WZB

Wissenschaftszentrum Berlin
für Sozialforschung

Kai Barron, Charles D.H. Parry, Debbie Bradshaw, Rob Dorrington, Pam Groenewald, Ria Laubscher, and Richard Matzopoulos

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

Discussion Paper

SP II 2022–301

February 2022

Research Area
Markets and Choice

Research Unit
Economics of Change

Wissenschaftszentrum Berlin für Sozialforschung gGmbH
Reichpietschufer 50
10785 Berlin
Germany
www.wzb.eu

Affiliation of the authors:

Kai Barron, WZB (kai.barron@wzb.eu)

Charles D.H. Parry, South African Medical Research Council, Stellenbosch University

Debbie Bradshaw, South African Medical Research Council, University of Cape Town

Rob Dorrington, University of Cape Town

Pam Groenewald, South African Medical Research Council

Ria Laubscher, South African Medical Research Council

Richard Matzopoulos, South African Medical Research Council, University of Cape Town

Abstract

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

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

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

JEL classification: I18, I12, K42

---

* The authors would like to thank Johannes Abeler, Peter Barron, Tilman Fries, Elisabeth Grewenig, Heather Jacklin, Simas Kucinskas, Jan Marcus, Melissa Newham, Paul Rodriguez Lesmes, Julia Schmieder, Marica Valente, Corne van Walbeek, Anna Wilke 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, reflecting the interdisciplinary nature of this research, which straddles economics and the health sciences. Author contributions: KB and RM conceptualized the study. KB performed the statistical analysis and wrote the draft manuscript. RD, PG, RL and DB contributed to the collection of the mortality data. All authors contributed to the revision of the manuscript. Barron gratefully acknowledges financial support from the German Science Foundation via CRC TRR 190 (project number 280092119).

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, 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 individuals in society other than the person consuming alcohol. These externalities may be imposed either directly (as in the case of interpersonal violence) or indirectly (as in the case of public health insurance).[1] Consequently, questions regarding the morality and correct societal regulation of alcohol have been debated in societies around the world for centuries, with virtually all modern and past societies placing legal and religious constraints on alcohol consumption (Phillips, 2014). It is crucial, therefore, to accumulate robust empirical evidence that allows us to construct a clear picture of the true influence of alcohol 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.[3] Research by health scientists has identified alcohol consumption as a major risk factor for injury-related deaths globally (see, e.g., Rehm et al., 2003, 2017).[4] This suggests that

[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).

[3] 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.

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

reducing alcohol consumption in an entire country 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 provided by a natural experiment in the form of a sudden alcohol sales ban to study the causal impact of alcohol on mortality due to unnatural causes at a societal level.[5] We also present evidence on one key potential mechanism behind this relationship by evaluating the impact that the ban had on aggressive behavior in society (e.g., homicides, assaults, reported rape cases). This is valuable as it provides policy-makers with robust evidence about the harm that alcohol consumption generates in society and informs our understanding of whether reducing alcohol consumption is an effective way to save lives and alleviate interpersonal violence. It therefore contributes evidence towards the larger discussion regarding the aggregate costs and benefits of alcohol consumption for society.

To do this, our main analysis uses daily mortality data from South Africa for the period between January 1, 2017 and December 31, 2021. 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 important because we show that there are highly regular, systematic patterns in the number of unnatural deaths observed according to the day-of-the-week and day-of-the-month. Using a difference-in-difference empirical strategy, we evaluate the change in mortality due to unnatural causes that occurred as a result of the alcohol sales ban implemented 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 in the middle of the so-called "Level 3" COVID-19 policy response period during which time other policies and regulations were largely held constant.[6] One important exception to this is that the alcohol ban was implemented together with a curfew between 9PM and 4AM. However, we consider this curfew as having had a largely secondary influence on mortality for several reasons. First, we show

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

[6]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. It is worth noting that there were 5 Levels of policy response, such that Level 3 involved intermediate restrictions that were far lighter than the Level 5 restrictions.

that when the alcohol ban was lifted, but the curfew remained in place, unnatural mortality jumped back up to pre-2020 levels, indicating that the curfew alone did not reduce unnatural mortality. Second, we conduct a sensitivity analysis that makes use of a one hour reduction in the length of the curfew (i.e. moving the start time from 9PM to 10PM) which occurred in the middle of the relevant period. This shift on the intensive margin during the alcohol ban had no effect on unnatural mortality.

Our main result is that the alcohol ban reduced the number of people dying from unnatural causes in South Africa by at least 120 per week. This reflects the lowest estimate of the effect size that we obtain across a range of different empirical specifications. It represents a substantial reduction in mortality due to unnatural causes, since it implies a 14% reduction in all unnatural deaths in the country when compared to the average level during the five weeks immediately preceding the July Alcohol Ban. In the analysis below, we show that this reduction in mortality is almost entirely confined to men. This is not entirely surprising since, 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).[7] This pattern is not unique to South Africa. For example, Gawryszewski and Rodrigues (2006) describe the gender distribution of injury-related mortality in Brazil in 2003 and show that 84.3% of the people that died from injury-related causes (e.g., homicides, suicides, transport-related deaths) were men. Furthermore, as in many countries around the world (e.g., Brazil, Russia), in South Africa men are far more likely than women to engage in heavy drinking (WHO, 2019). We find that the ban on alcohol reduced the number of men dying due to unnatural causes by at least 120 per week, but find no evidence that it had a statistically significant effect on the mortality of women in the population as a whole. (Importantly, this does not imply that the absence of alcohol had no impact on other outcomes for women, such as gender-based violence, which often does not result in death.) Further, we provide evidence that approximately half of the observed reduction in mortality is found amongst young men aged 15-34.

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

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

and the assumptions underlying our ability to use it to identify the impact of alcohol on mortality (Appendix Section C.1).[8]

To better understand what is driving this drop in mortality due to unnatural causes, we augment our main results by conducting an additional analysis that examines police crime data on homicides, assaults, and reported rape cases during the period of interest. We document evidence suggesting that the alcohol ban resulted in a sharp drop in all of these outcomes, with at least 77 fewer homicides, 790 fewer assaults and 105 fewer rape cases reported per week during the alcohol ban period in comparison to the preceding five weeks. These constitute a drop in each outcome of 21%, 33% and 19% respectively. To illustrate the dynamic effects of the alcohol ban over time, we also report the results from event study analyses considering the evolution in unnatural mortality and also the three violent crime outcomes.

The general pattern that emerges is that the effect of the ban appears to have been strongest in the first few weeks. A speculative possible reason for this is that black market trade and production started to reduce the effectiveness of the ban. Overall, the results provide compelling evidence that alcohol is causally responsible for inducing aggressive behavior in society at a significant scale, resulting in substantial harm.

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

Second, this paper provides a valuable contribution to the collective global effort to better understand the relationship between alcohol, violence and injury-related outcomes more generally. This is an extremely important endeavour, since alcohol is estimated to have been responsible for 5.3% of all deaths worldwide in 2016 (3 million), with 0.9 million of those being injury-related deaths (WHO, 2019; Shield et al., 2020). In addition, alcohol is implicated in many more incidents of violence and non-lethal injuries and was the leading risk factor for premature death in individuals 15-49 years old worldwide in 2016 (Griswold et al., 2018).

While it is essential to acknowledge that any evidence collected within a single country relates

[8] 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.

to behavior that occurs within a particular societal context, collecting rigorous evidence across a range of contexts makes it possible to aggregate the evidence and identify which alcohol-driven relationships occur systematically across contexts, and which are context-specific (i.e., mediated by an interaction between alcohol consumption and other societal factors). The evidence presented here is particularly useful for this exercise since South Africa is part of a class of countries: (i) for which alcohol is estimated to be responsible for a large number of injury-related deaths, and (ii) that make up a large part of the world's population, but tend to be underrepresented as the focus of academic research relative to more developed nations due partially to constraints on the availability of highly detailed data.

In South Africa a minority of individuals drink (31% of individuals aged 15 years and older, 43.2% of men and 19.4% of women). However, those who do drink, tend to drink heavily: six out of every ten drinkers (59%) engage in heavy episodic drinking (HED), which corresponds to 18.3% of the population over 15 years of age, or 30.6% of men and 6.5% of women (WHO, 2019).[9] South Africa is also a country that suffers from a relatively 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).[10]

Therefore, this evidence on the impact of alcohol from South Africa provides an informative benchmark for countries characterised by high levels of injury-related deaths, a sizable fraction of the population that drinks excessively, a strong asymmetry in drinking patterns between men and women, and high levels of poverty and inequality. This set of characteristics is reflective of several countries in Eastern Europe and South America, such as Brazil (where 32.6% of men and 6.9% of women were HEDs in 2016, which is nearly identical to the pattern in South Africa) and Russia (where 48.4% of men and 24.2% of women were HEDs in 2016). Both countries also share many other structural similarities with South Africa that could interact with alcohol consumption

[9]The average absolute amount of alcohol consumed per day by those who drink is 64.6 g or 5.4 standard drinks in South Africa. However, it is important to exercise caution in interpreting these consumption amounts that condition on being a drinker, especially in cross-country comparisons, since there is substantial heterogeneity across countries in the fraction of the population that drinks. Therefore, a country with a larger fraction of social drinkers will tend to have a lower conditional consumption amount, even if heavy drinking is present in society. For example, in Germany and France, the average daily consumption of alcohol conditional on drinking is 36.5 g and 36.1 g respectively. However, in these two countries, 79.4% and 73.3% of the population are classified as drinkers. This means that the individuals in the right-tail of the alcohol consumption distribution in these two countries are likely to drink substantially more than than 36.5 g or 36.1 g per day. In Germany, 34.2% of the population over 15 years of age are classified as heavy episodic drinkers (HEDs), while in France, this percentage is 31.2%, implying both countries have substantially more HEDs than the 18.3% in South Africa (WHO, 2019).

[10]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.

in influencing behavior, such as suffering from social issues including poverty, inequality and high levels of violence.[11] Both countries are also characterized by a strong gender asymmetry in unnatural deaths, like South Africa (see, e.g., Starodubov et al., 2018; Gawryszewski and Rodrigues, 2006).

Third, our results provide a society-level demonstration of the way in which alcohol can act as a catalyst in inducing violence. While contextual factors in different countries may shape the way in which excessive alcohol consumption manifests in behavior, the growing body of evidence of a deep link between excessive alcohol consumption and aggressive behavior is important for all countries. The evidence discussed in this paper complements a large body of existing work showing that there is a strong association between alcohol consumption and aggressive behavior across a range of domains (for a review of this evidence, see Tomlinson et al., 2016). More specifically, our causal evidence contributes to the existing literature that documents a strong association between homicides and alcohol, finding that a high fraction of homicide offenders (and victims) were under the influence of alcohol at the time of the offence (for systematic reviews, see Darke, 2010; Kuhns et al., 2011, 2014). For example, Kuhns et al. (2011) reports that from over 70 000 toxicology test results from 13 countries (predominantly from the United States), 48% of homicide victims tested positive for alcohol, while Kuhns et al. (2014) reports that from almost 30 000 homicide offenders across 9 countries (mostly Australia, the United States and Europe), 48% were reported to be under the influence of alcohol.[12]

The presence of a link between alcohol and aggression has also been demonstrated in the laboratory by experimentally varying the alcohol present in an individuals' system as they complete a task that involves administering electric shocks to a fictitious opponent (Duke et al., 2011). The authors found a strong link between the dosage of alcohol in an individual's system and the aggression they showed in administering the electric shocks. Taken together, this body of work on the underlying psychological mechanism relating alcohol to aggression indicates that it is also important for countries that differ substantially from South Africa, such as high-income countries, to pay attention to the evidence in this paper showing how large an impact alcohol can have on behavior at a societal level, since: (i) heavy episodic drinking is substantially more prevalent in many higher

[11] In line with this, the WHO (2019) provides estimates for the age-standardized alcohol-attributable injury death rate in countries around the world (p. 77). These estimates are high for the majority of countries in Sub-Saharan Africa, South America, and Eastern Europe (typically above 10.8 per 100 000 people), with South Africa in the interior of the range of estimates. Similarly, the prevalence of heavy episodic drinking amongst current drinkers is high in the same regions (Sub-Saharan Africa, South America, and Eastern Europe), with South Africa fairly typical in this (p. 48).

[12] In recent work that explores the causal role of alcohol in victimization more broadly, Bindler et al. (2021) show that obtaining increased access to alcohol at ages 16 and 18 in the Netherlands results in sharp discontinuous increases in the risk of being a crime victim.

income countries, such as the USA, UK, Germany and France, than it is in South Africa, (ii) in these countries, the aggression triggered by excessive alcohol consumption may manifest in harmful behaviors that result in harder-to-detect outcomes, such as sexual and gender-based violence, child abuse and emotional abuse, and (iii) it is not common to observe a societal level source of exogenous variation in alcohol consumption, which makes it challenging to evaluate the effect of alcohol in society even when the data on the relevant outcomes is available.[13] Therefore, in summary, the evidence discussed in this paper is highly informative for local policy discussions, but also helps to advance the wider global effort of constructing a clear evidence-based understanding of the relationship between alcohol, aggressive behaviour and harmful outcomes.

