dorrington, P. Groenewald, T. Glass, B. Nojilana, J. D. Joubert, R. Matzopoulos, M. Prinsloo, et al. (2016). Mortality trends and differentials in South Africa from 1997 to 2012: second National Burden of Disease Study. *Lancet Global Health 4*(9), e642–e653.

Poblete-Cazenave, R. (2020). The Great Lockdown and criminal activity-evidence from Bihar, India. *Working Paper*.

Probst, C., C. Kilian, S. Sanchez, S. Lange, and J. Rehm (2020). The role of alcohol use and drinking patterns in socioeconomic inequalities in mortality: a systematic review. *Lancet Public Health 5*(6), e324–e332.

Probst, C., C. D. Parry, H.-U. Wittchen, and J. Rehm (2018). The socioeconomic profile of alcohol-attributable mortality in South Africa: a modelling study. *BMC Medicine 16*(1), 97.

Probst, C., M. Roerecke, S. Behrendt, and J. Rehm (2014). Socioeconomic differences in alcohol-attributable mortality compared with all-cause mortality: a systematic review and meta-analysis. *International Journal of Epidemiology 43*(4), 1314–1327.

Probst, C., P. A. Shuper, and J. Rehm (2017). Coverage of alcohol consumption by national surveys in South Africa. *Addiction 112*(4), 705–710.

Ramaphosa, C. (2020). Progress in national effort to contain the Coronavirus COVID-19 pandemic. `https://www.gov.za/speeches/president-cyril-ramaphosa-progress-national-effort-contain-coronavirus-covid-19-pandemic-12`. Online; published 12-July-2020; accessed 26-September-2020.

Ravindran, S. and M. Shah (2020). Unintended consequences of lockdowns: COVID-19 and the shadow pandemic. *NBER Working Paper*.

Rehm, J., G. E. Gmel Sr, G. Gmel, O. S. Hasan, S. Imtiaz, S. Popova, C. Probst, M. Roerecke, R. Room, A. V. Samokhvalov, et al. (2017). The relationship between different dimensions of alcohol use and the burden of disease – an update. *Addiction 112*(6), 968–1001.

Rehm, J., O. S. Hasan, S. Imtiaz, C. Probst, M. Roerecke, and K. Shield (2018). Alcohol and noncommunicable disease risk. *Current Addiction Reports 5*(1), 72–85.

Rehm, J., R. Room, K. Graham, M. Monteiro, G. Gmel, and C. T. Sempos (2003). The relationship of average volume of alcohol consumption and patterns of drinking to burden of disease: an overview. *Addiction 98*(9), 1209–1228.

Rossow, I. and T. Norström (2012). The impact of small changes in bar closing hours on violence. The Norwegian experience from 18 cities. *Addiction 107*(3), 530–537.

Sanchez-Ramirez, D. C. and D. Voaklander (2018). The impact of policies regulating alcohol trading hours and days on specific alcohol-related harms: a systematic review. *Injury prevention 24*(1), 94–100.

Schilbach, F. (2019). Alcohol and Self-Control: A Field Experiment in India. *American Economic Review 109*(4), 1290–1322.

Schönberg, U. and J. Ludsteck (2014). Expansions in maternity leave coverage and mothers' labor market outcomes after childbirth. *Journal of Labor Economics 32*(3), 469–505.

Shield, K., J. Manthey, M. Rylett, C. Probst, A. Wettlaufer, C. D. Parry, and J. Rehm (2020). National, regional, and global burdens of disease from 2000 to 2016 attributable to alcohol use: a comparative risk assessment study. *Lancet Public Health 5*(1), e51–e61.

Shisana, O., D. Labadarios, T. Rehle, L. Simbayi, K. Zuma, A. Dhansay, P. Reddy, W. Parker, E. Hoosain, P. Naidoo, C. Hongoro, Z. Mchiza, N. Steyn, N. Dwane, M. Makoae, T. Maluleke, S. Ramlagan, N. Zungu, M. Evans, L. Jacobs, and M. Faber (2013). South African National Health and Nutrition Examination Survey (SANHANES-1). *Cape Town: HSRC Press*.

Silverio-Murillo, A., J. R. Balmori de la Miyar, and L. Hoehn-Velasco (2020). Families under confinement: COVID-19, domestic violence, and alcohol consumption. *Working Paper*.

van Walbeek, C., S. Filby, and K. van der Zee (2020). Smoking and quitting behaviour in lockdown South Africa: Results from a second survey. *Working Paper*.

Vellios, N. and C. Van Walbeek (2018). Self-reported alcohol use and binge drinking in South Africa: Evidence from the national income dynamics study, 2014-2015. *South African Medical Journal 108*(1), 33–39.

Wagenaar, A. C. and T. L. Toomey (2002, March). Effects of minimum drinking age laws: review and analyses of the literature from 1960 to 2000. *Journal of Studies on Alcohol, Supplement* (s14), 206–225.

Warburton, C. et al. (1932). *The economic results of prohibition*. Columbia university press New York.

WHO (2018). Safer: Preventing and reducing alcohol-related harms. `https://www.who.int/substance_abuse/safer/msb_safer_framework.pdf`. Online; accessed 28-September-2020.

WHO (2019). *Global status report on alcohol and health 2018*. World Health Organization.

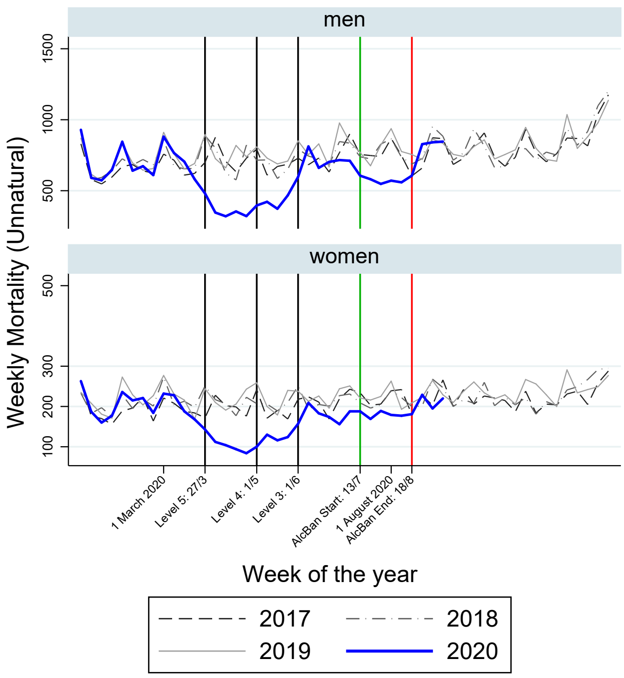
Wilkinson, C., M. Livingston, R. Room, et al. (2016). Impacts of changes to trading hours of liquor licences on alcohol-related harm: a systematic review 2005–2015. *Public Health Research and Practice 26*(4), e2641644.

APPENDICES

A Empirical regularities in unnatural mortality in South Africa

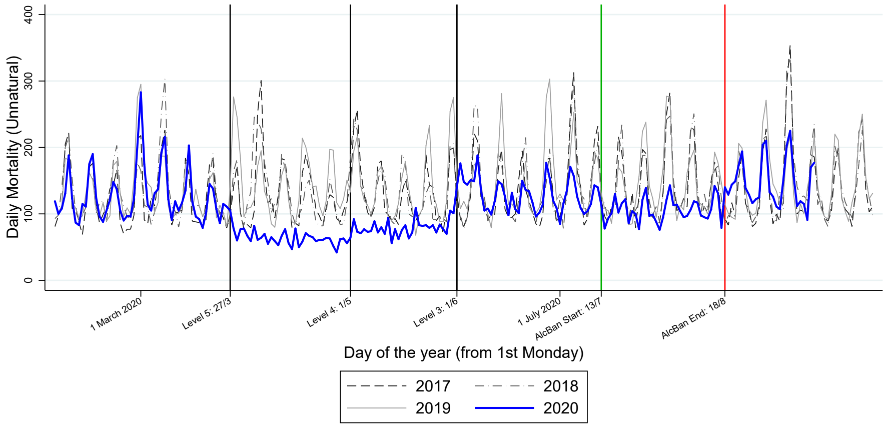
In order to study the change in unnatural mortality generated by the alcohol ban, it is important to understand the baseline patterns present in the mortality data. In this section we document several important regularities observed in the data. First, the number of daily deaths due to unnatural causes is markedly different for men and women. Figure 4 shows weekly mortality separated by gender. Comparing the scales of the two panels of the figure indicates the magnitude of the difference. We see that in 2017 to 2019 male unnatural mortality oscillated around 750 deaths per week, while female unnatural mortality oscillated slightly above 200 deaths per week.

Figure 4: Weekly mortality (unnatural deaths, by gender)



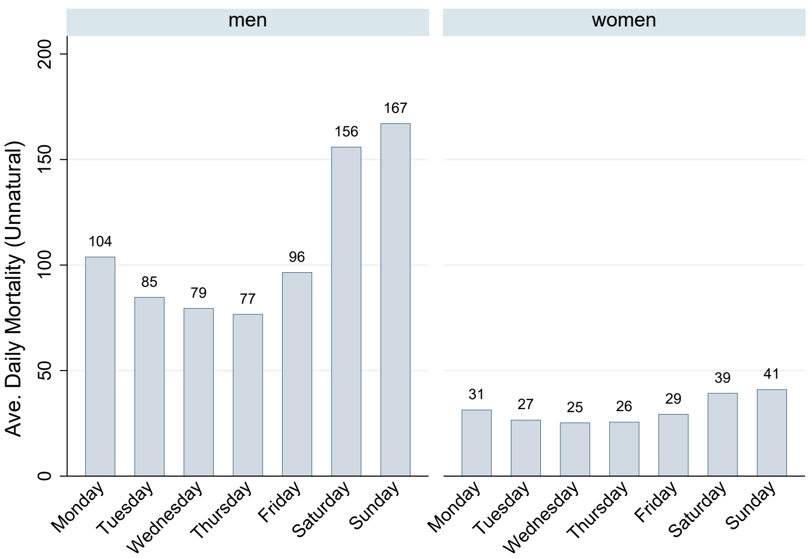
Second, unnatural mortality in South Africa follows a strong and systematic weekly pattern. Figure 5 shows this by reporting daily mortality levels, aligning days of the week across the four years by counting from the first Monday of the year. The peaks at regular intervals in the 2017, 2018, and 2019 data reflect the higher mortality levels observed on weekends. This figure reinforces the observation noted in the main body of the paper that mortality followed a highly regular pattern in the years preceding 2020 (i.e. 2017, 2018 and 2019).