This paper contributes to several strands of the literature. It relates most closely to the body of work that studies the short-run relationship between alcohol and harmful behavior, such as violence, suicide and crime (Carpenter, 2004, 2005a, 2007; Biderman et al., 2010; Rossow and Norström, 2012; Wilkinson et al., 2016), road traffic collisions (Baughman et al., 2001; Chikritzhs and Stockwell, 2006), risky sexual behavior (Carpenter, 2005b), and outcomes such as mortality and morbidity (Matzopoulos et al., 2006; Carpenter and Dobkin, 2009; Marcus and Siedler, 2015; Carpenter and Dobkin, 2017; Sanchez-Ramirez and Voaklander, 2018; Nakaguma and Restrepo, 2018). There are two main empirical approaches that have been employed in this literature to provide this type of causal evidence: (i) using changes in underage drunk driving laws or minimum drinking age laws (see, e.g., Wagenaar and Toomey, 2002; Carpenter and Dobkin, 2009, 2011, 2017), or (ii) using changes in the alcohol trading hour regulations (see, e.g., Biderman et al., 2010; Green et al., 2014; Marcus and Siedler, 2015; Wilkinson et al., 2016; Sanchez-Ramirez and Voaklander, 2018).[14] 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

[13]One example of a recent paper that does manage to find an interesting source of exogenous variation is the work by Ivandić et al. (2021) that exploits the timing of football games to examine the effect of alcohol consumption on domestic abuse in the Greater Manchester area in the United Kingdom, finding that alcohol consumption results in an aggregate increase in domestic abuse.

[14]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.

and morbidity more broadly (see, e.g., Bates, 1918; Emerson, 1932; Warburton et al., 1932, for some early contributions). This work emanates from the contentious social debates of the late nineteenth and early twentieth century in many Western societies, including the United States, about whether allowing alcohol consumption is good for society (Blocker, 2006). A set of more recent studies have tried to estimate the effect that state and federal prohibition statutes enacted in the United States during the early decades of the twentieth century had on mortality and morbidity (Miron and Zwiebel, 1991; Miron, 1999; Dills and Miron, 2004; Owens, 2011; Livingston, 2016; Law and Marks, 2020). This literature portrays a highly ambiguous picture regarding the health and safety impacts of alcohol prohibition. However, in a recent contribution, Law and Marks (2020) argue that they overcome several empirical challenges faced by the prior work and conclude that early prohibition laws enacted between 1900 and 1920 significantly reduced mortality rates in the United States.[15]

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

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

This paper also relates to the small body of literature that studies the impact of curfews on crime, which documents mixed results.[16] Last, our results add to the recent work studying the impact of COVID-19 policy responses on crime, violence, morbidity and mortality in South Africa and other countries (e.g. Poblete-Cazenave, 2020; Leslie and Wilson, 2020; Bullinger et al., 2021; Nivette et al., 2021; Asik and Ozen, 2021; Navsaria et al., 2021; Moultrie et al., 2021; Chu et al., 2022).[17]

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

2 Data and the Policy Landscape

2.1 Policy Timeline

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

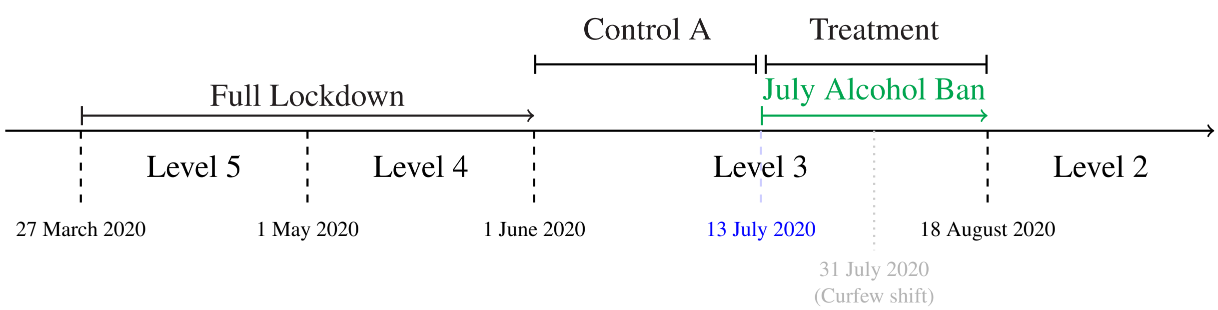
[16] Kline (2012) shows that the introduction of a juvenile curfew in Dallas reduced the arrest rate of individuals below the statutory curfew age for both violent and property crimes. In contrast, Carr and Doleac (2018) use variation in the timing of the onset curfews in Washington DC to provide evidence 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.

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

extreme policy bundle (Level 5) to the least extreme (Level 1). The alcohol ban was implemented in the middle of the Level 3 period.

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

Figure 1: 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 on May 1, 2020, but for much of the general population, this still involved a continuation of the state of lockdown. On June 1, 2020, the country entered Level 3, which is the key period of interest for this paper. Level 3 involved a further relaxation of policy restrictions on daily life. The key restrictions in place during Level 3 were the following: (i) off-premises and e-commerce alcohol sales were only permitted from Monday to Thursday between 9AM and 5PM,[18] (ii) there was no official curfew, but individuals were only permitted to leave their house when they had a valid reason (e.g. exercise between 6AM and 6PM, going to work), (iii) gathering in groups was still forbidden, with some exemptions for work or specific religious events.[19] In practise, these restrictions were not easy to regulate and enforce, particularly in areas

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

[19]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).

with informal housing and high-density living conditions.

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 many medical professionals in South Africa held the view that alcohol is responsible for a large proportion of trauma admissions and therefore advocated in favor of temporarily banning alcohol as a way to free up hospital resources for potential COVID-19 patients.[20] Along with this alcohol ban, a curfew from 9PM to 4AM was introduced. Below we provide evidence showing that the curfew is unlikely to have been a key determinant of any changes observed in mortality due to unnatural causes during this period.

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

2.2 Data

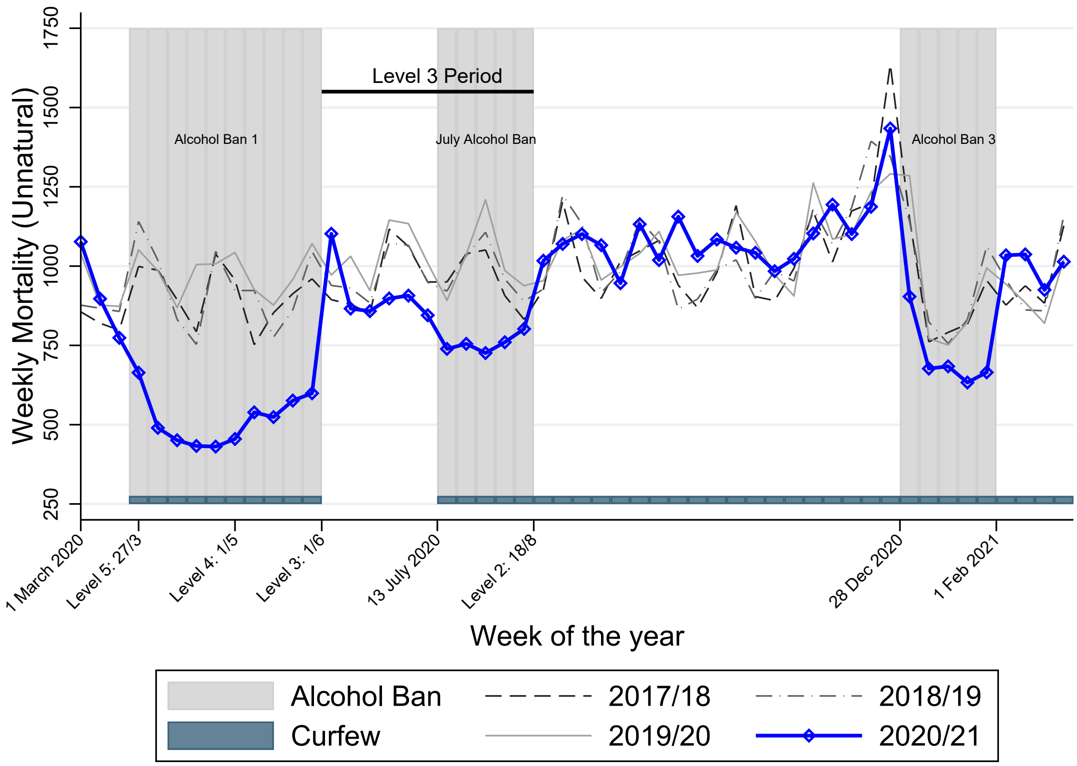
This paper uses outcome data from two sources. First, in our main analysis, we use national daily mortality data from January 1, 2017 to September 13, 2020. This dataset is collected by the Department of Home Affairs and curated by the South African Medical Research Council. It contains a record of all deaths of persons with a valid South African identity document (Dorrington et al., 2020). We focus on mortality due to *unnatural* causes. This includes deaths precipitated by road traffic injuries, interpersonal violence, and suicide, but excludes all deaths due to natural causes, such as illness (e.g., COVID-19). Unnatural deaths, therefore, are often caused by risky behavior with 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.

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

Figure 2 provides a descriptive illustration of our unnatural mortality data. The bold blue line

[20]This view held by medical professionals was strengthened by the fact that there were reports emerging that the number of alcohol-related trauma cases in hospitals had spiked during the first week of June 2020 after the move from Level 4 to Level 3. Since there was an initial alcohol ban during Level 5 and Level 4, the rapid increase in trauma cases during that single week after the first alcohol ban made it salient that banning alcohol represented a potential way for policy makers to free up space in hospitals. 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), van Walbeek and Chelwa (2021) and Matzopoulos et al. (2020).

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



denotes weekly mortality levels due to unnatural causes between March 2020 and February 2021, while the grey lines reflect the same measure for each of the previous three years. The shaded vertical bars reflect the three periods during which alcohol bans were implemented in 2020/21, with the July Alcohol Ban the second of these three alcohol bans. The thick teal line at the bottom of the figure indicates when a curfew was in place (although, the length of the curfew varied: see Table 15 for details). 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 to a level slightly below that observed in previous years. The figure also provides suggestive evidence that the introduction of the July Alcohol Ban then reduced the rate of unnatural mortality again. (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 figure suggests that during the first week following each of the alcohol bans, there was a sharp rise in unnatural mortality. For example, this appears to have occurred at the end of the Level 3 period. This occurred despite the curfew remaining in place when the alcohol ban was rescinded. This increase in mortality at the beginning of Level 2, and then persistent higher level despite the curfew, suggests that the curfew was not a crucial reason for the lower mortality levels observed during the second half of Level 3.

Our main analysis uses three versions of this unnatural mortality data. The first contains a record of daily mortality levels in the country as a whole.[21] 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 (we also report the corresponding results for other age groups in the Appendices). The main reason for examining this sub-population is that young adults are typically viewed as being the group that are 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 number observed during the period that immediately preceded it (in the first half of Level 3).

In doing this, it is important to isolate the effect of the alcohol ban from unrelated weekly and seasonal changes in behavior. Using detailed mortality data from the preceding years (i.e.

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

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

Our main specification, therefore, removes weekly, monthly, seasonal and yearly time trends that may play a role, allowing us to focus on the difference in mortality observed within the Level 3 period in 2020 before and after the implementation of the alcohol ban.[22] We therefore estimate the following model using Ordinary Least Squares:

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

where $M_{y,t,g}$ refers to the number of daily unnatural deaths in year y on day-of-the-year t in group g (i.e. for a specific gender or age group) and $\boldsymbol{\lambda}_{y,t}$ is a vector of time-related fixed effects that vary across specifications.[23] To control for seasonal mortality, we include two calendar period indicator variables: $L3_{y,t}$, which corresponds to the Level 3 calendar period, and $T_{y,t}$, which corresponds to the July Alcohol Ban calendar period. Importantly, both these variables take a value

[22] 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.

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

of 1 for the relevant calendar periods in *all* years in our data (i.e. in the years between 2017 and 2020) in order to account for seasonal effects. We then interact each of these two variables with an indicator variable that takes a value of 1 if the year is 2020. The first interaction variable, $L3_{y,t} \times Y_{2020}$, is crucial for our identification as it controls for the influence of the basket of Level 3 policies that were in place throughout the Level 3 period, including the July Alcohol Ban period. Controlling for the baseline level of mortality during the Level 3 period allows us to use the second interaction variable, $T_{y,t} \times Y_{2020}$, to examine the shift in unnatural mortality within the Level 3 period that occurred when the July Alcohol Ban was introduced.

The interpretation of the coefficients is as follows: α_1 reports the average difference in unnatural mortality between the first half of the Level 3 calendar period and the rest of the year in pre-2020 years, while α_2 reports the average difference in unnatural mortality between the first and second half of the Level 3 calendar period in pre-2020 years. The corresponding interaction coefficients for 2020, α_3 and β, report the change in the corresponding objects for 2020 relative to pre-2020 years: α_3 is the additional difference in unnatural mortality between the first half of the Level 3 period and the rest of the year in 2020 relative to pre-2020 years. Our main coefficient of interest, β, provides an estimate of the impact of the alcohol ban on mortality by estimating the shift in unnatural mortality that occurred when the alcohol ban was introduced. Specifically, β reports the change in the difference between the first and second halves of the Level 3 period in 2020, relative to pre-2020 years.