Figure 5: Daily mortality (2017 to 2020)



This weekly cyclical pattern is also clearly seen in Figure 6, which shows that mortality is at least 50% higher on Saturdays and Sundays for men, and at least 25% higher for women.[22]

Figure 6: Ave. Daily mortality by day of the week (by gender, 2017-2019)



[22]Note, one reason for the higher mortality levels recorded on Saturday and Sunday (as opposed to Friday and Saturday) is that deaths that result from injuries obtained during Friday [Saturday] night are often recorded on Saturday [Sunday].

Third, Figure 7 shows that 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.

Figure 7: Ave. Daily mortality by day of the month (by gender, 2017-2019)

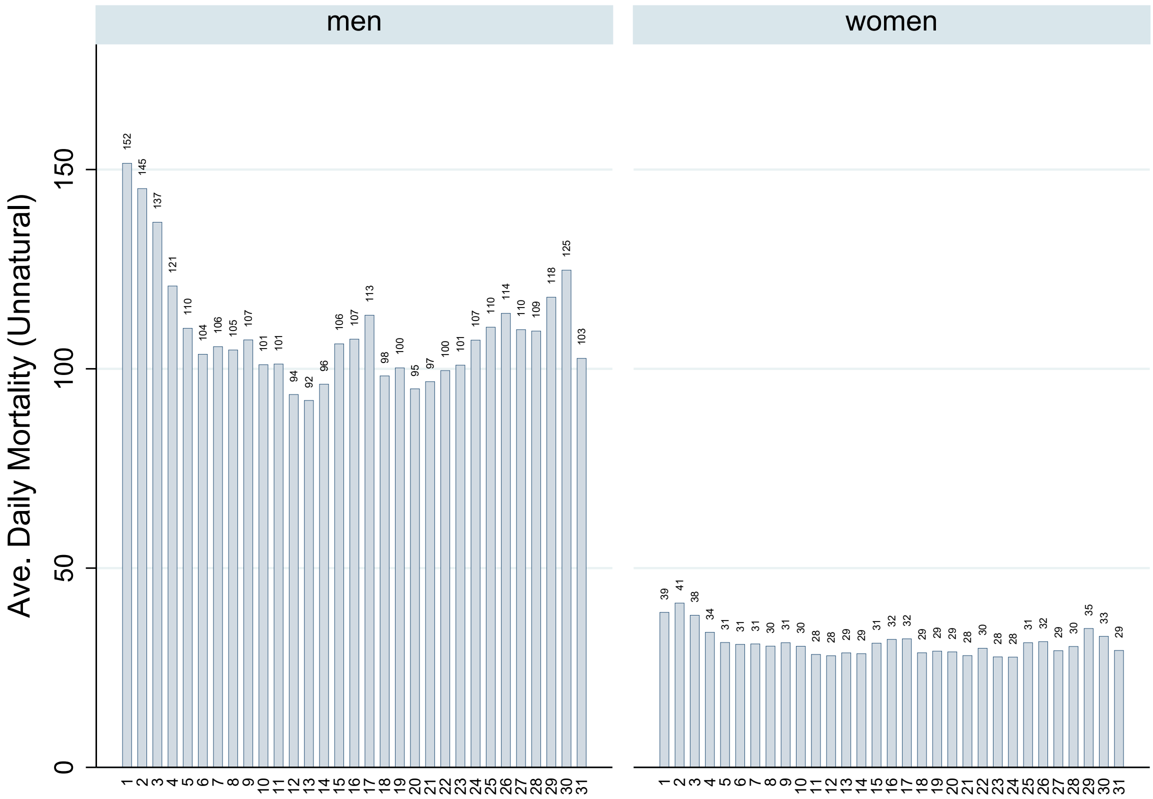
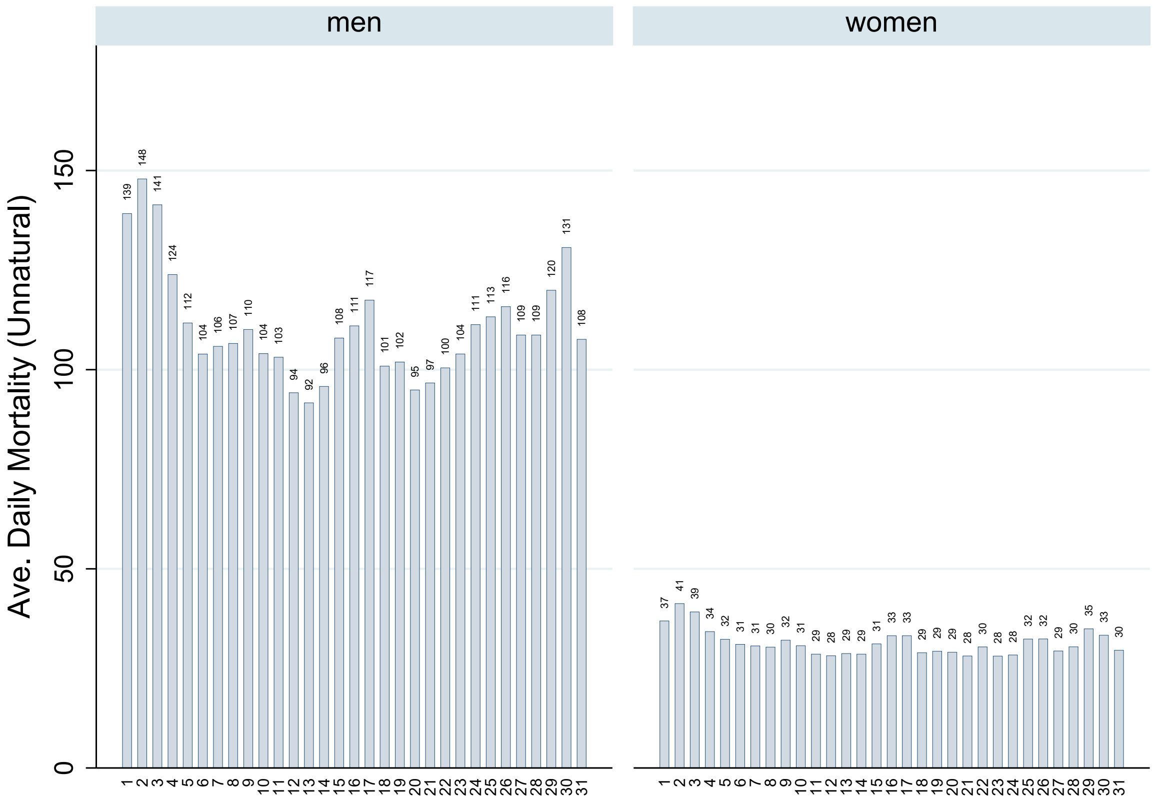


Figure 8 shows that this pattern is not driven by New Year's Eve by replicating Figure 7 except with January omitted (since the majority of New Year's Eve deaths would be recorded on the 1st of January). All of our main analyses exclude the period around New Year's Eve to avoid the influence of this unusual period.

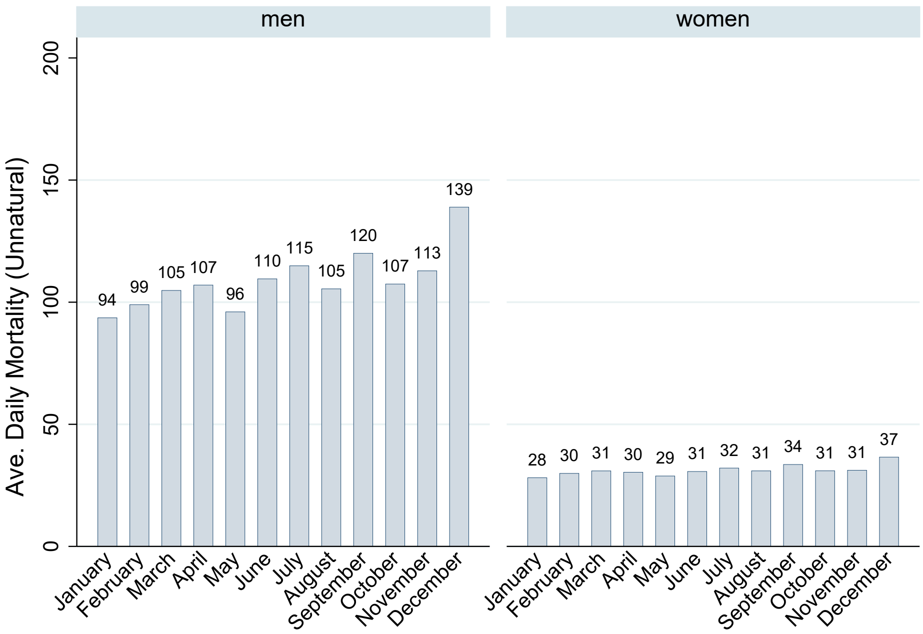
Figure 8: Ave. Daily mortality by day of the month (by gender, 2017-2019, excluding January)



Fourth, Figure 9 shows that 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.[23]

[23]It is worth noting that each of these patterns observed in the mortality data could plausibly correspond to a similar pattern in alcohol consumption. First, the prevalence of heavy episodic drinking is also substantially higher amongst men than women—WHO (2019) reports that in 2016, 7% of all females and 31% of all males aged 15 and older in the country were heavy episodic drinkers. Similarly, Vellios and Van Walbeek (2018) found that 22.8% of males and 6.4% of females over the age of 15 self-reported binge drinking (i.e. consuming more than 5 drinks per drinking session). Second, it is not unlikely that more alcohol is consumed during the weekend and close to paydays (i.e. at the end / beginning and the mid-point of the month). Third, the month of December is traditionally a holiday month in the middle of summer and contains the school holidays and also Christmas. It is therefore not implausible that more alcohol is consumed than in the average month. Since we are not aware of robust representative evidence on alcohol consumption patterns according to the day-of-the-week, day-of-the-month and month-of-the-year for South Africa, we simply point out the potential parallels in mortality and alcohol consumption patterns. Some sources that do contain detailed information on other dimensions of alcohol consumption regularities include: Shisana et al. (2013); Probst et al. (2017); WHO (2019).

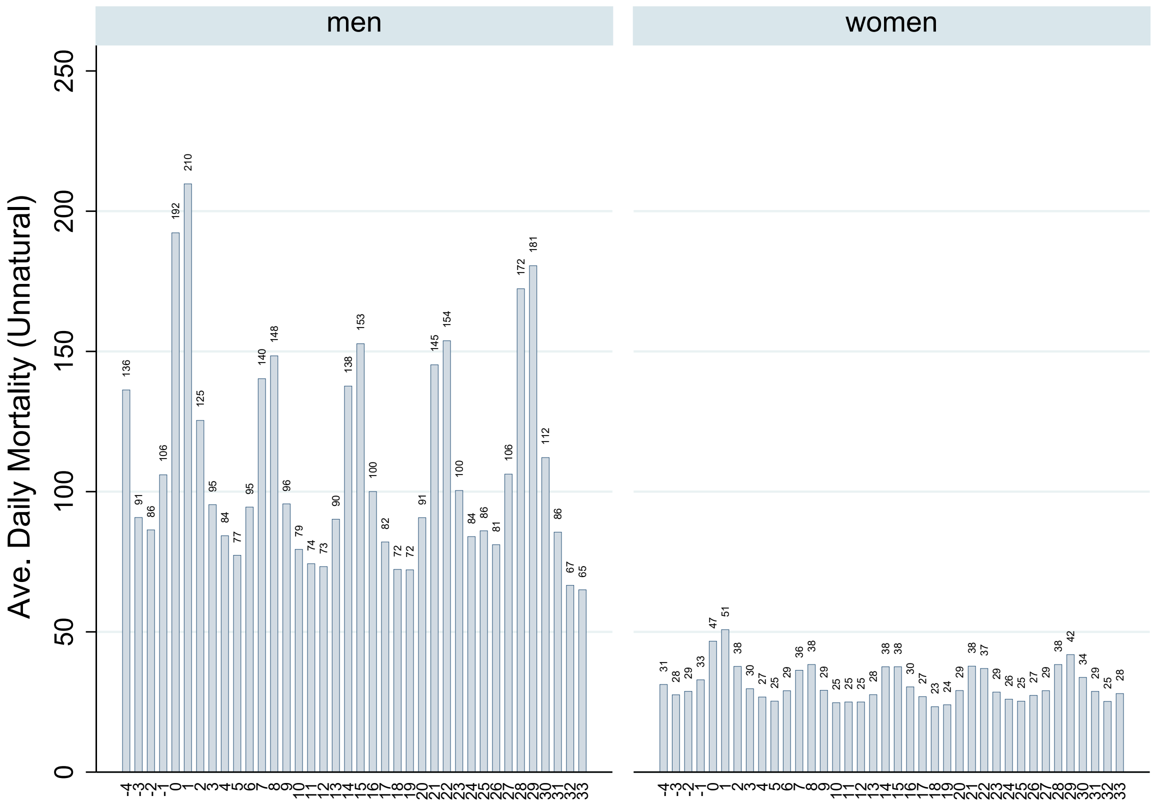
Figure 9: Ave. Daily mortality by month of the year (by gender, 2017-2019)



Last, to illustrate the influence of weekends and the monthly regularity in mortality together, Figure 10 aligns the days of the week in different months by counting the first Saturday that follows the last Monday of the previous month as day $t = 0$. For example, if the first day of a month is a Sunday, this is counted as day $t = 1$, but if the first day of the month is a Friday, it is counted as day $t = -1$. The reason for doing this is two-fold: (i) it aligns weekdays and weekends across different months, and (ii) any Saturday falling on $t = 0$ or Sunday falling on $t = 1$ will be the part of the first weekend after the last weekday of the previous month (a proxy for the payment day). This shows that weekends that fall at the beginning or end of the month (i.e. close to a payday) were associated with higher levels of mortality between 2017 and 2019.[24]

[24]It is important to note that the bars associated with dates indexed with negative numbers or numbers above 30 only draw on data from a small subset of months. The main focus of the figure is on the dates indexed from 0 to 30.

Figure 10: Ave. Daily mortality by day of the month (aligned by day of the week; 2017-2019)



B Robustness exercises

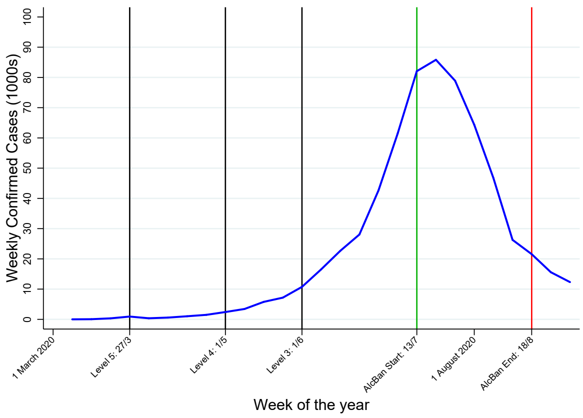
B.1 Addressing concerns regarding the quality of the natural experiment

A key requirement for a causal interpretation of the results discussed in the main text above is that the July Alcohol Ban induced an exogenous shift in alcohol consumption, and that no other change occurred at the same time that independently influenced unnatural mortality levels. There are two main candidate confounding factors that would bring our identification into question. First, the fundamental factors present in society that led to the introduction of the alcohol ban may also have shifted unnatural mortality directly. Since the alcohol ban was a reaction to the COVID-19 pandemic, in Section B.1.1 we consider the possibility that COVID-19 affected unnatural mortality through channels other than the alcohol ban. Second, in Section B.1.2 we discuss the possibility that other contemporaneous policy changes may have influenced unnatural mortality.

B.1.1 The role of COVID-19

New policies are often a reaction to other events or changes in society. Therefore, the changes that precipitated the alcohol ban could also be causing the observed change in unnatural mortality rather than the alcohol ban being the causal factor itself. As indicated above, the stated aim of the alcohol ban was to reduce pressure on hospitals during the COVID-19 pandemic by reducing alcohol-related trauma admissions.[25] A concern, therefore, is that COVID-19 induced some other important change in society that coincided with the implementation of the alcohol ban. Since we are studying unnatural mortality, which is predominantly caused by the types of behavior that individuals engage in, a key concern is that fear of COVID-19 induced a sharp reduction in risky behavior during the period of the alcohol ban.[26] While we do not have direct data on levels of fear over time, we are able to alleviate this concern by examining the progression of COVID-19 cases during this period.

Figure 11: Weekly confirmed COVID-19 cases in South Africa



Source: World Health Organization (https://covid19.who.int/)

Figure 11 reports the total number of weekly confirmed COVID-19 cases in South Africa over

[25]In the speech announcing the alcohol sales ban on the evening of July 12, 2020, the South African president, Cyril Ramaphosa stated: "[...] it is vital that we do not burden our clinics and hospitals with alcohol-related injuries that could have been avoided. [...] We have therefore decided that in order to conserve hospital capacity, the sale, dispensing and distribution of alcohol will be suspended with immediate effect." (Ramaphosa, 2020) The legal details pertaining to the policy changes are documented in Government Gazette (2020b).

[26]There is a growing body of work that documents how individuals change their behavior in response to the prevalence of, or information about, a new health risk (Ahituv et al., 1996; Lakdawalla et al., 2006; Adda, 2007; Bennett et al., 2015; Oster, 2018; Barron et al., 2019; Gamboa and Lesmes, 2019; Fetzer et al., 2020; Akesson et al., 2020).

time.[27] From this we see that the July Alcohol Ban was introduced when COVID-19 cases were already at their peak level (green vertical line). Thereafter, during the period in which the alcohol ban was in place, the number of new confirmed cases dropped rapidly. It seems implausible that the level of fear was higher when the number of cases was falling (to the right of the green line) than when it was rising (to the left of the green line). It is therefore unlikely that fear of COVID-19 was responsible for much of the reduction in unnatural mortality in mid-July 2020 by directly influencing behavior.

B.1.2 The role of the curfew

The second concern is that other policies may have changed at the same time as the alcohol ban, and that these other policy changes may have caused (part of) the reduction in mortality. While the alcohol ban was imposed unexpectedly in the middle of the Level 3 period implying that almost all other regulations stayed constant, there was one important exception to this—a curfew was introduced concurrently with the July Alcohol Ban. If the curfew affected unnatural mortality levels, then this would change the interpretation of the results: it would imply that we are estimating the effect of a reduction in alcohol consumption combined with a curfew, rather than just estimating the effect of a reduction in alcohol consumption.[28] However, for the following reasons, we believe that the alcohol ban was the key policy change that occurred on July 13, 2020. First, the introduction of the curfew did not change the legal landscape substantially: During the Level 3 period prior to the alcohol ban there was a de facto curfew for most individuals. Meeting in groups was illegal and being outside your home at night without a valid reason was also not permitted. Second, to support this claim that the curfew was not the key mechanism causing the drop in mortality, we investigate the impact of shifting the starting time of the curfew one hour later (from 9PM to 10PM) from July 31, 2020.[29] This one-hour shortening of the curfew in the middle of the alcohol ban period provides the opportunity to separately assess the influence of the curfew. If the curfew

[27]This measure of confirmed cases comes with the caveat that it represents some fraction of the true number of COVID-19 cases. As in every country, it is also plausible there may also have been some biases in the measurement (e.g. along socio-economic dimensions). However, the numbers shown in Figure 11 represent the numbers that were reported in the media and it is worth noting that the channels through which reported numbers might influence fear levels seem more direct than the channels through which the (unknown) aggregate number of actual cases in the country might influence fear levels.