4 Results

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

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

When interpreting these results, there are two additional important considerations to keep in mind. First, it is also worth noting that there is a large estimated relationship between weekends and mortality. Table 5 in the Appendices reports the coefficients for the full set of weekend controls. These results show that: (i) substantially more individuals die from unnatural causes on

Saturdays and Sundays in comparison to other days of the week, (ii) this weekend effect is even more pronounced on the first and last weekend of the month, and (iii) these weekend effects were dampened during 2020 (as can also be seen in Figure 15 in the appendices). However, controlling for this weekend effect does not affect the estimated impact of the alcohol ban much. Second, Figure 2 showed that there was a spike in unnatural mortality during the first week of the Level 3 period (i.e., the first week of June 2020). This week forms part of our control period and since this one-week spike in unnatural mortality may have been a reaction to the end of the previous alcohol ban, it could be argued that it is appropriate to also consider an empirical specification omits this week from the control period. For this reason, we also conduct this exercise, replicating Table 1, but essentially reducing the Level 3 period by one week in our estimation by omitting the first week of June 2020 from the Level 3 period variable. These results are reported in Table 6 in the Appendices. As expected, this slightly reduces the magnitude of our estimate for β, with these results indicating that the reduction in unnatural mortality due to the alcohol ban was 17.96 deaths per day (95% CI: 7.60–28.33). We view it as reassuring that the results are not very sensitive to the inclusion or exclusion of this week and also yield highly consistent estimates across the range of different exercises we conduct (discussed below).

4.2 Heterogeneity by gender

Next, we consider heterogeneity by gender. There are two reasons for this. First, unnatural mortality levels of men and women are very different, with approximately 3.5 men dying from unnatural causes for every 1 woman (see, e.g., Figure 16 in the 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 many countries, including South Africa. For example, the WHO (2019) reports that heavy episodic drinking was five times higher amongst men in comparison to women in South Africa in 2016 (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 reports the estimated impact of the alcohol ban on the unnatural mortality of men and women. For men, the pattern is similar to that observed in the population as a whole, with the estimates indicating that the alcohol ban reduced mortality by approximately 21 deaths per day—our preferred specification in column (1c) reports a reduction of 21.43 (95% CI: 12.13–30.74). For

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

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

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

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

4.3 Focusing on younger adults

Young adults comprise a group that is of particular interest when studying the impact of alcohol on short-run outcomes. The reasons for this is that they are typically more likely to engage in risky behavior (e.g. risky drinking). We therefore estimate the impact of the alcohol ban on the sub-population of younger adults between the ages of 15 and 34 years. Table 3 reports these results. We find that the alcohol ban reduced mortality amongst men in this age-group by approximately 12 deaths per day, with an estimated reduction of 11.78 (95% CI: 5.49–18.07) in column (1c), and may have had a small impact on the mortality of younger women. An important implication of these results is that the reduction in mortality observed for men of all ages does not seem to be

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

	Men			Women		
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
Level 3 Period = 1	5.86	2.68	2.03	1.02	0.32	-0.28
(1/6-17/8)	(4.41)	(2.36)	(1.90)	(0.90)	(0.69)	(0.61)
Alcohol Ban Period = 1	-1.99	-0.44	0.46	-0.19	0.16	0.44
(13/7-17/8)	(5.96)	(2.83)	(2.39)	(1.18)	(0.88)	(0.81)
Level 3 Period x Year=2020	-8.24	0.97	0.40	-5.22***	-3.26***	-2.18*
	(5.30)	(3.71)	(3.77)	(1.23)	(1.08)	(1.19)
Alcohol Ban Period x Year=2020	-20.62***	-21.07***	-21.43***	-0.31	-0.48	-0.55
	(7.26)	(4.71)	(4.74)	(1.71)	(1.41)	(1.43)
Constant	106.54***	87.90***	100.89***	30.42***	27.24***	29.87***
	(1.31)	(0.94)	(3.58)	(0.31)	(0.27)	(1.34)
Weekend Controls		Y	Y		Y	Y
Day of Week FEs			Y			Y
Day of Month FEs			Y			Y
Year FEs			Y			Y
Observations	1460	1460	1460	1460	1460	1460
Adjusted R^2	0.008	0.535	0.606	0.009	0.306	0.374

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

completely due to a reduction in risky behavior by young adults. The 12 lives of younger men saved per day by the alcohol ban is only slightly over half of the 21 male lives of all ages saved per day.[24] When we omit the first week of Level 3 from our control period (see Table 8 in the Appendices), we obtain an estimated reduction in the mortality of young men of 10.24 (95% CI: 3.90–16.58), and no significant impact for young women.

To investigate the relationship between age and the impact of the alcohol ban further, in Figure 6 in the Appendices, we compare the age distribution of unnatural mortality during the 5 weeks preceding the alcohol ban with the age distribution during the 5 weeks of the alcohol ban. This figure suggests that the majority of the decrease in unnatural mortality due to the alcohol ban was

[24]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.

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	4.20	2.17	1.81	-0.16	-0.55	-0.75*
(1/6-17/8)	(2.88)	(1.52)	(1.32)	(0.53)	(0.42)	(0.40)
Alcohol Ban Period = 1	-2.68	-1.73	-1.36	0.38	0.57	0.72
(13/7-17/8)	(3.85)	(1.78)	(1.64)	(0.71)	(0.54)	(0.50)
Level 3 Period x Year=2020	-6.90**	-0.84	-0.30	-1.18	-0.02	0.26
	(3.50)	(2.30)	(2.48)	(0.73)	(0.67)	(0.74)
Alcohol Ban Period x Year=2020	-11.33**	-11.56***	-11.78***	-1.48	-1.59*	-1.64*
	(4.65)	(2.94)	(3.21)	(0.99)	(0.89)	(0.91)
Constant	53.16***	40.89***	46.85***	11.56***	9.79***	11.56***
	(0.85)	(0.58)	(2.09)	(0.17)	(0.14)	(0.67)
Weekend Controls		Y	Y		Y	Y
Day of Week FEs			Y			Y
Day of Month FEs			Y			Y
Year FEs			Y			Y
Observations	1460	1460	1460	1460	1460	1460
Adjusted R^2	0.008	0.551	0.611	0.004	0.320	0.367

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

observed for individuals between 18 and 35 years of age. However, it also appears to show that there was also a decrease in unnatural mortality for individuals over 35. Tables 9, 10 and 11 in the Appendices provide further evidence by replicating the empirical analysis used in Table 3 for other age groups. These results are largely in line with the suggestive evidence from Figure 6: (i) for individuals aged 14 or younger, we find no effect of the alcohol ban on mortality, (ii) for individuals between 35 and 54 years, we estimate that the alcohol ban resulted in 6.7 fewer male deaths per day, with a possible smaller reduction in the mortality of women by 1.3 deaths per day, (iii) for individuals over 55 years, we estimate that there were 2.5 fewer male deaths per day, and 1.6 additional female deaths per day during the alcohol ban period.[25] Overall, the results indicate that most of the effect of the alcohol ban on unnatural mortality was concentrated amongst younger male adults, with a smaller, but still sizable, impact also observed amongst middle aged men.

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

4.4 Robustness exercises

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

The first two exercises address concerns regarding the suitability of the natural experiment for providing causal evidence on the impact of reducing alcohol consumption (see Section C.1). To do this, we show that the primary candidate confounding factors were unlikely to have contributed to the observed reduction in unnatural mortality. Aside from the alcohol ban, the two main sources of behavioral change in society during the period we are studying were 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 evaluate the possibility that the main contemporaneous regulatory change, namely the introduction of a curfew, influenced unnatural mortality. To do this, we make two observations. First, we note that when the July Alcohol Ban ended, the curfew remained in place and Figure 2 shows that unnatural mortality increased sharply at this point in time and remained at pre-2020 levels despite the ongoing curfew. Second, we estimate the impact of a one hour reduction in the curfew length which occurred in the middle of the July Alcohol Ban period. We show that it did not have a statistically significant impact on unnatural mortality. These observations on both the extensive and intensive margin support our assessment that the curfew was unlikely to have been a key factor in reducing mortality during the July Alcohol Ban.

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

Second, we examine whether our results are sensitive to the precise choice of time window used for our estimation. To do this, we conduct an additional robustness exercise where we vary the length of the treatment-control time window around the introduction of the alcohol ban used in our analysis. Instead of including a indicator variable for the entire Level 3 period, we consider time windows of between 2 weeks and 5 weeks in length. We find that the estimated impact of the alcohol ban remains fairly stable when considering windows of 5 weeks, 4 weeks and 3 weeks in length. The only exception to this is that when we use a very narrow window of only 2 weeks in

length, we no longer observe a significant coefficient estimate.[26]

Third, we consider alternative approaches to calculating the standard errors and drawing inference from our regressions. Specifically, we conduct a series of exercises that reproduce the results from Table 2 but relax the assumptions on the error structure to allow for serial correlation in the error term. The results from these exercises are discussed in detail in Section C.2.3 and report (i) standard errors estimated by clustering at the calendar-week level, (ii) standard errors calculated using the Newey-West (1987) variance estimator that allows for autocorrelation up to a pre-specified lag length, and (iii) p-values for the main coefficient of interest, calculated using the wild cluster bootstrap to correct for the small number of clusters when clustering at the year level (Cameron and Miller, 2015; Roodman et al., 2019). These exercises yield estimates that are all consistent with the findings reported above.

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

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

The point estimates from our preferred specification, which includes fixed effects, are very close to those in our main results.[27]

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

[26]In Section C.2.2, we discuss several potential explanations for this, including: (i) the possibility that there could have been a lag between the introduction of the alcohol sales ban and a substantial reduction in alcohol consumption as individuals might 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 week period afterwards does not. Given the very short time window being considered, this imbalance between the treatment and control period could provide an explanation for absence of a significant estimate if our weekend and fixed effect controls are not perfectly accounting for this imbalance. Our event study analyses in Sections 5 and 6 suggests that the second of these explanations is more plausible than the first, since they indicate that there was a drop in the outcomes we study immediately after the introduction of the alcohol ban.

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

5 Event study analysis

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

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

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

where $\boldsymbol{\lambda}_{y,t}$ is a vector of time-related fixed effects (same as in equation 1 above), $\boldsymbol{\mu}_{y,t}$ contains indicator variables for the Level 3 calendar period ($L3_{y,t}$) and the July Alcohol Ban calendar period ($T_{y,t}$) in all years (i.e., not only 2020), and $b^j_{y,t}$ is a treatment indicator that takes a value of one when the event of interest, namely the start [end] of the July Alcohol Ban, takes place $j \in [\underline{j}, \overline{j}]$ weeks away from $w(t)$, where $w(t)$ refers to the week in which day t occurs.

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

Following the standard event-study approach, the indicator variables at the endpoints of the effect window serve to bin together all observations that occur outside the effect window (see, e.g., Schmidheiny and Siegloch, 2019). Put simply, in our setting, all observations that occurred seven weeks or more before the July Alcohol Ban (including all observations from 2019 or earlier) are binned together and the variable $b^{\underline{j}}_{y,t} = b^{-7}_{y,t} = 1$ for these observations. Similarly, all observations that occurred six weeks or more after the July Alcohol Ban are binned together and $b^{\overline{j}}_{y,t} = b^{6}_{y,t} = 1$ for these observations. For the weeks within the eleven week effect window, $b^j_{y,t} = 1$ when an observation occurs in a week that is j weeks away from the onset of the July Alcohol Ban (with negative values denoting weeks before the ban, and positive values denoting weeks after the ban).[28] Importantly, since the transition into the July Alcohol Ban occurred between two weeks (i.e., between Sunday and Monday), we define $j = \{-7, ..., -1, 1, ..., 6\}$, with $j = -1$ denoting the last week before the ban and $j = 1$ the first week after the onset of the ban, so there is no period $j = 0$. In our analysis of the transition into the ban, the benchmark week is $j = -1$.[29]

The results from our event study analysis for the transition into the July Alcohol Ban are displayed in the left-hand panels of Figures 3 and 4.[30] Figure 3 reports the results corresponding to specification (*b) of our main regressions, including a range of weekend controls. Figure 4 corresponds to specification (*c), which additionally includes a full set of calendar fixed effects (see footnote 23 for details). In both figures, the top-left panel reports the results for men, while the

[28]This definition of j is obtained by normalizing the week counter index such that the event of interest occurs in week 0, such that $e = 0$.