[28]It is worth noting that the channels through which curfews and alcohol restrictions operate are closely linked. A curfew would typically reduce the prevalence of large groups gathering in public spaces late at night, and also likely reduce alcohol consumption. Similarly, an alcohol ban would reduce the likelihood that individuals congregate in bars and public spaces late at night, along with reducing alcohol consumption. They are essentially complementary policies that both aim to: (i) reduce large gatherings late at night, and (ii) reduce alcohol consumption.

[29]The curfew end-time remained constant at 4AM.

was effective in reducing mortality, one might expect this shift to a later start-time to be associated with an increase in mortality levels.

Table 4: Impact of the one hour curfew change on mortality (entire population)

	Men		Women	
	(1a)	(1b)	(2a)	(2b)
Level 3 Period = 1	7.73***	7.56***	0.61	0.55
(1/6-17/8)	(2.12)	(2.12)	(0.67)	(0.67)
Alcohol Ban Period = 1	0.80	4.86	0.55	1.63
(13/7-17/8)	(2.75)	(3.46)	(0.84)	(1.05)
Level 3 Period x Year=2020	9.82**	9.82**	0.75	0.75
	(4.57)	(4.58)	(1.32)	(1.32)
Alcohol Ban Period x Year=2020	-20.67***	-20.11***	-0.40	1.48
	(5.99)	(7.15)	(1.55)	(1.80)
Curfew Shortened Period = 1		-7.86**		-2.07*
(31/7-17/8)		(3.94)		(1.25)
Curfew Shortened x Year=2020		-1.11		-3.74*
		(9.59)		(2.16)
Constant	72.50***	72.51***	24.98***	24.98***
	(3.81)	(3.75)	(1.51)	(1.48)
Day of Week FEs	Y	Y	Y	Y
Day of Month FEs	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y
Observations	968	968	968	968
Adjusted R^2	0.665	0.665	0.417	0.420

Notes: (i) Each observation refers to a single day, (ii) Robust standard errors are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between the dates of January, 15 and September, 13 of each year, excluding the abnormal period around New Year's Eve and also February, 29, (iv) All columns report estimates for for the outcome variable: unnatural mortality. Columns (1*) report estimates for men, and columns (2*) report estimates for women; columns (*a) replicate the main regressions, and specifications (*b) examine the influence of the curfew on unnatural mortality.

The regression results reported in Tables 4 and 5 provide an estimate of the impact of this change in the curfew onset time. Essentially, these regressions augment our main empirical specification above to include a binary variable indicating the period during which the curfew was shortened (31/7-17/8), which comprised the second half of the alcohol ban period. The results show that there was no significant change in mortality levels (for men or women) during the period when the curfew was shortened in comparison to the rest of the alcohol ban period. In fact, the coefficient signs on the interaction variable (*Curfew Shortened x Year=2020*) are all negative—

if the shortening of the curfew increased mortality, we would expect to see positive coefficients. These results, therefore, support our assertion that the curfew is unlikely to be the main driver of the reduction in mortality.[30]

Table 5: Impact of the one hour curfew change on mortality (15-34 years)

	Men		Women	
	(1a)	(1b)	(2a)	(2b)
Level 3 Period = 1	5.37***	5.29***	-0.51	-0.52
(1/6-17/8)	(1.45)	(1.46)	(0.43)	(0.43)
Alcohol Ban Period = 1	-1.02	0.97	0.78	0.74
(13/7-17/8)	(1.86)	(2.31)	(0.52)	(0.57)
Level 3 Period x Year=2020	-0.70	-0.70	0.26	0.26
	(3.30)	(3.30)	(0.89)	(0.89)
Alcohol Ban Period x Year=2020	-11.19***	-11.02**	-1.40	-0.23
	(4.03)	(4.93)	(1.00)	(1.21)
Curfew Shortened Period = 1		-3.85		0.09
		(2.64)		(0.70)
Curfew Shortened x Year=2020		-0.34		-2.33
		(6.51)		(1.42)
Constant	34.38***	34.38***	10.10***	10.10***
	(2.58)	(2.55)	(0.86)	(0.86)
Day of Week FEs	Y	Y	Y	Y
Day of Month FEs	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y
Observations	968	968	968	968
Adjusted R^2	0.608	0.608	0.320	0.320

Notes: (i) Each observation refers to a single day, (ii) Robust standard errors are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between the dates of January, 15 and September, 13 of each year, excluding the abnormal period around New Year's Eve and also February, 29, (iv) All columns report estimates for for the outcome variable: unnatural mortality. Columns (1*) report estimates for men, and columns (2*) report estimates for women; columns (*a) replicate the main regressions, and specifications (*b) examine the influence of the curfew on unnatural mortality.

[30]It is important to note that these results do not imply that curfews are an ineffective policy tool for reducing injury. There are two reasons for this: (i) in this context, the curfew did not involve a large shift in the legal constraints placed on behavior, (ii) the effectiveness of a curfew that is implemented when alcohol is available may be very different to the effectiveness of a curfew implemented when alcohol is not available (i.e. there may be a strong interaction effect between the availability of alcohol and the effectiveness of a curfew).

B.2 Additional robustness exercises

To add further empirical support to our main results, we conduct several additional robustness exercises.

B.2.1 Placebo exercises

First, we conduct a set of placebo regressions. Essentially, these involve replicating our main analysis, but replacing 2020 with 2019. These exercises serve as a robustness check for possible confounding factors in our analysis, including the possibility that either undetected time trends in mortality, or the estimation strategy are generating the results. To do this, we replicate our main regressions, but compare mortality observed during the relevant periods in 2019 with the preceding three years (2016-2018). The results are reported in Tables 6 (all ages) and 7 (younger adults).

Table 6: Placebo regressions for impact of the alcohol ban on mortality entire population)

	Men			Women		
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
Level 3 Period = 1	7.39*	7.35***	7.13***	0.78	0.86	0.90
(1/6-17/8)	(4.44)	(2.54)	(1.91)	(0.86)	(0.70)	(0.61)
L3 Alcohol Ban Period = 1	-0.08	-0.18	2.25	0.26	0.24	0.80
(13/7-17/8)	(5.87)	(3.11)	(2.43)	(1.17)	(0.91)	(0.80)
Level 3 Period x Year=2019	9.21	8.22*	3.06	2.21	1.70	-0.27
	(8.21)	(4.79)	(4.14)	(1.85)	(1.37)	(1.34)
L3 Alcohol Ban Period x Year=2019	-3.25	-4.70	-5.04	-1.42	-1.68	-1.73
	(11.88)	(6.82)	(5.03)	(2.36)	(1.90)	(1.62)
Weekend Day = 1		74.26***			12.22***	
		(2.67)			(0.70)	
Weekend Day x Year=2019		3.50			1.80	
		(4.82)			(1.47)	
Constant	100.75***	79.57***	89.45***	29.69***	26.12***	31.58***
	(1.58)	(0.91)	(2.77)	(0.38)	(0.31)	(1.21)
Day of Week FEs			Y			Y
Day of Month FEs			Y			Y
Year FEs			Y			Y
Observations	968	968	968	968	968	968
Adjusted R^2	0.009	0.654	0.794	0.002	0.366	0.514

Notes: (i) Each observation corresponds to a single day, (ii) Robust standard errors are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2016 to 2019, between the dates of January, 15 and September, 13 of each year, excluding the abnormal period around New Year's Eve and also February, 29, (iv) All columns report estimates for the outcome variable, unnatural mortality, and differ only in their specifications, with column (*a) the simplest specification, (*b) adding controls for weekend days, and (*c) adding fixed effects, (v) The table is essentially a replication of Table 2, except shifted one year backwards in time.

Focusing on our preferred specification, reported in columns (1c) for men and (2c) for women, we observe no significant difference between mortality in 2019 and the preceding three years during the calendar period of the July alcohol sales ban for individuals of all ages. However, for younger adults, we do observe a slight reduction in the mortality level amongst men during the 2019 calendar period that corresponds to the 2020 July Alcohol Ban, significant at the 10% level. This should be kept in mind as a potential caveat to the subset of our results that focus on young adults.[31]

Table 7: Placebo regressions for impact of the alcohol ban on mortality (15 to 34 years)

		Men			Women	
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
Level 3 Period = 1	4.32	4.14***	4.14***	-0.85*	-0.82**	-0.87**
(1/6-17/8)	(2.87)	(1.59)	(1.28)	(0.46)	(0.39)	(0.38)
L3 Alcohol Ban Period = 1	0.05	-0.01	1.30	0.72	0.71	0.93*
(13/7-17/8)	(3.84)	(1.97)	(1.61)	(0.68)	(0.54)	(0.49)
Level 3 Period x Year=2019	5.70	5.64*	3.07	2.29*	2.07**	1.52*
	(5.50)	(3.05)	(2.82)	(1.19)	(0.87)	(0.88)
L3 Alcohol Ban Period x Year=2019	-4.57	-5.49	-5.82*	-1.17	-1.32	-1.36
	(7.72)	(4.36)	(3.52)	(1.51)	(1.16)	(1.01)
Weekend Day = 1		49.77***			6.93***	
		(1.79)			(0.39)	
Weekend Day x Year=2019		0.19			0.75	
		(3.22)			(0.81)	
Constant	50.39***	36.34***	40.73***	11.49***	9.48***	11.81***
	(1.04)	(0.55)	(1.89)	(0.21)	(0.17)	(0.72)
Day of Week FEs			Y			Y
Day of Month FEs			Y			Y
Year FEs			Y			Y
Observations	968	968	968	968	968	968
Adjusted R^2	0.005	0.667	0.780	0.004	0.368	0.479

Notes: (i) Each observation corresponds to a single day, (ii) Robust standard errors are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2016 to 2019, between the dates of January, 15 and September, 13 of each year, excluding the abnormal period around New Year's Eve and also February, 29, (iv) All columns report estimates for the outcome variable, unnatural mortality, and differ only in their specifications, with column (*a) the simplest specification, (*b) adding controls for weekend days, and (*c) adding fixed effects, (v) The table is essentially a replication of Table 3, except shifted one year backwards in time.

[31] However, it is worth noting that in order for this to be driving our main results for younger adults, we would need to observe an *increase* in mortality during this period in 2019. This reduction in mortality during the alcohol ban period in 2019 may therefore imply that we slightly underestimate the impact of the alcohol ban on younger adults in 2020.

B.2.2 Varying the length of the time window used in estimation

Second, we examine the influence of the time window used for our estimation. When examining the weekly and daily mortality patterns in Figures 3 and 5, a potential concern is the increase in mortality observed immediately after the relaxation of restrictions on June 1, 2020 (i.e. at the beginning of the Level 3 period). It appears that the cessation of the lockdown may have induced a short-run response that inflated mortality in the first week of Level 3. Since our identification relies on comparing the first half of Level 3 with the second half of Level 3, this raises the concern that it is this increased mortality level at the beginning of Level 3 that is driving our results, rather than a reduction in mortality during the alcohol ban period.

To address this concern, we conduct an additional robustness exercise where we vary the length of the time window around the introduction of the alcohol ban in our analysis. This essentially involves replicating our main estimation specification, but instead of using the entire Level 3 period, we rather use a smaller window of x weeks on either side of the change in policy (where $x \in \{5, 4, 3, 2\}$). We therefore compare the mortality level during the x weeks after the policy change to mortality during the x weeks before the policy change, controlling for the full set of fixed effects used in our main specification. The results are reported in Tables 8 and 9, considering time windows of between 5 weeks and 2 weeks in length.

Table 8: Varying the time window used for estimation (entire population)

			Men					Women		
	(1a)	(1b)	(1c)	(1d)	(1e)	(2a)	(2b)	(2c)	(2d)	(2e)
	Full Period	X=5 weeks	X=4 weeks	X=3 weeks	X=2 weeks	Full Period	X=5 weeks	X=4 weeks	X=3 weeks	X=2 weeks
Before & After Ban [t-X,t+X]	7.73***	8.55***	10.14***	8.91***	14.14***	0.61	0.97	1.15	0.97	1.17
	(2.12)	(2.29)	(2.64)	(3.18)	(4.24)	(0.67)	(0.71)	(0.78)	(0.86)	(1.09)
After Ban [t,t+X]	0.80	-0.21	-0.73	-0.53	-8.01	0.55	0.34	0.47	0.33	0.86
	(2.75)	(2.92)	(3.48)	(4.20)	(4.90)	(0.84)	(0.89)	(0.96)	(1.13)	(1.44)
Before & After Ban x Year=2020	9.82**	6.22	2.77	2.41	-9.56	0.75	0.22	-0.72	-1.23	-2.09
	(4.57)	(4.71)	(5.18)	(5.73)	(6.14)	(1.32)	(1.38)	(1.47)	(1.51)	(1.70)
After Ban x Year=2020	-20.67***	-17.33***	-19.56***	-16.57*	4.52	-0.40	-0.04	0.79	2.32	4.10*
	(5.99)	(6.24)	(7.23)	(8.46)	(9.20)	(1.55)	(1.64)	(1.82)	(2.07)	(2.35)
Constant	72.50***	73.14***	74.48***	75.43***	75.29***	24.98***	25.02***	25.26***	25.34***	25.22***
	(3.81)	(3.75)	(3.62)	(3.64)	(3.63)	(1.51)	(1.50)	(1.44)	(1.42)	(1.39)
Day of Week FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Day of Month FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	968	968	968	968	968	968	968	968	968	968
Adjusted R^2	0.665	0.662	0.663	0.658	0.657	0.417	0.418	0.418	0.417	0.419

Notes: (i) Each observation corresponds to a single day, (ii) Robust standard errors are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between the dates of January, 15 and September, 13 of each year, excluding the abnormal period around New Year's Eve and also February, 29, (iv) All columns report estimates for the outcome variable, unnatural mortality, and differ only in terms of the time window before and after the alcohol ban used for the estimation—this time window is indicated in the column header, (v) Columns (1*) consider men, and columns (2*) consider women.

Table 9: Varying the time window used for estimation (15-34 years of age)

	Men					Women				
	(1a)	(1b)	(1c)	(1d)	(1e)	(2a)	(2b)	(2c)	(2d)	(2e)
	Full Period	X=5 weeks	X=4 weeks	X=3 weeks	X=2 weeks	Full Period	X=5 weeks	X=4 weeks	X=3 weeks	X=2 weeks
Before & After Ban [t-X,t+X]	5.37***	5.85***	6.64***	5.58***	8.85***	-0.51	-0.21	-0.19	-0.43	-0.22
	(1.45)	(1.57)	(1.82)	(2.09)	(2.78)	(0.43)	(0.47)	(0.53)	(0.60)	(0.79)
After Ban [t,t+X]	-1.02	-1.82	-2.84	-2.60	-6.33**	0.78	0.66	0.55	0.22	0.54
	(1.86)	(1.97)	(2.35)	(2.77)	(3.20)	(0.52)	(0.55)	(0.63)	(0.70)	(0.92)
Before & After Ban x Year=2020	-0.70	-1.93	-3.19	-2.69	-9.22**	0.26	-0.59	-1.01	-0.78	-0.72
	(3.30)	(3.39)	(3.60)	(3.93)	(4.43)	(0.89)	(0.92)	(0.95)	(1.02)	(1.29)
After Ban x Year=2020	-11.19***	-9.72**	-11.23**	-8.90	1.12	-1.40	-0.87	-0.31	0.06	1.45
	(4.03)	(4.23)	(4.80)	(5.58)	(6.57)	(1.00)	(1.05)	(1.18)	(1.39)	(1.70)
Constant	34.38***	34.42***	34.67***	34.81***	34.58***	10.10***	10.18***	10.19***	10.08***	9.87***
	(2.58)	(2.51)	(2.48)	(2.38)	(2.33)	(0.86)	(0.84)	(0.82)	(0.80)	(0.79)
Day of Week FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Day of Month FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	968	968	968	968	968	968	968	968	968	968
Adjusted R^2	0.608	0.608	0.609	0.604	0.604	0.320	0.320	0.320	0.320	0.319

Notes: (i) Each observation corresponds to a single day, (ii) Robust standard errors are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between the dates of January, 15 and September, 13 of each year, excluding the abnormal period around New Year's Eve and also February, 29, (iv) All columns report estimates for the outcome variable, unnatural mortality, and differ only in terms of the time window before and after the alcohol ban used for the estimation—this time window is indicated in the column header, (v) Columns (1*) consider men, and columns (2*) consider women.

Table 8 shows that for individuals of all ages, when we use a window of 5 weeks (which essentially spans the entire Level 3 period excluding the potentially problematic first week) the results remain similar to those in our main estimation. This is also the case as we shorten the window to 4 weeks and to 3 weeks, with the estimated impact of the alcohol ban remaining fairly stable in columns (1a) to (1d) for men and (2a) to (2d) for women. However, the exception to this is that when we reduce the window to only 2 weeks in length, we no longer observe a significant coefficient estimate. There are a few potential reasons for this. First, it is possible that an alcohol sales ban takes time to translate into a substantial reduction in consumption. During the first week or two after the ban is implemented, some individuals may consume alcohol that they bought prior to the ban. This would result in a lagged or gradual realization of the impact of the ban. Second, Figure 3 shows that in previous years the mortality level immediately before the 13th of July (i.e. the alcohol ban date) was much higher than the mortality level immediately after this date.[32] While the fixed effects that we include should control for some of this variation, narrowing the window to only two weeks implies that this difference observed in previous years could influence the estimates. Consistent with this explanation, we observe a substantially larger negative coefficient on

[32]One reason for this is that the two weeks preceding the 13th of July would normally include the June payday weekend, while the two weeks after this date would not include a payday weekend. Our day-of-the-week and day-of-the-month fixed effects should control for this to a certain degree, but perhaps not completely. Once the period is extended to three or four weeks, both the period before and after would include one payday weekend.

the *After Ban* coefficient in column (1e), reflecting this difference in mortality observed before and after the alcohol ban in previous years when considering only a very narrow window.

Overall, we view these results as providing support for the validity of our main estimates, demonstrating that the higher mortality at the beginning of Level 3 is not driving the estimates, and that the results remain stable when narrowing the window to 5, 4, or 3 weeks in length.

B.2.3 Restricting the data to only the Level 3 period

Third, we replicate our main results, but restrict the data that we use to only the Level 3 calendar period. Therefore, we use data for the years 2017 to 2020, between 1 June and 17 August of each year. This allows us to estimate the following simpler version of our main estimation equation specified in the main text:

$$Mort_{y,t,g} = \alpha_0 + \alpha_1 \cdot AlcBan_{y,t} + \beta \cdot AlcBan_{y,t} \times Y_{2020} + \lambda_{DoW} + \gamma_{DoM} + \mu_{year} + \epsilon_{y,t,g} \quad (3)$$

where $Mort_{y,t,g}$ 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). In comparison to the estimates in the main text, this approach is closer to the standard difference-in-difference empirical strategy. It essentially compares mortality before and after the introduction of the July Alcohol Ban, and controls for the trends in mortality observed during the same calendar period during the previous three years. It also includes fixed effects for the day-of-the-week, day-of-the-month and month-of-the-year.

This empirical approach allows us to rule out the possibility that events occurring outside the period of interest influenced our estimation. For example, the mortality data from the COVID-19 lockdown between March and May 2020 is completely excluded from this analysis, and therefore cannot influence the estimates. An additional attractive feature of this approach is that it allows us to check whether our main results are robust to using this simpler empirical strategy.