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

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

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

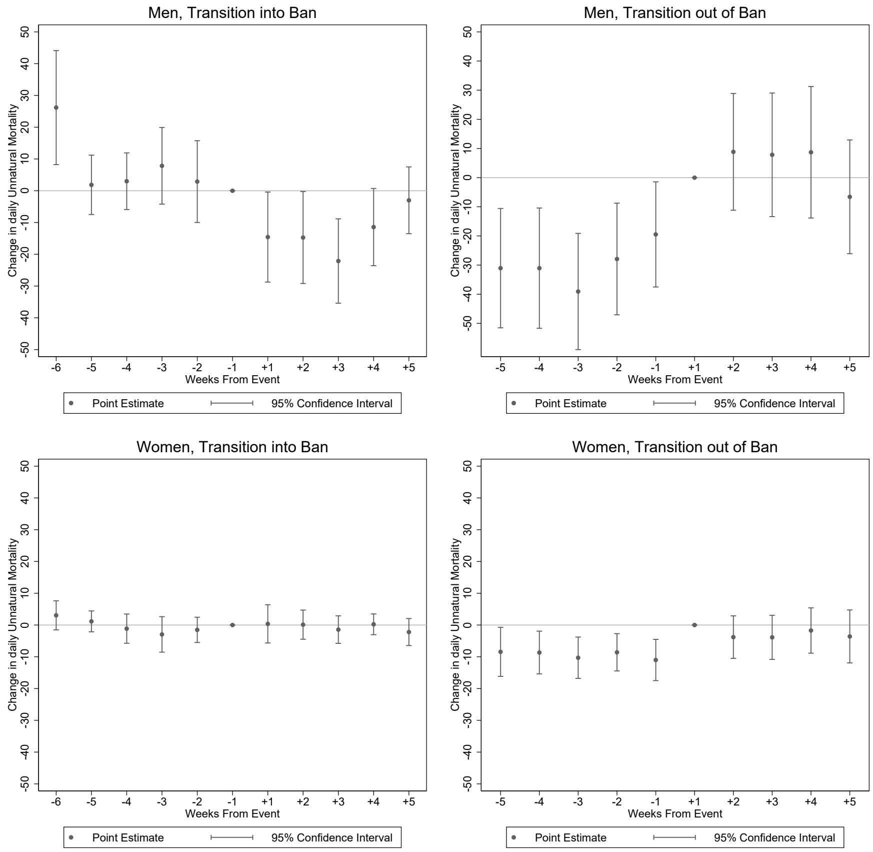
bottom-left panel reports the results for women.

These results for the transition into the alcohol ban reveal several insights. First, the high coefficient estimate for men in week $j = -6$ is consistent with the observation that there was a jump in unnatural mortality during the first week of June 2020 (recall that $j = -6$ is the first week after Alcohol Ban 1, which lasted over two months). This jump in unnatural mortality can also be seen clearly in the raw data in Figure 2. Second, for men, all the point estimates in weeks during the July Alcohol Ban (i.e., $j > 0$) in both specifications are negative.[31] Therefore, these results are consistent with the aggregate level results above that pool the weeks together. Third, the lower left-hand panels of both figures show that the coefficient estimates for women are almost completely flat and close to 0 (only β_{-2} in Figure 4 is statistically different from zero at the 10% level). This null result for women is highly informative as it shows that there was no change in the causes of unnatural mortality that affected women when the July Alcohol Ban was introduced. This implies that the results that we observe for men must be driven by some change in the causes of unnatural mortality that effect men differently from women. Since alcohol consumption patterns in South Africa are highly gendered, this provides further support for the idea that the reduction in alcohol consumption is the likely explanation for the effect we observe.

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

[31] It is worth noting that in specification (*c), but not specification (*b), the point estimate for β_{-2} also appears to be negative (although 0 still lies within the 95% CI). One potential reason for this negative point estimate two weeks prior to the July Alcohol Ban is that this week includes the first weekend of the month. If our calendar fixed effects do not perfectly account for all the variation in unnatural mortality due to payday effects, this could make the estimates for payday weekends more sensitive to the choice of specification. This is perhaps supported by the observation that for women in the lower-left panel of Figure 4 the only coefficient with a 95% confidence interval lying fully below zero is also week $j = -2$.

Figure 3: Event study: Dynamics of unnatural mortality [Specification (*b)]

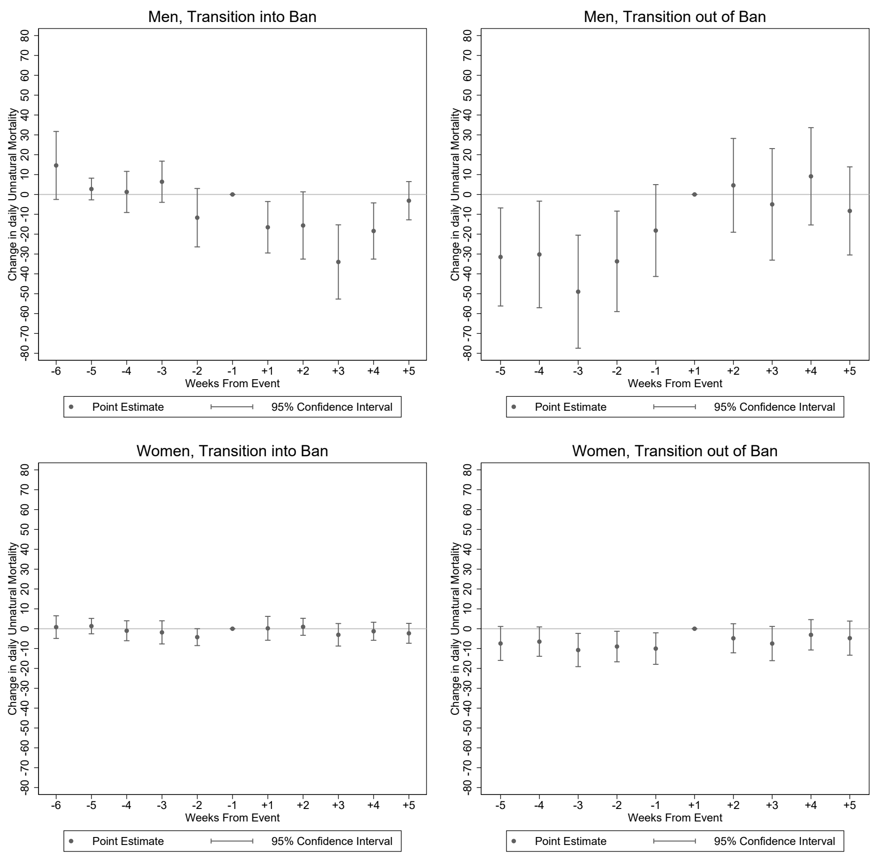


have also needed to be reversed at the same time as the alcohol ban ended to generate the down-up pattern in unnatural mortality that we observe. Second, the curfew that was initiated at the same time as the July Alcohol Ban remained in place when the alcohol ban was lifted, so the transition out provides a test of whether the curfew alone reduced unnatural mortality. If it did, then one would expect unnatural mortality to remain low when the alcohol ban was lifted and the curfew remained in place.

Examining the top right-hand panels of Figures 3 and 4 shows that for men the transition out of the July Alcohol Ban was associated with a step-up in unnatural mortality. These results are consistent with our main results, and also provide evidence that the curfew alone was not the main driver of the reduction in unnatural mortality observed during the July Alcohol Ban. Turning to the bottom-right panels of the two figures, we see that in contrast to the transition into the alcohol ban, the transition out was associated with a change in unnatural mortality for women. This is consistent with the idea that the change in unnatural mortality when transitioning out of the alcohol ban was

driven by both the re-availability of alcohol and also by the relaxation of other regulations that occurred when moving from Level 3 to Level 2. This is also consistent with the observation that the event-study coefficient estimates for men in Figures 3 and 4 are larger in magnitude (more negative) during the transition out in comparison to the transition in. Examining the raw data in Figure 2 shows this even more clearly.

Figure 4: Event study: Dynamics of unnatural mortality [Specification (*c)]



Overall, we interpret the results of this event-study analysis to be highly supportive of our main results. However, as a caveat, it is important not to place too much weight on the estimates for each individual week in our event-study analysis because: (i) the coefficient for each week is estimated from a small number of observations, and (ii) as we see from comparing Figures 3 and 4, there is some sensitivity in the estimates to the precise choice of empirical specification.

6 Crime: interpersonal violence as a mediator

One of the primary causes of injury-induced mortality is interpersonal violence. Therefore, to shed further light on one potential mechanism that could be driving our main results, in this section we examine the impact of the July Alcohol Ban on three outcomes related to interpersonal violence. To do this, we use data collected by the South African Police Service (SAPS). This data contains the daily number of reported contact crimes in South Africa during the three month period between June 1, 2020 and August 31, 2020 in three categories: homicides, assault with intent to inflict grievous bodily harm (GBH), and rape. Figure 7 in the Appendices displays the raw data. This figure reveals that each of these three outcomes followed a very similar week-by-week trajectory around the July Alcohol Ban to that observed for unnatural mortality. Specifically, in the very first week of June, which is also the first week following Alcohol Ban 1, we observe an elevated level of each of the three outcomes. Thereafter, in the next five weeks which precede the July Alcohol Ban, each of the outcomes stays relatively flat, with a slight increase around the change of month between June and July.[32] At the onset of the July Alcohol Ban, all three outcomes drop to a lower level for the duration of the ban, and when the ban is lifted, we again see a large jump upwards in all three outcomes. Therefore, the raw data suggests that the July Alcohol Ban affected each of these three outcomes similarly to how it affected injury-induced mortality.

To assess whether the pattern observed in the raw data reflects a statistically significant shift in each of the outcomes of interest at the onset of the July Alcohol Ban, we use a simplified version of our empirical strategy from above (simplified due to the reduced data availability).[33] Using only the eleven weeks and one day of the Level 3 period between June 1, 2020 and August 17, 2020, we estimate the following specification for each of our three contact crime outcomes and also for unnatural mortality:

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

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

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

where Z_t refers to outcome Z at time t, T_t is an indicator variable that takes a value of one during the July Alcohol Ban, and $\boldsymbol{\kappa}_t$ is a vector of indicator variables that control for the effect of weekends on each of the outcomes.[34]

Table 4: Impact of the alcohol ban on criminal offences and mortality

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

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

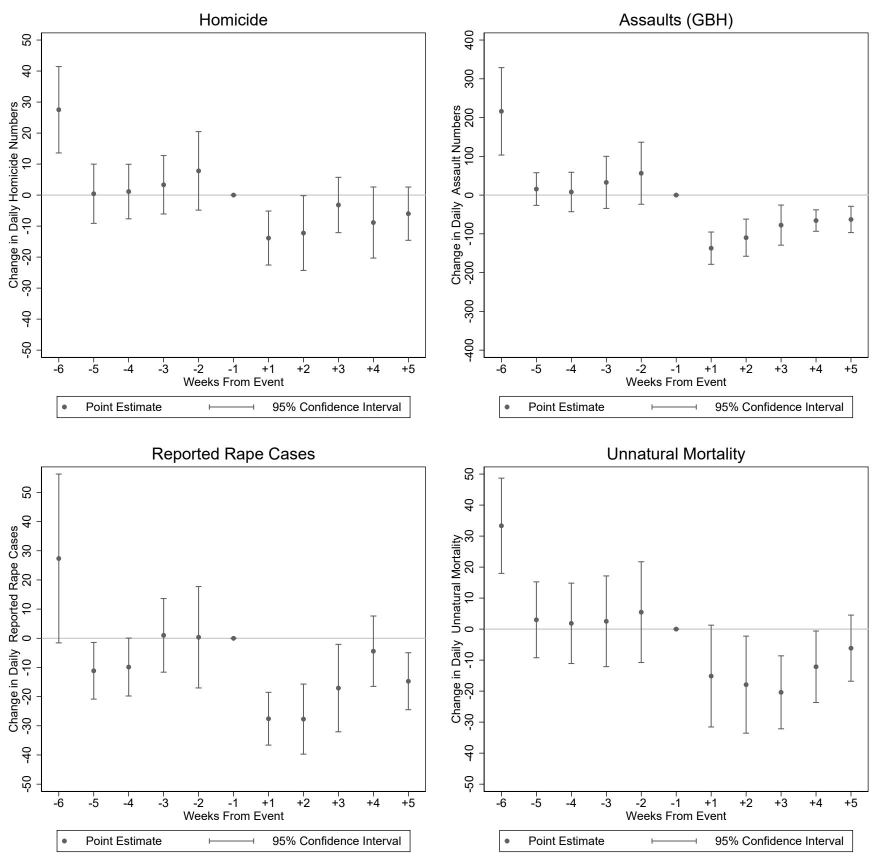
The results are reported in Table 4. They show that there was a statistically significant drop in all of our outcomes of interest when comparing the July Alcohol Ban period to the preceding six weeks. However, as discussed in the main results above, one important caveat to these results is that this six week comparison period includes the first week of June, which saw a jump in all of our outcomes of interest. Therefore, we also replicate the analysis reported in Table 4, excluding this first week of June. These replication results are reported in Table 13 in the Appendices and also show a statistically significant drop in all four outcomes, but of a slightly smaller magnitude. The first two columns of Tables 4 and 13, labelled (H*), estimate that the number of homicides was reduced by 11 [15] per day, depending on the specification, during the July Alcohol Ban period in comparison to the five [six] week preceding period. This reduction represents a substantial fraction of the 54 [58] daily homicides that occurred on average during the five [six] weeks leading up to the July Alcohol Ban. It also suggests that a large proportion of the 18 [22] fewer daily unnatural

[34] In specification (*2) in Table 4, $\boldsymbol{\kappa}_t$ contains three indicator variables, *Weekend Day*, which takes a value of one for Saturdays and Sundays, *First Weekend of Month* and *Last Weekend of Month*, which indicate whether it is the first or last weekend in the month. In specification (*1), $\boldsymbol{\kappa}_t$ is an empty vector.

deaths during the July Alcohol Ban period can be attributed to this reduction in interpersonal violence.