The results from this exercise are reported in Tables 10 (all ages) and 11 (young adults). In comparison to our main results, these estimates using a smaller sample size are less stable across the different specifications. However, when we focus on our preferred specification, which includes fixed effects, in the (*c) columns in both tables all the estimates for the interaction term of interest, *Alcohol Ban Period* $\times$ *Year=2020*, are very close to those in our main results. These results, therefore, provide support for the validity of the estimates in the main text.

Table 10: Impact of the alcohol ban on mortality (only considering the Level 3 period)

	Men			Women		
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
Alcohol Ban Period = 1	0.25	-3.76	0.02	1.19	0.53	0.39
(13/7-17/8)	(5.35)	(2.92)	(2.64)	(1.08)	(0.89)	(0.87)
Alcohol Ban Period x Year=2020	-29.81***	-13.86***	-20.61***	-5.85***	-3.23***	-0.41
	(4.96)	(3.23)	(6.46)	(1.19)	(1.11)	(1.53)
Weekend Day = 1		84.18***			12.56***	
		(4.55)			(1.24)	
Weekend Day x Year=2020		-54.63***			-9.03***	
		(5.45)			(1.62)	
Constant	110.07***	89.92***	92.52***	30.05***	27.11***	26.90***
	(3.29)	(1.76)	(5.54)	(0.70)	(0.56)	(2.12)
Day of Week FEs			Y			Y
Day of Month FEs			Y			Y
Year FEs			Y			Y
Observations	312	312	312	312	312	312
Adjusted R^2	0.045	0.709	0.747	0.034	0.371	0.490

Notes: (i) Each observation corresponds to a single day, (ii) Robust standard errors are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between the dates of June, 1 and August, 17 of each year (i.e. only the Level 3 period) (iv) All columns report estimates for the outcome variable, unnatural mortality, and differ only in their specifications, with column (*a) the simplest specification, (*b) adding controls for weekend days, and (*c) adding fixed effects.

Table 11: Impact on mortality of 15-34 year olds (only considering the Level 3 period)

	Men			Women		
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
Alcohol Ban Period = 1	-0.95	-3.63*	-1.78	0.69	0.29	0.76
(13/7-17/8)	(3.45)	(1.86)	(1.84)	(0.64)	(0.53)	(0.54)
Alcohol Ban Period x Year=2020	-18.39***	-7.74***	-11.11***	-2.64***	-1.02	-1.41
	(3.08)	(1.94)	(4.20)	(0.68)	(0.66)	(1.01)
Weekend Day = 1		54.96***			7.04***	
		(3.05)			(0.77)	
Weekend Day x Year=2020		-36.52***			-5.58***	
		(3.95)			(1.13)	
Constant	55.59***	42.49***	37.57***	11.08***	9.47***	10.11***
	(2.16)	(1.08)	(3.42)	(0.40)	(0.32)	(1.28)
Day of Week FEs			Y			Y
Day of Month FEs			Y			Y
Year FEs			Y			Y
Observations	312	312	312	312	312	312
Adjusted R^2	0.044	0.717	0.729	0.018	0.330	0.407

Notes: (i) Each observation corresponds to a single day, (ii) Robust standard errors are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between the dates of June, 1 and August, 17 of each year (i.e. only the Level 3 period) (iv) All columns report estimates for the outcome variable, unnatural mortality, and differ only in their specifications, with column (*a) the simplest specification, (*b) adding controls for weekend days, and (*c) adding fixed effects.

C Unnatural mortality during the COVID-19 Lockdown

While the central focus of this paper is to evaluate the impact of the July Alcohol Ban, it is clear from Figure 3 that unnatural mortality dropped substantially during other COVID-19 policy response periods in 2020—especially during the full lockdown (Levels 5 and 4). It is therefore also informative to provide careful documentation of how unnatural mortality changed during each policy phase. As an auxiliary result, we therefore estimate the magnitude of the change in unnatural mortality during the earlier policy phases of the COVID-19 pandemic. Here, we do not claim to be able to identify the causal mechanism, but rather simply document the deviation in unnatural mortality relative to the pre-COVID benchmark. It is likely that the change in unnatural mortality during lockdown was influenced by both the policies implemented by the government as well as by changes in individual behaviour due to fear of the pandemic. The lockdown regulations during Level 5 and Level 4 in South Africa were extremely strict, including highly restrictive stay-at-home orders as well as a ban on the sale of alcohol that preceded the July Alcohol Ban studied in this paper.[33] In addition, during these early stages of the pandemic when the full lockdown was in place (March 27 - May 31), the level of fear and uncertainty regarding the pandemic was high. Given the various changes occurring in society during the tumultuous early period of the pandemic, it is difficult to parse the different causal factors from one another. We therefore offer a measure of the aggregate change in unnatural mortality as a contribution to the documentation of how society was influenced during different periods of this pandemic.

C.1 The impact of other COVID-19 policy bundles (Levels 5, 4 and 3)

Table 12 reports the estimates the change in unnatural mortality in the population during these periods using a regression analysis similar to the empirical approach used for our main results. The interaction terms provide an estimate of the shift in mortality during each of the policy response periods in 2020. We find that both the Level 5 (most extreme) and Level 4 lockdown periods reduced mortality markedly for men and women. We observe a reduction of 61 deaths per day for men and 14 deaths per day for women in Level 5. In the slightly more relaxed Level 4 period, we observe a reduction in mortality by 42 deaths per day for men and 11 deaths per day for women. These numbers indicate a large number of lives saved in terms of short-run mortality

[33] Level 4: (i) permitted individuals to leave the house for exercise between 6am and 9am within a 5km radius of their residence, (ii) introduced mandatory mask wearing in public, (iii) put in place a curfew between 8pm and 5am, but (iv) allowed some businesses in specific industries to re-open (e.g. mining, agriculture, energy, manufacturing, construction and public works, wholesale trade), (v) alcohol and cigarette sales remained banned as in Level 5 (Government Gazette, 2020a,d).

due to unnatural causes. However, it is important to keep in mind that these lockdown periods also imposed massive costs on society in terms of their economic impact and the resulting hunger, poverty and hardship they generated.

Table 12: Impact of different COVID-19 policy responses on mortality (All ages)

	Men			Women		
	M1	M2	M3	W1	W2	W3
LD Light Period = 1	-8.59	-11.69***	-0.83	-3.54**	-4.10***	-1.74
	(6.42)	(3.18)	(3.01)	(1.40)	(1.58)	(1.53)
Level 5 Period = 1	4.91	2.25	4.32	-0.25	-0.74	-0.72
	(4.79)	(3.29)	(2.66)	(1.12)	(0.93)	(0.87)
Level 4 Period = 1	-6.69*	-7.31***	-4.50**	-1.58	-1.72*	-1.58*
	(3.91)	(2.30)	(1.94)	(1.06)	(0.90)	(0.80)
Level 3 Period = 1	9.57**	6.91**	7.77***	1.00	0.52	0.10
	(4.75)	(2.75)	(2.25)	(1.00)	(0.81)	(0.73)
L3 Alcohol Ban Period = 1	-1.98	-2.08	0.74	-0.18	-0.20	0.52
	(5.98)	(3.23)	(2.72)	(1.19)	(0.95)	(0.84)
Treatment year (2020)						
LD Light x Year=2020	-7.25	4.76	-5.36	-3.37	-1.22	-1.46
	(8.94)	(5.24)	(7.42)	(2.37)	(2.23)	(2.34)
Level 5 x Year=2020	-58.11***	-47.89***	-60.51***	-15.33***	-13.46***	-14.27***
	(4.50)	(4.29)	(6.09)	(1.33)	(1.36)	(1.76)
Level 4 x Year=2020	-34.90***	-28.11***	-42.04***	-11.06***	-9.74***	-10.81***
	(3.75)	(3.82)	(6.80)	(1.25)	(1.29)	(1.84)
Level 3 Period x Year=2020	-8.91*	1.31	-11.31***	-5.47***	-3.59***	-4.41***
	(5.34)	(3.72)	(4.20)	(1.23)	(1.11)	(1.36)
L3 Alcohol Ban Period x Year=2020	-20.90***	-20.50***	-20.67***	-0.38	-0.32	-0.40
	(7.29)	(4.81)	(6.02)	(1.71)	(1.47)	(1.56)
Weekend Day = 1		73.59***			12.27***	
		(2.65)			(0.75)	
Weekend Day x Year=2020		-35.78***			-6.56***	
		(5.00)			(1.41)	
Constant	102.72***	84.35***	93.61***	30.42***	27.40***	30.62***
	(2.16)	(1.23)	(3.49)	(0.53)	(0.41)	(1.43)
Day of Week FEs			Y			Y
Day of Month FEs			Y			Y
Year FEs			Y			Y
Observations	968	968	968	968	968	968
Adjusted R^2	0.110	0.653	0.733	0.146	0.407	0.499

Notes: (i) Each observation corresponds to a single day, (ii) Robust standard errors are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between the dates of January, 15 and September, 13 of each year, excluding the abnormal period around New Year's Eve and also February, 29, (iv) All columns report estimates for the outcome variable, unnatural mortality, and differ only in their specifications, with column (*a) the simplest specification, (*b) adding controls for weekend days, and (*c) adding fixed effects, (v) The specification is similar to that used in our main estimations, with additional dummy variables (and 2020 interaction terms) added for each lockdown period.

Table 13 reproduces these regression results but focuses on the restricted sample of adults between the ages of 15 and 34 years. The pattern of results is similar, with Level 5 associated with 39 fewer male and 9 fewer female unnatural deaths amongst young adults per day. Similarly, Level 4 was associated with a reduction in daily unnatural mortality by 31 for men and 7 for women.

Table 13: Impact of different COVID-19 policy responses on mortality (15 to 34 years)

	Men			Women		
	M1	M2	M3	W1	W2	W3
LD Light Period = 1	-4.17	-5.86***	2.04	-1.53*	-1.78**	-0.06
	(4.58)	(1.88)	(1.89)	(0.89)	(0.87)	(0.78)
Level 5 Period = 1	1.42	0.00	2.42	0.07	-0.14	0.34
	(3.20)	(2.13)	(1.73)	(0.61)	(0.52)	(0.48)
Level 4 Period = 1	-6.04**	-6.15***	-2.96**	-1.95***	-1.98***	-1.34***
	(2.61)	(1.59)	(1.28)	(0.62)	(0.52)	(0.47)
Level 3 Period = 1	4.64	3.23*	5.43***	-0.84	-1.06**	-0.69
	(3.17)	(1.86)	(1.55)	(0.60)	(0.49)	(0.46)
L3 Alcohol Ban Period = 1	-2.71	-2.77	-1.06	0.38	0.37	0.77
	(3.87)	(2.08)	(1.85)	(0.71)	(0.57)	(0.52)
Treatment year						
LD Light x Year=2020	-7.79	-1.14	-13.60***	-1.58	-0.61	-3.03***
	(5.64)	(3.12)	(5.27)	(1.17)	(1.04)	(1.13)
Level 5 x Year=2020	-30.75***	-25.40***	-39.28***	-7.49***	-6.68***	-9.40***
	(2.93)	(2.96)	(4.57)	(0.73)	(0.81)	(1.16)
Level 4 x Year=2020	-19.22***	-16.22***	-30.81***	-4.39***	-3.91***	-6.74***
	(2.53)	(2.91)	(4.98)	(0.71)	(0.75)	(1.13)
Level 3 Period x Year=2020	-7.04**	-1.69	-15.57***	-1.25*	-0.45	-3.17***
	(3.52)	(2.54)	(3.63)	(0.73)	(0.71)	(1.00)
L3 Alcohol Ban Period x Year=2020	-11.35**	-11.06***	-11.19***	-1.38	-1.35	-1.40
	(4.68)	(3.23)	(4.04)	(1.00)	(0.93)	(1.01)
Weekend Day = 1		47.25***			6.60***	
		(1.78)			(0.43)	
Weekend Day x Year=2020		-18.74***			-2.81***	
		(4.60)			(1.01)	
Constant	52.71***	40.62***	49.19***	12.24***	10.57***	13.69***
	(1.55)	(0.99)	(3.16)	(0.32)	(0.27)	(0.93)
Day of Week FEs			Y			Y
Day of Month FEs			Y			Y
Year FEs			Y			Y
Observations	968	968	968	968	968	968
Adjusted R^2	0.077	0.595	0.678	0.094	0.330	0.419

Notes: (i) Each observation corresponds to a single day, (ii) Robust standard errors are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between the dates of January, 15 and September, 13 of each year, excluding the abnormal period around New Year's Eve and also February, 29, (iv) All columns report estimates for the outcome variable, unnatural mortality, and differ only in their specifications, with column (*a) the simplest specification, (*b) adding controls for weekend days, and (*c) adding fixed effects, (v) The specification is similar to that used in our main estimations, with additional dummy variables (and 2020 interaction terms) added for each lockdown period.

For readers interested in seeing the raw data, in Tables 14 and 15 below we report descriptive statistics summarize the mean mortality levels observed during different phases of 2020 in comparison to 2019. Table 14 considers the population as a whole, while Table 15 restricts attention to young adults aged 15 to 34. For the population as a whole, the pattern of mortality observed in the raw data is completely in line with the regression results. We see that for both men and women, mortality levels were markedly lower in 2020 than in 2019 during the Level 5 and Level 4 periods—for individuals of all ages, unnatural mortality was halved during Level 5, and reduced by more than 40% during Level 4. This reduction was even more acute on weekends. For example, during the Level 5 period, unnatural mortality in 2019 on Saturdays and Sundays averaged 160 deaths per day in comparison to 51.8 in 2020, implying a reduction of more than 67%. Amongst younger adults, Table 15 shows that the reduction in mortality in 2020 largely mirrored the patterns observed in the general population.

Tables 16 and 17 are similar to Tables 14 and 15, but compare 2019 with 2018. These can be viewed as a type of placebo comparison, showing that mortality levels prior to 2020 were fairly stable year-on-year. Taken together, these results indicate that there were no systematic differences in mortality levels during the periods of interest in 2019 and 2018—i.e. none of the twenty-four comparisons that consider average mortality across the entire week are statistically significant.[34]

[34]When considering all 72 comparisons, including those that disaggregate the data into weekdays and weekend days, we observe six differences that are statistically significant at the 10% level or lower. However, none of these differences pertain to the alcohol ban period of interest. In addition, given the number of statistical tests conducted, and the lack of a detectable systematic pattern, it is plausible that these statistical differences are generated by random variation.

C.2 Descriptive Statistics

C.2.1 Comparison of 2020 with 2019

Table 14: Comparison of daily mortality in 2019 and 2020 (entire population)

		Entire Week			Mon-Fri			Sat-Sun		
Date	Lockdown Phase	2019	2020	Diff.	2019	2020	Diff.	2019	2020	Diff.
	Men									
18Jan-18Mar	Pre-LD	103.10	99.68	-3.42	82.86	82.23	-0.62	150.33	143.82	-6.51
		(38.36)	(37.92)		(14.39)	(17.89)		(35.11)	(39.66)	
19Mar-26Mar	LD Light	95.88	86.88	-9.00	82.67	78.50	-4.17	135.50	112.00	-23.50**
		(27.04)	(19.81)		(13.00)	(14.57)		(9.19)	(1.41)	
27Mar-30Apr	L5	110.00	49.51	-60.49***	89.96	48.56	-41.40***	160.10	51.90	-108.20***
		(41.39)	(8.30)		(24.51)	(8.88)		(31.06)	(6.40)	
1May-31May	L4	101.97	61.13	-40.84***	85.65	58.86	-26.80***	148.88	65.90	-82.97***
		(31.68)	(10.34)		(14.69)	(10.18)		(15.27)	(9.43)	
1Jun-12Jul	L3 PreAlcBan	118.71	103.38	-15.33*	94.67	94.77	0.10	178.83	124.92	-53.92***
		(48.07)	(21.30)		(18.49)	(17.88)		(46.82)	(11.85)	
13Jul-17Aug	L3+AlcBan	114.03	80.50	-33.53***	89.08	74.81	-14.27***	170.73	95.30	-75.43***
		(45.66)	(15.77)		(19.87)	(13.32)		(35.43)	(11.70)	
	Women									
18Jan-18Mar	Pre-LD	32.35	29.33	-3.02	28.14	26.58	-1.56	42.17	36.29	-5.87
		(10.42)	(10.03)		(6.58)	(6.37)		(11.26)	(13.90)	
19Mar-26Mar	LD Light	25.88	23.50	-2.38	27.33	21.50	-5.83	21.50	29.50	8.00
		(6.49)	(5.98)		(6.92)	(4.76)		(2.12)	(6.36)	
27Mar-30Apr	L5	30.83	14.83	-16.00***	27.48	15.12	-12.36***	39.20	14.10	-25.10***
		(9.74)	(5.33)		(8.18)	(5.64)		(8.43)	(4.68)	
1May-31May	L4	30.87	17.77	-13.10***	27.43	17.43	-10.01***	40.75	18.50	-22.25***
		(11.20)	(4.73)		(8.57)	(5.16)		(12.50)	(3.81)	
1Jun-12Jul	L3 PreAlcBan	32.76	25.95	-6.81***	27.80	24.33	-3.47**	45.17	30.00	-15.17***
		(11.06)	(5.78)		(6.82)	(5.48)		(9.90)	(4.53)	
13Jul-17Aug	L3+AlcBan	32.00	25.39	-6.61***	29.32	24.12	-5.20***	38.09	28.70	-9.39***
		(7.58)	(5.23)		(6.28)	(4.84)		(6.91)	(4.97)	

Notes: (i) Mean daily unnatural mortality values of the raw data for the periods indicated in the *date* column are reported for 2019 and 2020, (ii) Standard deviations are reported in parentheses, (iii) The *diff.* column reports the difference in means between 2019 and 2020, (iv) A Wald test is used to check for a statistically significant difference between the means, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (v) The *Entire Week* columns include all seven days of the week, while the *Mon-Fri* columns focus on only the weekdays, and the *Mon-Fri* columns focus only on the weekend days.

Table 15: Comparison of daily mortality in 2019 and 2020 (15-34 years)

		Entire Week			Mon-Fri			Sat-Sun		
Date	Lockdown Phase	2019	2020	Diff.	2019	2020	Diff.	2019	2020	Diff.
Men										
18Jan-18Mar	Pre-LD	50.00	46.70	-3.30	37.12	35.74	-1.37	80.06	74.41	-5.64
		(23.87)	(24.08)		(8.27)	(10.45)		(21.11)	(26.71)	
19Mar-26Mar	LD Light	47.88	40.75	-7.13	38.17	35.83	-2.33	77.00	55.50	-21.50***
		(18.35)	(10.94)		(3.92)	(7.17)		(4.24)	(0.71)	
27Mar-30Apr	L5	55.94	23.37	-32.57***	42.96	22.40	-20.56***	88.40	25.80	-62.60***
		(26.13)	(5.15)		(14.95)	(4.77)		(18.57)	(5.49)	
1May-31May	L4	49.48	27.45	-22.03***	39.48	25.76	-13.72***	78.25	31.00	-47.25***
		(20.27)	(7.88)		(9.49)	(6.28)		(14.28)	(9.94)	
1Jun-12Jul	L3 PreAlcBan	61.12	50.31	-10.81*	44.53	44.17	-0.37	102.58	65.67	-36.92***
		(32.46)	(14.16)		(10.50)	(10.42)		(31.79)	(10.06)	
13Jul-17Aug	L3+AlcBan	56.28	36.25	-20.03***	40.56	33.31	-7.25***	92.00	43.90	-48.10***
		(28.73)	(9.01)		(10.68)	(6.53)		(24.36)	(10.35)	
Women										
18Jan-18Mar	Pre-LD	12.23	10.92	-1.32	10.12	9.49	-0.63	17.17	14.53	-2.64
		(5.42)	(5.45)		(3.39)	(4.17)		(6.11)	(6.68)	
19Mar-26Mar	LD Light	10.00	9.13	-0.88	10.67	8.17	-2.50	8.00	12.00	4.00**
		(2.88)	(2.47)		(3.01)	(2.04)		(1.41)	(0.00)	
27Mar-30Apr	L5	12.66	4.83	-7.83***	11.08	5.04	-6.04***	16.60	4.30	-12.30***
		(4.99)	(3.02)		(4.66)	(3.41)		(3.47)	(1.77)	
1May-31May	L4	11.10	5.90	-5.19***	9.00	5.62	-3.38***	17.13	6.50	-10.63***
		(6.40)	(2.66)		(4.09)	(2.58)		(8.18)	(2.88)	
1Jun-12Jul	L3 PreAlcBan	12.86	10.14	-2.71**	9.50	9.67	0.17	21.25	11.33	-9.92***
		(7.23)	(3.44)		(4.26)	(3.10)		(6.28)	(4.08)	
13Jul-17Aug	L3+AlcBan	12.64	9.14	-3.50***	10.80	8.92	-1.88*	16.82	9.70	-7.12***
		(4.72)	(2.78)		(3.93)	(2.73)		(3.63)	(2.98)	