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

Figure 5: Event study: Dynamics of crime and unnatural mortality



To illustrate the dynamic effects in each of these four outcomes around the onset of the July Alcohol Ban, we adjust the specification in equation 5 by replacing T_t with indicator variables for each of the eleven weeks in the Level 3 period. This allows us to conduct a simple version of the

event-study analysis that we used above.[35] Figure 5 displays the coefficients from this analysis for each of the four outcomes. Overall, the figure shows a relatively sharp drop for all four outcomes at the onset of the July Alcohol Ban, providing support for our main findings.

7 Concluding discussion

In this paper we have documented evidence that a five-week-long nationwide ban on the sale and transport of alcohol resulted in a reduction of at least 14% of all unnatural deaths during that period. This is a large and meaningful number of lives saved. We have also shown that the alcohol ban lead to a sharp drop in violent crimes, suggesting that the relationship between alcohol and aggressive behavior is one of the key mechanisms driving our mortality results. Our findings provide unique causal evidence on the impact that a short-term *absence* of alcohol can have in a society; or rather, perhaps much more importantly, they provide a clear illustration of the impact that the *presence* of alcohol has on society every day. They demonstrate that alcohol can substantially increase the amount of behavior-induced harm observed in the population. These empirical results, therefore, support the predictions of earlier modeling studies that estimate that alcohol consumption places a heavy morbidity and 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. They illuminate the substantial benefits to society that can be achieved by carefully implementing policies that will successfully curb alcohol consumption in the long-run—policies other than a complete prohibition on alcohol sales may well be more effective avenues for pursuing this objective.[36]

Second, our estimates of the impact of the alcohol sales ban likely constitute a lower bound

[35] Note, in contrast to the specification used in our event study in Section 5, here we only use data from the eleven weeks, so there are no binned categories at the end points. We, therefore, include the indicators for weeks $j = -6$ to $j = -2$, and $j = 1$ to $j = 5$, with week $j = -1$ serving as the omitted benchmark week (i.e., we include indicator variables for 10 weeks, and omit an indicator for one week to serve as the benchmark).

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

on the true impact of alcohol on short-run unnatural mortality in South Africa. The main reason for this is that the July Alcohol Ban occurred against the backdrop of COVID-19 which implies that during our control period (the first half of the Level 3 period), people were more likely to be at home and less likely to be going out to bars and restaurants in comparison to the same period in previous years (e.g., see the Google mobility trends in Figure 10 in the Appendices, which also shows that there was not a sharp change in these trends at the onset of the alcohol ban). This depressed level of social activity translated into a lower benchmark level of unnatural mortality in our main control period in comparison to the same period in previous years (see Figure 2). Therefore, we are comparing the outcomes observed during the July Alcohol Ban period to an already lowered base level of these outcomes, which suggests that our estimated effect sizes are likely to be smaller than they would be if an alcohol ban were implemented during a year with more active social activity.[37] Importantly, since the effect sizes that we observe are still so large, we view these results as being highly informative for indicating how large the influence of alcohol is when social activity is at normal pre-COVID levels.[38]

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

Fourth, while the evidence that we report on homicides and assaults suggests that a large part of the influence that alcohol has on mortality due to unnatural causes is mediated by alcohol-induced aggressive behavior, it is important not to neglect road-traffic collisions as another important potential channel through which alcohol can induce injury and death. Unfortunately, at the time of

[37] An additional reason why our estimates likely constitute a lower bound on the true effect size of the presence of alcohol in society is that, according to media reports, compliance with the alcohol sales ban was imperfect. Some examples of the media reports include articles in the Guardian (2020), the Economist (2020), and a letter by Prinesha Naidoo published in Bloomberg (2020). Further, Onya et al. (2012) and Londani et al. (2019) report that many South Africans are experienced at making homemade alcohol.

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

writing this paper, it was not possible for us to obtain data that would allow us to study this channel directly. However, in the future more detailed cause-of-death data may become available and could be used retrospectively to provide direct evidence on the effect that the ban had on road-traffic collision mortality. Given the background pandemic context, an important consideration to keep in mind when thinking about road-traffic collision deaths during this period is that it is not entirely clear a priori whether the lower-than-usual level of traffic volume and road congestion would increase or decrease the number of road-traffic collision fatalities. It is plausible that emptier roads can result in more speeding and more deaths (for example, in the United States there were more motor vehicle deaths and lower traffic volumes during the period from June 2020 to December 2020 in comparison to the same period in 2019, according to preliminary estimates by the National Safety Council).[39] However, in the South African context there are two reasons to believe that road-traffic fatalities were lower than usual during our control period. First, Navsaria et al. (2021) show that during June 2020 (the first four weeks of our control period) the number of trauma patients admitted due to a road traffic collisions was 32% lower than pre-COVID levels in a large tertiary urban trauma centre in Cape Town, South Africa. Second, Figure 2 shows that the level of unnatural mortality was lower-than-usual during our control period (relative to previous years), indicating that if road-traffic fatalities were higher-than-usual, there would have needed to be a much larger drop in some other cause of unnatural mortality. Taken together, the available evidence suggests that road-traffic fatalities were likely lower-than-usual in our control period, which implies that there was less scope for reducing road-traffic fatalities by banning alcohol than would normally be the case. This further supports the idea that we are estimating a lower bound on the impact of alcohol on unnatural mortality.

The results discussed above raise important questions regarding the optimal design of alcohol control policy. In particular, since several of the social harms we study (e.g., homicide, assault, rape) involve the cost of an action being borne by another individual in society, there seems to be a potential mandate for policy intervention. However, policy discussions surrounding optimal alcohol control are complex as they involve taking a global perspective and balancing all of the social benefits of alcohol consumption (which are non-trivial to measure) against the large set of potential short-term and long-term social costs (which are also typically difficult to causally

[39]For further details, see Bolotnikova (2022) and National Safety Council (2022). However, it is also worth noting that this pattern of behavior in the United States is not representative of the evidence from around the world during the pandemic. Yasin et al. (2021) review the evidence on traffic volumes and fatalities during April 2020 from 36 countries and find that 32 of those countries experienced a reduction in road deaths in comparison to 2019. Amberber et al. (2021) show that in Toronto there was a substantial reduction in the collision rate leading to a fatality or seriously injured person between March 2020 and June 2020 relative to 2019, with the collision rate remaining at a lower-than-usual level after “re-opening”.

estimate).[40] Welfare analysis is further complicated by the fact that some of the costs of alcohol consumption are borne by the individual themselves, such as the influence on their long-term health and cognitive functioning, and on the quality of their short-term decision-making. A fully rational model would ascribe a limited role for policy intervention if these were the only costs, since the individual consuming alcohol is assumed to be factoring in these costs when they maximize their own lifetime utility and decide to consume alcohol. However, a behavioral model that allows for hyperbolic discounting, (non-rational) addiction, bounded rationality or imperfect foresight regarding the future costs of consuming alcohol would permit a role for welfare-enhancing intervention that assists individuals in overcoming their own behavioral biases (see, e.g., Simon, 1984; Ainslie, 1991; Orphanides and Zervos, 1995; Laibson, 1997; Suranovic et al., 1999; Rubinstein, 2003). Consequently, there is an entire literature devoted to the design of optimal alcohol control (see the Handbook chapter by Cawley and Ruhm, 2011, for a discussion of traditional and behavioral economics models of risky behavior, and their implications for the design of policy interventions). In relation to the current paper, the discussion in Carpenter and Dobkin (2011) provides the most useful benchmark case of welfare analysis. Carpenter and Dobkin (2011) conduct an exercise in which they take evidence on the impact of minimum legal drinking age laws on short-term alcohol-related harms and, via a series of assumptions, discuss the social costs and benefits of lowering the legal drinking age in the United States. Their discussion highlights the numerous challenges faced in making progress when taking a general perspective that weighs up all the costs and benefits in an exercise of this nature. The compromises involved in making progress include omitting all long-run costs from the analysis and making strong assumptions in estimating the consumer surplus generated by alcohol consumption.

Here, therefore, we have opted not to try to take a general perspective that considers all the costs and benefits, but rather highlight what we view as the main policy lessons of our results. One of the key take-aways from our results is that alcohol consumption can play a pivotal role in inducing

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

aggressive behavior in society at a significant scale, resulting in substantial harm.[41] When thinking about the policy implications of this, it is important to consider that the evidence from the existing literature, along with the pattern of drinking observed in South Africa (where heavy drinking is common amongst those who drink), points towards heavy drinking, as opposed to social drinking, as a key driver of this aggressive behavior (Duke et al., 2011; Kuhns et al., 2014; Tomlinson et al., 2016; Matzopoulos et al., 2021). This implies that a society that wishes to alleviate these short-run harms from alcohol consumption should start by targeting a reduction in the alcohol consumption of the heaviest drinkers in society. This provides a clear principle to guide which policy levers to prioritize.[42] For example, one candidate policy lever that has been proposed to reduce heavy drinking is the use of minimum unit pricing (MUP). The rationale for this is that the heaviest drinkers typically spend the least per unit of pure alcohol and will be the group that reduces their consumption most in response to an increase in the floor price of a standard drink (Holmes et al., 2014; O'Donnell et al., 2019; van Walbeek and Chelwa, 2021; Gibbs et al., 2021). As noted in the introduction, South Africa is not at all unusual in terms of the proportion of the adult population that engage in heavy episodic drinking, with many countries around the world observing a higher proportion of the population engaging in 'binge-drinking'. While other background social factors may influence the way that intoxication manifests in behavior, potentially making South Africa a better model for countries facing similar social issues (e.g., Brazil, Russia), the results in this paper should serve as a warning for all countries with substantial levels of heavy drinking.

The results show how large an influence alcohol can have in causally generating aggressive behavior and harmful outcomes. Even in countries where homicide and assault rates are lower, the role that alcohol can play in generating these outcomes is important to pay attention to (as evidenced by the study by Kuhns et al., 2014, that documents that a high proportion of homicide perpetrators in the United States, Europe and Australia, have alcohol in their system). Furthermore, since alcohol-induced aggressive behavior may manifest in other harder-to-detect forms of abuse,

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

[42] Furthermore, a policy that targets the heaviest drinkers in society is likely to have a smaller negative impact on consumer surplus than one that targets all drinkers. However, a caveat that should be mentioned is that a policy that targets the heaviest drinkers is also likely to target individuals from lower socio-economic backgrounds, which can raise concerns regarding equality and paternalism.

these society level results suggest that more work should be done to evaluate the causal effect of alcohol on other aggression-related outcomes such as gender-based violence, child abuse, and emotional abuse in the home. 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.

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.

Ainslie, G. (1991). Derivation of "rational" economic behavior from hyperbolic discount curves. *American Economic Review 81*(2), 334–340.

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.

Amberber, N., A. Howard, M. Winters, M. A. Harris, I. Pike, A. Machpherson, M.-S. Cloutier, S. A. Richmond, B. Hagel, P. Fuselli, et al. (2021). Road traffic injury during the COVID-19 pandemic: Cured or a continued threat? *University of Toronto Journal of Public Health 2*(1).

Asik, G. A. and E. N. Ozen (2021). It takes a curfew: The effect of COVID-19 on female homicides. *Economics Letters 200*, 109761.

Barron, K., D. Bradshaw, C. D. Parry, R. Dorrington, P. Groenewald, R. Laubscher, and R. Matzopoulos (2020). Alcohol and short-run mortality: Evidence from a modern-day prohibition. *Available at SSRN 3744031*.

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.

Bindler, A., R. Hjalmarsson, N. Ketel, and A. Mitrut (2021). Discontinuities in the age-victimization profile and the determinants of victimization. *IZA Discussion Paper No. 14917*.

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.

Bolotnikova, M. (2022). Vox: America's car crash epidemic. `https://www.vox.com/22675358/us-car-deaths-year-traffic-covid-pandemic`. Online; published 19-September-2021; accessed 11-February-2022.

Bullinger, L. R., J. B. Carr, and A. Packham (2021). COVID-19 and crime: Effects of stay-at-home orders on domestic violence. *American Journal of Health Economics 7*(3), 249–280.

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

Cameron, C. and D. Miller (2015). A practitioner's guide to cluster-robust inference. *Journal of Human Resources 50*(2), 317–372.

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.

Cawley, J. and C. J. Ruhm (2011). The economics of risky health behaviors. In *Handbook of Health Economics*, Volume 2, pp. 95–199. Elsevier.

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.

Chu, K. M., J.-L. Marco, E. O. Owolabi, R. Duvenage, M. Londani, C. Lombard, and C. D. Parry (2022). Trauma trends during COVID-19 alcohol prohibition at a South African regional hospital. *Drug and Alcohol Review 41*(1), 13–19.

Clarke, D. and K. Tapia-Schythe (2021). Implementing the panel event study. *The Stata Journal 21*(4), 853–884.