Notes: (i) Mean daily unnatural mortality values of the raw data for the periods indicated in the *date* column are reported for 2019 and 2020, (ii) Standard deviations are reported in parentheses, (iii) The *diff.* column reports the difference in means between 2019 and 2020, (iv) A Wald test is used to check for a statistically significant difference between the means, $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$, (v) The *Entire Week* columns include all seven days of the week, while the *Mon-Fri* columns focus on only the weekdays, and the *Mon-Fri* columns focus only on the weekend days.

C.2.2 Comparison of 2019 with 2018 (Placebo comparisons)

Table 16: Placebo comparison of daily mortality in 2018 and 2019 (entire population)

		Entire Week			Mon-Fri			Sat-Sun		
Date	Lockdown Phase	2018	2019	Diff.	2018	2019	Diff.	2018	2019	Diff.
Men										
18Jan-18Mar	Pre-LD	99.88	103.10	3.22	80.29	82.86	2.57	145.61	150.33	4.72
		(36.73)	(38.36)		(15.48)	(14.39)		(30.66)	(35.11)	
19Mar-26Mar	LD Light	91.38	95.88	4.50	73.83	82.67	8.83	144.00	135.50	-8.50
		(34.11)	(27.04)		(11.91)	(13.00)		(7.07)	(9.19)	
27Mar-30Apr	L5	107.11	110.00	2.89	89.64	89.96	0.32	150.80	160.10	9.30
		(43.98)	(41.39)		(32.33)	(24.51)		(39.39)	(31.06)	
1May-31May	L4	91.13	101.97	10.84	75.96	85.65	9.70**	134.75	148.88	14.13
		(31.22)	(31.68)		(15.21)	(14.69)		(22.77)	(15.27)	
1Jun-12Jul	L3 PreAlcBan	111.64	118.71	7.07	86.37	94.67	8.30*	174.83	178.83	4.00
		(47.23)	(48.07)		(19.57)	(18.49)		(34.73)	(46.82)	
13Jul-17Aug	L3+AlcBan	109.83	114.03	4.19	86.69	89.08	2.39	170.00	170.73	0.73
		(42.02)	(45.66)		(14.22)	(19.87)		(27.14)	(35.43)	
Women										
18Jan-18Mar	Pre-LD	30.57	32.35	1.78	26.60	28.14	1.55	39.83	42.17	2.33
		(9.65)	(10.42)		(5.98)	(6.58)		(10.34)	(11.26)	
19Mar-26Mar	LD Light	27.50	25.88	-1.63	27.00	27.33	0.33	29.00	21.50	-7.50
		(6.26)	(6.49)		(6.87)	(6.92)		(5.66)	(2.12)	
27Mar-30Apr	L5	30.97	30.83	-0.14	28.96	27.48	-1.48	36.00	39.20	3.20
		(8.84)	(9.74)		(8.68)	(8.18)		(7.39)	(8.43)	
1May-31May	L4	28.84	30.87	2.03	26.09	27.43	1.35	36.75	40.75	4.00
		(7.93)	(11.20)		(6.61)	(8.57)		(5.97)	(12.50)	
1Jun-12Jul	L3 PreAlcBan	30.86	32.76	1.90	28.47	27.80	-0.67	36.83	45.17	8.33*
		(8.58)	(11.06)		(5.79)	(6.82)		(11.46)	(9.90)	
13Jul-17Aug	L3+AlcBan	30.47	32.00	1.53	27.12	29.32	2.20	39.20	38.09	-1.11
		(8.44)	(7.58)		(5.45)	(6.28)		(8.79)	(6.91)	

Notes: (i) Mean daily unnatural mortality values of the raw data for the periods indicated in the *date* column are reported for 2018 and 2019, (ii) Standard deviations are reported in parentheses, (iii) The *diff.* column reports the difference in means between 2018 and 2019, (iv) A Wald test is used to check for a statistically significant difference between the means, $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$, (v) The *Entire Week* columns include all seven days of the week, while the *Mon-Fri* columns focus on only the weekdays, and the *Mon-Fri* columns focus only on the weekend days.

Table 17: Placebo comparison of daily mortality in 2018 and 2019 (15-34 years)

		Entire Week			Mon-Fri			Sat-Sun		
Date	Lockdown Phase	2018	2019	Diff.	2018	2019	Diff.	2018	2019	Diff.
Men										
18Jan-18Mar	Pre-LD	48.18	50.00	1.82	35.81	37.12	1.31	77.06	80.06	3.00
		(22.73)	(23.87)		(8.85)	(8.27)		(18.50)	(21.11)	
19Mar-26Mar	LD Light	45.63	47.88	2.25	33.00	38.17	5.17*	83.50	77.00	-6.50
		(24.14)	(18.35)		(4.69)	(3.92)		(12.02)	(4.24)	
27Mar-30Apr	L5	51.49	55.94	4.46	39.36	42.96	3.60	81.80	88.40	6.60
		(28.51)	(26.13)		(19.46)	(14.95)		(25.10)	(18.57)	
1May-31May	L4	43.32	49.48	6.16	33.96	39.48	5.52**	70.25	78.25	8.00
		(18.64)	(20.27)		(7.94)	(9.49)		(13.20)	(14.28)	
1Jun-12Jul	L3 PreAlcBan	56.02	61.12	5.10	40.23	44.53	4.30	95.50	102.58	7.08
		(30.05)	(32.46)		(12.23)	(10.50)		(24.30)	(31.79)	
13Jul-17Aug	L3+AlcBan	52.56	56.28	3.72	37.96	40.56	2.60	90.50	92.00	1.50
		(26.89)	(28.73)		(9.71)	(10.68)		(18.28)	(24.36)	
Women										
18Jan-18Mar	Pre-LD	11.48	12.23	0.75	9.24	10.12	0.88	16.72	17.17	0.44
		(5.15)	(5.42)		(3.41)	(3.39)		(4.76)	(6.11)	
19Mar-26Mar	LD Light	11.63	10.00	-1.63	10.33	10.67	0.33	15.50	8.00	-7.50
		(5.40)	(2.88)		(4.97)	(3.01)		(6.36)	(1.41)	
27Mar-30Apr	L5	12.03	12.66	0.63	10.80	11.08	0.28	15.10	16.60	1.50
		(4.77)	(4.99)		(4.33)	(4.66)		(4.61)	(3.47)	
1May-31May	L4	10.00	11.10	1.10	8.30	9.00	0.70	14.88	17.13	2.25
		(4.41)	(6.40)		(3.36)	(4.09)		(3.36)	(8.18)	
1Jun-12Jul	L3 PreAlcBan	10.88	12.86	1.98	9.60	9.50	-0.10	14.08	21.25	7.17***
		(4.58)	(7.23)		(3.37)	(4.26)		(5.70)	(6.28)	
13Jul-17Aug	L3+AlcBan	11.22	12.64	1.42	9.38	10.80	1.42	16.00	16.82	0.82
		(5.17)	(4.72)		(3.34)	(3.93)		(6.15)	(3.63)	

Notes: (i) Mean daily unnatural mortality values of the raw data for the periods indicated in the *date* column are reported for 2018 and 2019, (ii) Standard deviations are reported in parentheses, (iii) The *diff.* column reports the difference in means between 2018 and 2019, (iv) A Wald test is used to check for a statistically significant difference between the means, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (v) The *Entire Week* columns include all seven days of the week, while the *Mon-Fri* columns focus on only the weekdays, and the *Mon-Fri* columns focus only on the weekend days.

D A comment on gender-based violence in South Africa during COVID-19

Interestingly, while many other countries observed spikes in gender-based violence (GBV) during COVID-19 lockdown periods (Agüero, 2020; Anderberg et al., 2020; Boserup et al., 2020; Bullinger et al., 2020; Leslie and Wilson, 2020; Mahmud and Riley, 2020; Perez-Vincent et al., 2020; Ravindran and Shah, 2020; Silverio-Murillo et al., 2020), the emerging evidence from telephonic reporting and counselling services, as well as police data, suggests that there was no such increase in GBV during the lockdown in South Africa (Gould, 2020). This is in spite of the fact that the South African lockdown (Level 5 + Level 4) was one of the strictest worldwide and lasted for 66 days. One hypothesis is that the first alcohol sales ban that was implemented during the Level 5 and Level 4 periods acted as a countervailing force and reduced the propensity for partners to resort to extreme physical violence. However, there are several caveats to these observations. First, the evidence on GBV in South Africa cited here does not come from a peer-reviewed academic research article and therefore should be viewed as highly tentative until it is robustly corroborated. Second, even if this evidence is valid, there are many other potential explanations for not observing an increase during lockdown. For example, the lockdown may have resulted in depressed reporting rates—telephonic reporting may have been difficult during lockdown with the partner always present (however, note, call data was also used in several of the studies cited above that did document an increase in GBV). Further careful research to resolve these questions would be extremely valuable.