Darke, S. (2010). The toxicology of homicide offenders and victims: a review. *Drug and Alcohol Review 29*(2), 202–215.

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.

Duke, A. A., P. R. Giancola, D. H. Morris, J. C. Holt, and R. L. Gunn (2011). Alcohol dose and aggression: Another reason why drinking more is a bad idea. *Journal of Studies on Alcohol and Drugs 72*(1), 34–43.

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.

Gawryszewski, V. P. and E. M. S. Rodrigues (2006). The burden of injury in Brazil, 2003. *São Paulo Medical Journal 124*(4), 208–213.

Gibbs, N., C. Angus, S. Dixon, C. Parry, and P. Meier (2021). Effects of minimum unit pricing for alcohol in South Africa across different drinker groups and wealth quintiles: A modelling study. *BMJ Open 11*(8), e052879.

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/gcis_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.

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.

Holmes, J., Y. Meng, P. S. Meier, A. Brennan, C. Angus, A. Campbell-Burton, Y. Guo, D. Hill-McManus, and R. C. Purshouse (2014). Effects of minimum unit pricing for alcohol on different income and socioeconomic groups: A modelling study. *The Lancet 383*(9929), 1655–1664.

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.

Huber, P. J. (1967). The behavior of maximum likelihood estimates under nonstandard conditions. In *Proceedings of the Fifth Berkeley Symposium on Mathematical Statistics and Probability: Weather modification*, Volume 1, pp. 221–233. Berkeley: University of California Press.

Ivandić, R., T. Kirchmaier, and N. Torres-Blas (2021). Football, alcohol and domestic abuse. *CEP Discussion Paper No. 1781*.

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.

Kuhns, J. B., M. L. Exum, T. A. Clodfelter, and M. C. Bottia (2014). The prevalence of alcohol-involved homicide offending: A meta-analytic review. *Homicide studies 18*(3), 251–270.

Kuhns, J. B., D. B. Wilson, T. A. Clodfelter, E. R. Maguire, and S. A. Ainsworth (2011). A meta-analysis of alcohol toxicology study findings among homicide victims. *Addiction 106*(1), 62–72.

Laibson, D. (1997). Golden eggs and hyperbolic discounting. *Quarterly Journal of Economics 112*(2), 443–478.

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. *Journal of Public Economics 189*, 104241.

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.

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., A. Cois, C. Probst, C. D. Parry, N. Vellios, K. Sorsdahl, J. Joubert, and R. Pacella (2021). Estimating the burden of disease from alcohol use in South Africa in 2000, 2006, 2012. *Available at SSRN 3854745*.

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). 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., 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., 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*.

Moultrie, T., R. Dorrington, R. Laubscher, P. Groenewald, C. Parry, R. Matzopoulos, and D. Bradshaw (2021). Unnatural deaths, alcohol bans and curfews: Evidence from a quasi-natural experiment during COVID-19. *South African Medical Journal 111*(9), 834–837.

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.

National Safety Council (2022). Monthly Preliminary Motor-Vehicle Fatality Estimates - November 2021. `https://injuryfacts.nsc.org/motor-vehicle/overview/p`

reliminary-monthly-estimates/. Online; published November-2021; accessed 11-February-2022.

Navsaria, P., A. Nicol, C. Parry, R. Matzopoulos, S. Maqungo, and R. Gaudin (2021). The effect of lockdown on intentional and non-intentional injury during the COVID-19 pandemic in Cape Town, South Africa: A preliminary report. *South African Medical Journal 111*(2), 110–113.

Newey, W. K., K. D. West, et al. (1987). A simple, positive semi-definite, heteroskedasticity and autocorrelation consistent covariance matrix. *Econometrica 55*(3), 703–708.

Nivette, A. E., R. Zahnow, R. Aguilar, A. Ahven, S. Amram, B. Ariel, M. J. A. Burbano, R. Astolfi, D. Baier, H.-M. Bark, et al. (2021). A global analysis of the impact of COVID-19 stay-at-home restrictions on crime. *Nature Human Behaviour 5*(7), 868–877.

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'Donnell, A., P. Anderson, E. Jané-Llopis, J. Manthey, E. Kaner, and J. Rehm (2019). Immediate impact of minimum unit pricing on alcohol purchases in Scotland: Controlled interrupted time series analysis for 2015-18. *bmj 366*.

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.

Orphanides, A. and D. Zervos (1995). Rational addiction with learning and regret. *Journal of Political Economy 103*(4), 739–758.

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 100*(4), 426–429.

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

Phillips, R. (2014). *Alcohol: A History*. Chapel Hill: University of North Carolina Press.

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 impact of lockdowns on crime and violence against women–evidence from India. *Available at SSRN 3623331*.

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.

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.

Roodman, D., M. Ø. Nielsen, J. G. MacKinnon, and M. D. Webb (2019). Fast and wild: Bootstrap inference in stata using boottest. *The Stata Journal 19*(1), 4–60.

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.

Rubinstein, A. (2003). "Economics and psychology"? The case of hyperbolic discounting. *International Economic Review 44*(4), 1207–1216.

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.

Schmidheiny, K. and S. Siegloch (2019). On event study designs and distributed-lag models: Equivalence, generalization and practical implications.

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*.

Simon, H. A. (1984). *Models of bounded rationality*, Volume 1. The MIT Press.

Starodubov, V. I., L. B. Marczak, E. Varavikova, B. Bikbov, S. P. Ermakov, J. Gall, S. D. Glenn, M. Griswold, B. Idrisov, M. Kravchenko, D. Lioznov, E. Loyola, I. Rakovac, S. Vladimirov,

V. Vlassov, C. Murray, and M. Naghavi (2018). The burden of disease in russia from 1980 to 2016: a systematic analysis for the global burden of disease study 2016. *The Lancet 392*(10153), 1138–1146.

Stock, J. H., M. W. Watson, et al. (2012). *Introduction to econometrics*, Volume 3. Pearson New York.

Suranovic, S. M., R. S. Goldfarb, and T. C. Leonard (1999). An economic theory of cigarette addiction. *Journal of Health Economics 18*(1), 1–29.

Tomlinson, M. F., M. Brown, and P. N. Hoaken (2016). Recreational drug use and human aggressive behavior: A comprehensive review since 2003. *Aggression and violent behavior 27*, 9–29.

van Walbeek, C. and G. Chelwa (2021). The case for minimum unit prices on alcohol in South Africa. *South African Medical Journal 111*(7), 680–684.

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.

Viscusi, W. K. and J. E. Aldy (2003). The value of a statistical life: a critical review of market estimates throughout the world. *Journal of Risk and Uncertainty 27*(1), 5–76.

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.

Webb, M. D. (2013). Reworking wild bootstrap based inference for clustered errors. *Queen's Economics Department Working Paper*.

White, H. (1980). A heteroskedasticity-consistent covariance matrix estimator and a direct test for heteroskedasticity. *Econometrica*, 817–838.

White, H. (1982). Maximum likelihood estimation of misspecified models. *Econometrica*, 1–25.

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.

Yasin, Y. J., M. Grivna, and F. M. Abu-Zidan (2021). Global impact of COVID-19 pandemic on road traffic collisions. *World Journal of Emergency Surgery 16*(1), 1–14.

APPENDICES

A Additional Tables

Table 5: Replication of Table 1 (Reporting full set of weekend variable coefficients)

	(1a)	(1b)	(1c)
Level 3 Period = 1	6.88	3.00	1.75
(1/6-17/8)	(5.14)	(2.80)	(2.20)
Alcohol Ban Period = 1	-2.17	-0.28	0.91
(13/7-17/8)	(6.88)	(3.31)	(2.73)
Level 3 Period x Year=2020	-13.46**	-2.29	-1.78
	(6.19)	(4.32)	(4.51)
Alcohol Ban Period x Year=2020	-20.93**	-21.55***	-21.99***
	(8.41)	(5.30)	(5.40)
Weekend Day = 1		66.71***	
		(2.86)	
Weekend Day x Year=2020		-26.61***	-25.24***
		(7.35)	(7.51)
First Weekend of Month = 1		55.63***	37.33***
		(6.95)	(6.10)
Last Weekend of Month = 1		28.18***	21.85***
		(5.21)	(5.50)
First Weekend x Year=2020		-29.19*	-28.46*
		(16.00)	(15.52)
Last Weekend x Year=2020		-16.55	-18.07
		(16.47)	(16.57)
Constant	136.96***	115.14***	130.76***
	(1.56)	(1.13)	(4.42)
Day of Week FEs			Y
Day of Month FEs			Y
Year FEs			Y
Observations	1460	1460	1460
Adjusted R^2	0.008	0.524	0.599

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

A.1 Replications of main tables, excluding week 1 of Level 3 period

Table 6: Replication of Table 1 (1st week of Level 3 excluded from control period)

	(1a)	(1b)	(1c)
Level 3 Period (Excl. week 1) = 1	4.13	2.35	2.89
(8/6-17/8)	(5.41)	(3.11)	(2.38)
Alcohol Ban Period = 1	0.12	0.17	-0.14
(13/7-17/8)	(7.09)	(3.61)	(2.89)
Level 3 Period (Excl. week 1) x Year=2020	-16.57***	-6.46	-6.24
	(6.40)	(4.02)	(4.36)
Alcohol Ban Period x Year=2020	-17.82**	-17.48***	-17.96***
	(8.57)	(5.10)	(5.28)
Constant	137.41***	115.37***	131.09***
	(1.54)	(1.11)	(4.40)
Weekend Controls		Y	Y
Day of Week FEs			Y
Day of Month FEs			Y
Year FEs			Y
Observations	1460	1460	1460
Adjusted R^2	0.008	0.524	0.599

Notes: (i) Each observation contains unnatural mortality data for a single day, (ii) Heteroskedasticity robust standard errors (Huber, 1967; White, 1980, 1982) are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between January, 1 and December, 31 of each year, excluding February, 29, (iv) All three columns report estimates of the impact on unnatural mortality, and differ only in their specifications, with column (1a) the simplest specification, (1b) adding controls for the weekend, and (1c) adding fixed effects. The weekend controls include an indicator variable for a weekend day, an indicator for the first weekend of the month, and one for the last weekend of the month. They also include all three variables interacted with a 2020 indicator variable. In column (1c), the weekend day indicator is excluded due to the Day of Week FEs, (v) This table is a replication of Table 1 and differs in only one respect—the Level 3 period is shortened by one week here, with the first week (1 June 2020 - 7 June 2020) excluded. The reason for this is that we observe a jump in unnatural mortality during this week, perhaps due to it being the first week after the previous alcohol ban.

Table 7: Replication of Table 2 (1st week of Level 3 excluded from control period)

	Men			Women		
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
Level 3 Period (Excl. week 1) = 1	3.51	2.06	2.85	0.62	0.29	0.03
(8/6-17/8)	(4.70)	(2.63)	(2.07)	(0.93)	(0.77)	(0.68)
Alcohol Ban Period = 1	-0.04	-0.00	-0.30	0.16	0.17	0.17
(13/7-17/8)	(6.18)	(3.08)	(2.53)	(1.20)	(0.95)	(0.86)
Level 3 Period (Excl. week 1) x Year=2020	-10.91**	-2.56	-3.40	-5.66***	-3.91***	-2.85**
	(5.56)	(3.41)	(3.63)	(1.26)	(1.16)	(1.24)
Alcohol Ban Period x Year=2020	-17.95**	-17.65***	-18.05***	0.13	0.17	0.08
	(7.45)	(4.52)	(4.64)	(1.73)	(1.47)	(1.47)
Constant	106.94***	88.13***	101.22***	30.47***	27.24***	29.87***
	(1.29)	(0.93)	(3.57)	(0.31)	(0.26)	(1.33)
Weekend Controls		Y	Y		Y	Y
Day of Week FEs			Y			Y
Day of Month FEs			Y			Y
Year FEs			Y			Y
Observations	1460	1460	1460	1460	1460	1460
Adjusted R^2	0.007	0.535	0.606	0.009	0.307	0.374

Notes: (i) Each observation contains unnatural mortality data for a single day, (ii) Heteroskedasticity robust standard errors (Huber, 1967; White, 1980, 1982) are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between January, 1 and December, 31 of each year, excluding February, 29, (iv) All columns report estimates of the impact on unnatural mortality, and differ only in their specifications, with column (*a) the simplest specification, (*b) adding controls for the weekend, and (*c) adding fixed effects. The weekend controls include an indicator variable for a weekend day, an indicator for the first weekend of the month, and one for the last weekend of the month. They also include all three variables interacted with a 2020 indicator variable. In column (*c), the weekend day indicator is excluded due to the Day of Week FEs, (v) This table is a replication of Table 2 and differs in only one respect—the Level 3 period is shortened by one week here, with the first week (1 June 2020 - 7 June 2020) excluded. The reason for this is that we observe a jump in unnatural mortality during this week, perhaps due to it being the first week after the previous alcohol ban.

Table 8: Replication of Table 3 (1st week of Level 3 excluded from control period)

	Men			Women		
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
Level 3 Period (Excl. week 1) = 1	2.85	1.88	2.19	-0.27	-0.46	-0.53
(8/6-17/8)	(3.07)	(1.71)	(1.45)	(0.57)	(0.48)	(0.45)
Alcohol Ban Period = 1	-1.56	-1.54	-1.74	0.47	0.48	0.54
(13/7-17/8)	(3.99)	(1.96)	(1.76)	(0.74)	(0.59)	(0.54)
Level 3 Period (Excl. week 1) x Year=2020	-8.04**	-2.44	-2.01	-1.88***	-0.86	-0.67
	(3.76)	(2.23)	(2.50)	(0.72)	(0.69)	(0.74)
Alcohol Ban Period x Year=2020	-10.19**	-10.00***	-10.24***	-0.79	-0.77	-0.82
	(4.84)	(2.91)	(3.23)	(0.98)	(0.90)	(0.91)
Constant	53.38***	41.01***	46.99***	11.58***	9.79***	11.63***
	(0.84)	(0.57)	(2.08)	(0.16)	(0.14)	(0.66)
Weekend Controls		Y	Y		Y	Y
Day of Week FEs			Y			Y
Day of Month FEs			Y			Y
Year FEs			Y			Y
Observations	1460	1460	1460	1460	1460	1460
Adjusted R^2	0.008	0.551	0.611	0.006	0.321	0.367

Notes: (i) Each observation contains unnatural mortality data for a single day, (ii) Heteroskedasticity robust standard errors (Huber, 1967; White, 1980, 1982) are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between January, 1 and December, 31 of each year, excluding February, 29, (iv) All columns report estimates of the impact on unnatural mortality, and differ only in their specifications, with column (*a) the simplest specification, (*b) adding controls for the weekend, and (*c) adding fixed effects. The weekend controls include an indicator variable for a weekend day, an indicator for the first weekend of the month, and one for the last weekend of the month. They also include all three variables interacted with a 2020 indicator variable. In column (*c), the weekend day indicator is excluded due to the Day of Week FEs, (v) This table is a replication of Table 3 and differs in only one respect—the Level 3 period is shortened by one week here, with the first week (1 June 2020 - 7 June 2020) excluded. The reason for this is that we observe a jump in unnatural mortality during this week, perhaps due to it being the first week after the previous alcohol ban.

A.2 Reproducing Table 3 for other age groups

Table 9: Impact of the alcohol ban on mortality ($\leq$ 14 years)

	Men			Women		
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
Level 3 Period = 1	-1.01***	-1.13***	-1.22***	0.13	0.08	0.02
(1/6-17/8)	(0.28)	(0.27)	(0.27)	(0.20)	(0.19)	(0.19)
Alcohol Ban Period = 1	0.22	0.29	0.37	-0.10	-0.09	-0.07
(13/7-17/8)	(0.38)	(0.37)	(0.37)	(0.28)	(0.28)	(0.28)
Level 3 Period x Year=2020	-0.22	0.11	0.29	-1.02***	-0.86**	-0.78*
	(0.46)	(0.48)	(0.51)	(0.38)	(0.38)	(0.40)
Alcohol Ban Period x Year=2020	-0.39	-0.48	-0.48	0.68	0.72	0.71
	(0.67)	(0.66)	(0.66)	(0.52)	(0.51)	(0.51)
Constant	7.06***	6.57***	6.32***	4.39***	4.07***	4.13***
	(0.10)	(0.10)	(0.53)	(0.07)	(0.07)	(0.40)
Weekend Controls		Y	Y		Y	Y
Day of Week FEs			Y			Y
Day of Month FEs			Y			Y
Year FEs			Y			Y
Observations	1460	1460	1460	1460	1460	1460
Adjusted R^2	0.015	0.089	0.108	0.002	0.053	0.058

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

Table 10: Impact of the alcohol ban on mortality (35 to 54 years)

	Men			Women		
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
Level 3 Period = 1	1.90	1.12	1.13*	0.34	0.18	0.01
(1/6-17/8)	(1.31)	(0.82)	(0.68)	(0.32)	(0.29)	(0.27)
Alcohol Ban Period = 1	-0.24	0.17	0.49	-0.23	-0.14	-0.07
(13/7-17/8)	(1.85)	(1.11)	(0.94)	(0.45)	(0.40)	(0.39)
Level 3 Period x Year=2020	0.15	2.23	0.91	-1.15**	-0.70	-0.38
	(1.80)	(1.45)	(1.39)	(0.49)	(0.49)	(0.54)
Alcohol Ban Period x Year=2020	-6.56**	-6.59***	-6.70***	-1.16	-1.24*	-1.26*
	(2.59)	(1.98)	(1.84)	(0.72)	(0.66)	(0.66)
Constant	32.86***	27.58***	33.16***	8.12***	7.24***	7.54***
	(0.39)	(0.31)	(1.45)	(0.12)	(0.11)	(0.60)
Weekend Controls		Y	Y		Y	Y
Day of Week FEs			Y			Y
Day of Month FEs			Y			Y
Year FEs			Y			Y
Observations	1460	1460	1460	1460	1460	1460
Adjusted R^2	0.004	0.450	0.527	0.007	0.159	0.202

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

Table 11: Impact of the alcohol ban on mortality ($\geq$ 55 years)

	Men			Women		
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
Level 3 Period = 1	0.78*	0.52	0.32	0.70**	0.61**	0.44
(1/6-17/8)	(0.40)	(0.36)	(0.34)	(0.29)	(0.28)	(0.28)
Alcohol Ban Period = 1	0.71	0.83	0.96*	-0.24	-0.18	-0.14
(13/7-17/8)	(0.57)	(0.52)	(0.50)	(0.37)	(0.36)	(0.36)
Level 3 Period x Year=2020	-1.26*	-0.53	-0.50	-1.87***	-1.68***	-1.28***
	(0.66)	(0.66)	(0.68)	(0.42)	(0.43)	(0.45)
Alcohol Ban Period x Year=2020	-2.35**	-2.44**	-2.48**	1.66***	1.64***	1.64***
	(1.06)	(1.04)	(1.01)	(0.64)	(0.63)	(0.63)
Constant	13.46***	12.87***	14.56***	6.35***	6.14***	6.65***
	(0.13)	(0.14)	(0.78)	(0.09)	(0.10)	(0.52)
Weekend Controls		Y	Y		Y	Y
Day of Week FEs			Y			Y
Day of Month FEs			Y			Y
Year FEs			Y			Y
Observations	1460	1460	1460	1460	1460	1460
Adjusted R^2	0.014	0.089	0.151	0.008	0.035	0.067

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

A.3 Robustness checks for crime data

Table 12: Testing for a pre-trend leading up to the July alcohol ban

	Homicide		Assault (GBH)		Reported Rape		Unnatural Mortality	
	(H1)	(H2)	(A1)	(A2)	(R1)	(R2)	(U1)	(U2)
Week Counter	0.63	0.22	3.66	-2.44	3.43*	2.90**	0.10	-0.50
	(1.65)	(0.99)	(14.12)	(4.71)	(1.89)	(1.18)	(2.35)	(1.40)
Weekend Day = 1		20.61***		210.68***		27.01***		31.15***
		(2.87)		(16.79)		(3.43)		(3.75)
First Weekend of Month = 1		14.38***		213.42***		18.64***		21.17***
		(3.09)		(58.67)		(4.43)		(7.11)
Last Weekend of Month = 1		21.59**		100.98**		16.03**		20.67**
		(10.24)		(46.02)		(5.92)		(9.09)
Constant	55.63***	46.45***	349.97***	253.52***	87.94***	76.65***	125.27***	112.17***
	(6.17)	(3.91)	(53.13)	(16.96)	(7.30)	(4.97)	(8.30)	(4.84)
Observations	35	35	35	35	35	35	35	35
Adjusted R^2	-0.027	0.747	-0.029	0.878	0.033	0.697	-0.030	0.670

Notes: (i) Each observation contains the total number of cases for the relevant outcome for a single day, (ii) Heteroskedasticity robust standard errors (Huber, 1967; White, 1980, 1982) are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2020, between June, 8 and July, 12 (iv) The outcome of interest is described in the column header, with two specifications estimated for each of the four outcomes, (v) This table examines whether there is a pre-tend in the outcomes of interest leading up to the July alcohol ban. The key variable *Week Counter* takes a value of -5 for the 5th week before the ban and progresses to a value of -1 for the week before the ban. The 6th week before the ban is excluded due to the clear jump observed in the raw data during that week, following the previous longer alcohol ban.

Table 13: Replication of Table 4 (First week of Level 3 dropped)

	Homicide		Assault (GBH)		Reported Rape		Unnatural Mortality	
	(H1)	(H2)	(A1)	(A2)	(R1)	(R2)	(U1)	(U2)
Alcohol Ban Period = 1	-11.52***	-11.36***	-114.44***	-112.83***	-14.77***	-14.51***	-17.69***	-17.44***
	(3.04)	(1.92)	(26.71)	(13.11)	(4.72)	(3.14)	(4.86)	(3.26)
Weekend Day = 1		13.72***		166.81***		29.90***		26.28***
		(2.53)		(16.06)		(4.56)		(4.12)
First Weekend of Month = 1		14.17***		146.50**		14.92		16.25**
		(3.74)		(55.36)		(9.89)		(7.40)
Last Weekend of Month = 1		16.92**		37.00		-3.08		14.75*
		(7.17)		(52.55)		(10.11)		(7.83)
Constant	53.74***	48.05***	339.00***	280.85***	77.66***	68.44***	124.97***	115.69***
	(2.62)	(1.48)	(23.87)	(10.01)	(3.35)	(2.39)	(3.80)	(2.56)
Observations	71	71	71	71	71	71	71	71
Adjusted R^2	0.162	0.662	0.201	0.806	0.112	0.608	0.150	0.615

Notes: (i) Each observation contains the total number of cases for the relevant outcome for a single day, (ii) Heteroskedasticity robust standard errors (Huber, 1967; White, 1980, 1982) are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2020, between June, 8 and August, 17 (iv) The outcome of interest is described in the column header, with two specifications estimated for each of the four outcomes, (v) This Table is a replication of Table 4, with the only difference being that the first week of the Level 3 period is dropped, since the raw data suggests that there was a jump in the outcomes of interest during this week, perhaps due to the previous alcohol ban.

Table 14: Replication of Table 4 (Standard errors clustered at the calendar week level)

	Homicide		Assault (GBH)		Reported Rape		Unnatural Mortality	
	(H1)	(H2)	(A1)	(A2)	(R1)	(R2)	(U1)	(U2)
Alcohol Ban Period = 1	-15.59***	-15.06***	-148.94***	-142.15***	-20.23**	-19.28**	-23.10***	-22.28***
	(4.50)	(3.94)	(37.48)	(32.07)	(7.00)	(6.44)	(5.68)	(5.41)
Weekend Day = 1		11.17**		146.74***		26.83***		23.14***
		(3.90)		(27.32)		(5.58)		(5.81)
First Weekend of Month = 1		14.41***		149.39***		17.87**		19.04***
		(2.61)		(41.20)		(6.21)		(5.47)
Last Weekend of Month = 1		16.92**		37.00		-3.08		14.75**
		(6.08)		(58.23)		(11.80)		(6.59)
Constant	57.81***	52.44***	373.50***	315.59***	83.12***	73.90***	130.38***	121.26***
	(4.18)	(4.60)	(34.84)	(36.43)	(5.69)	(5.73)	(5.36)	(5.68)
Observations	78	78	78	78	78	78	78	78
Adjusted R^2	0.221	0.528	0.250	0.657	0.153	0.484	0.209	0.554

Notes: (i) Each observation contains the total number of cases for the relevant outcome for a single day, (ii) Standard errors are clustered at the calendar week level to account for potential interdependence on the time dimension (it also accounts for interdependence across years for a particular week of the year). These standard errors are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2020, between June, 1 and August, 17 (iv) The outcome of interest is described in the column header, with two specifications estimated for each of the four outcomes.

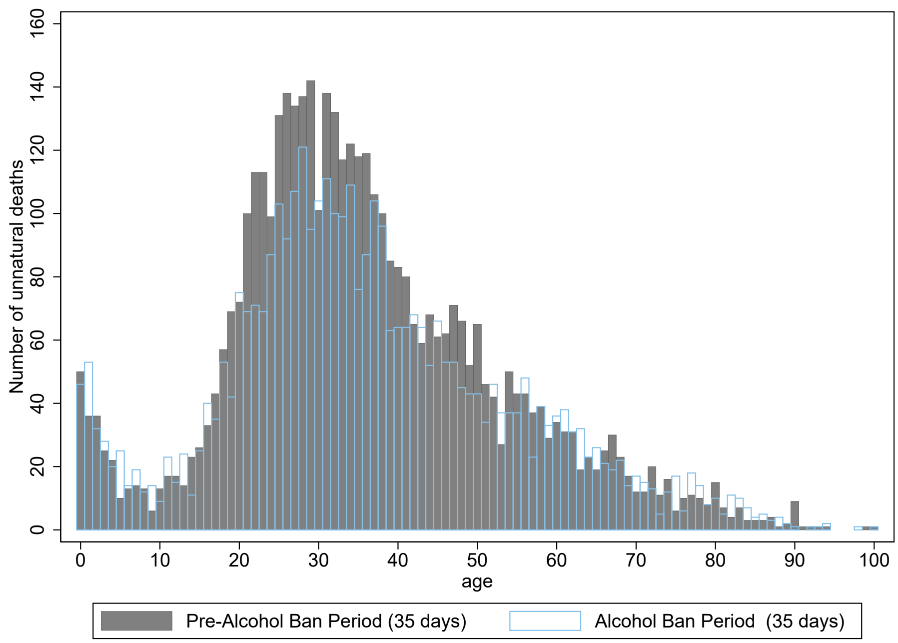
Table 15: Summary of relevant regulation changes during 2020 in South Africa

Start Date	End Date	ISO Weeks	COVID Level	Alcohol Policy	Curfew	Other Relevant Policy Changes	Announcement Date	Number of Days	Start Day	End Day
1/1/2020	3/18/2020	1 to 11	None	Normal Pre-COVID Regulations	No Curfew	Normal Pre-COVID Policy Landscape		78	Wed	Wed
3/19/2020	3/26/2020	12 to 13	Pre-Lockdown	Alcohol sales only permitted between 9AM and 6PM, Mon - Sat, and before 1PM on Sundays.	No Curfew	(i) Prohibition of gatherings above 100 persons, (ii) School closures, (iii) travel ban, (iv) non-legally binding encouragement to work from home.	Wed, 18 March 2020	8	Thur	Thur
3/27/2020	4/30/2020	14 to 18	Level 5	Complete ban on sale and transport of alcohol	Strict stay-at-home orders for non-essential services	(i) Legally required to stay home, unless providing an essential service. (ii) No gatherings permitted, (iii) No sales of non-essential goods (e.g. alcohol, cigarettes).	Wed, 25 March 2020	35	Fri	Thur
5/1/2020	5/31/2020	19 to 22	Level 4	Complete ban on sale and transport of alcohol	Weaker stay-at-home orders (selected businesses opened), Curfew 8PM - 5AM	Movement from to Level 5 to 4, including: (i) mandatory mask wearing in public, (ii) continued general lockdown, with allowance for: exercise between 6am and 9am within 5km radius of residence, (iii) curfew between 8pm and 5am, (iv) some businesses allowed to open (e.g. mining at 50%, agriculture, energy, manufacturing, construction and public works, wholesale trade).	Wed, 29 April 2020	31	Fri	Sun
6/1/2020	7/12/2020	23 to 28	Level 3	Alcohol sales permitted between 9AM and 5PM, Mon-Thur (off-site consumption)	No Curfew	Relaxing from Level 4 to 3: (i) Many businesses permitted to operate, (ii) Schools permitted to open [staggered implementation by Grade] (iii) exceptions to "no gathering" rule for work and religion, (iv) borders still largely closed.	Thur, 28 May 2020	42	Mon	Sun
7/13/2020	8/17/2020	29 to 33	Level 3 (Adjusted)	Complete ban on sale and transport of alcohol	Curfew between 9PM and 4AM (adjusted to 10PM to 4AM on 31 July 2020).	Level 3 restrictions as described above (except the additional of the alcohol ban and curfew).	Sun, 12 July 2020	36	Mon	Mon
8/18/2020	9/20/2020	34 to 38	Level 2	Off-site alcohol sales permitted Mon to Thur, 9AM to 5PM. On-site alcohol consumption, Monday to Sunday, until curfew.	Curfew between 10PM and 4AM	Further relaxation of economic restrictions, but (i) international travel restrictions retained, (ii) gatherings restricted to 50 people.	Sat, 15 August 2020	34	Tue	Sun
9/21/2020	12/28/2020	39 to 52	Level 1	Off-site alcohol sales permitted Mon to Fri, 9AM to 5PM. On-site alcohol consumption, Monday to Sunday, until curfew.	Curfew between midnight and 4AM (21/9/2020 - 14/12/2020); between 11PM and 4AM (15/12/2020 - 28/12/2020)	Removal of most restrictions, allowing (i) indoor gatherings up to 250 people, (ii) gradual easing of restrictions on international travel.	Wed, 16 September 2020	99	Mon	Mon
12/29/2020	2/1/2021	53 to 4	Level 3 (Adjusted)	Complete ban on sale of alcohol	Curfew between 9PM and 6AM (29/12/2021 - 11/1/2021); between 9PM and 5AM (12/1/2021 - 28/12/2020)	(i) Gatherings prohibited (with exceptions for businesses, e.g., restaurants, gyms, museums, and casinos); (ii) Non-essential establishments required to close at 8PM;	Mon, 28 December 2020	35	Tue	Mon
2/2/2021	2/28/2021	5 to 8	Level 3	Off-site alcohol sales permitted Mon to Thur, 9AM to 6PM. On-site alcohol consumption, Monday to Sunday (10AM to 10PM).	Curfew between 11PM and 4 AM	(i) Large social gatherings still not permitted, (ii) Mask wearing in public spaces remains compulsory, (iii) Vaccination programme started.	Mon, 1 February 2021	26	Tue	Sun

Notes: (i) We use a standardized international approach for counting weeks, adopting the International Organization of Standardization (ISO) method, which involves weeks starting on Mondays and ending on Sundays, starting with the first week of the Gregorian year containing a Thursday (i.e., the first week with at least 4 days). This is useful for our analysis, since the important policy periods we consider start on Mondays. An alternative standardization approach is the EPI Week, often used by the US CDC, which involves weeks starting on Sundays, and is therefore less appropriate here. (ii) When an ISO week spans two policy periods in the table, we've included the ISO week into the period that contains at least 4 days from that week. (iii) The details for all the relevant regulatory changes implemented during 2020 and summarized in this table can be found at this website: `https://www.gov.za/covid-19/resources/regulations-and-guidelines-coronavirus-covid-19`. (iv) Speeches made by the South African president during 2020 are collected and stored `https://www.thepresidency.gov.za/speeches` and `https://www.gov.za/speeches/statement-south-`.

B Additional Figures

Figure 6: Age distribution of unnatural mortality, before and during alcohol ban



Notes: (i) The figure shows the age distribution of unnatural mortality during the 5 weeks preceding the alcohol ban, and the 5 weeks during the July Alcohol Ban itself, (ii) This implies that the first week of the Level 3 period is omitted. This is done for two reasons. First, it keeps the length of the two periods constant, which allows comparison of raw numbers. Second, the inclusion of that week would make the difference between the two periods even more striking, since there was a spike in unnatural mortality during the first week of June, but this seems partially due to the end of the previous alcohol ban. Therefore, we take the conservative approach and omit this week, (iii) Since the July Alcohol Ban was 36 days, the last day is omitted.

Figure 7: Weekly violent crime statistics, June - August, 2020

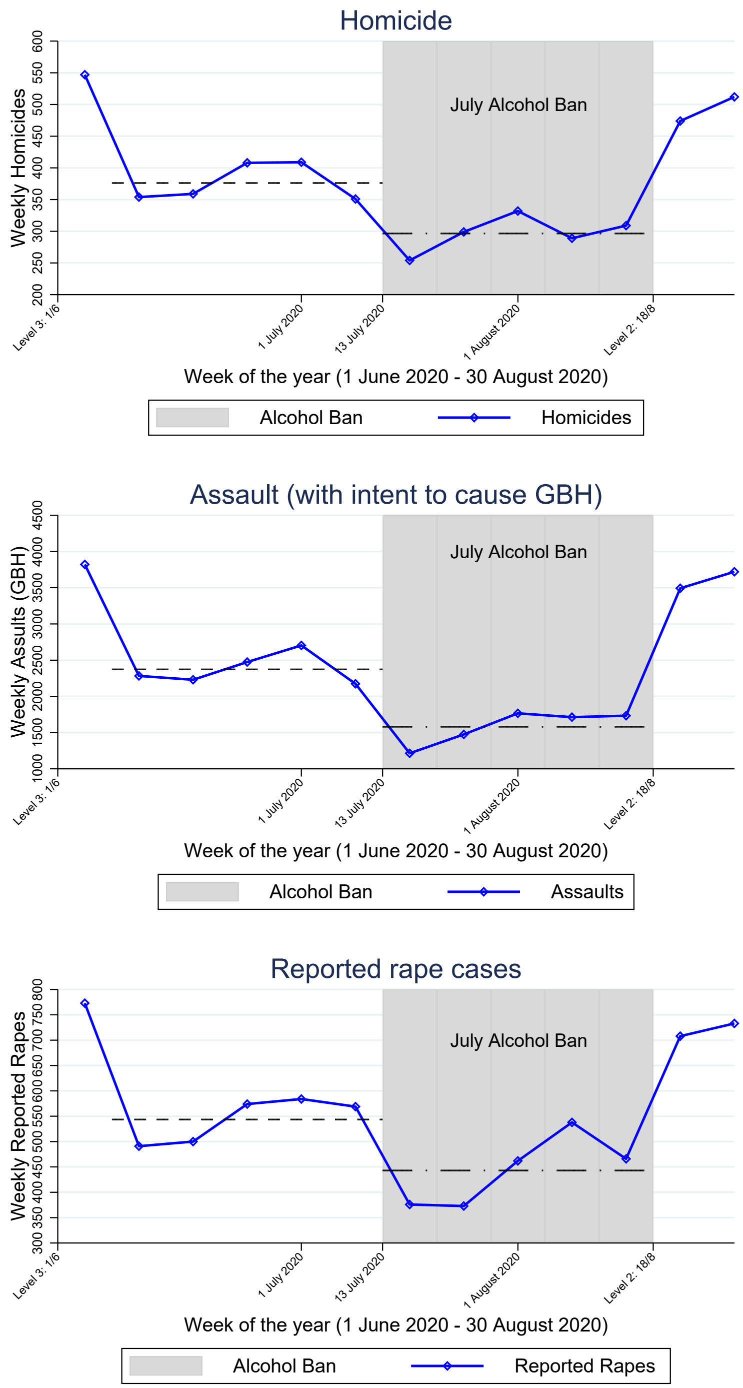
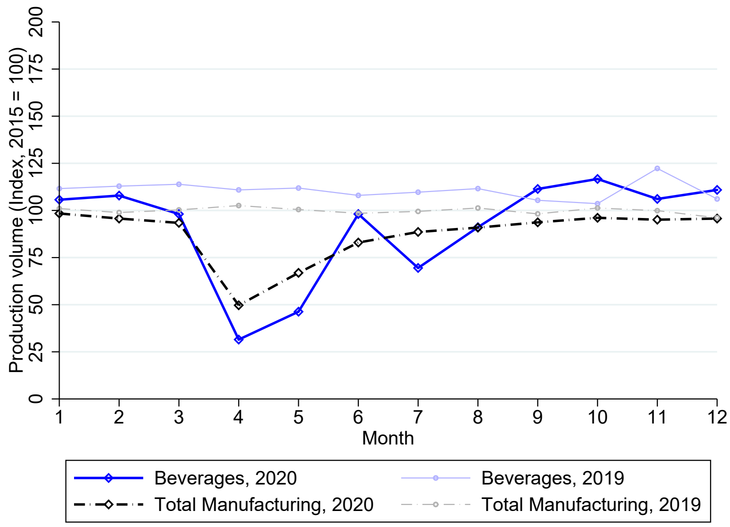
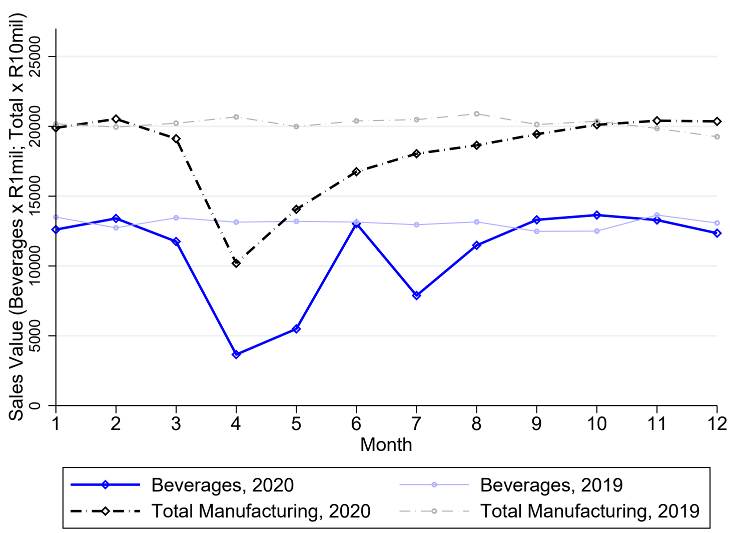


Figure 8: Monthly production volume in 2019 & 2020



Notes: (i) The figure reports the seasonally adjusted total volume of monthly production relative to the index year of 2015 for beverages and total manufacturing in South Africa for the years of 2019 and 2020, (ii) Source of data: Department of Statistics South Africa (www.statssa.gov.za).

Figure 9: Monthly sales value in 2019 and 2020.



Notes: (i) The figure reports the seasonally adjusted value of monthly sales for beverages and total manufacturing in South Africa for the years of 2019 and 2020, (ii) Source of data: Department of Statistics South Africa (www.statssa.gov.za).

Figure 10: Weekly Google Mobility statistics, March 2020 - March 2021

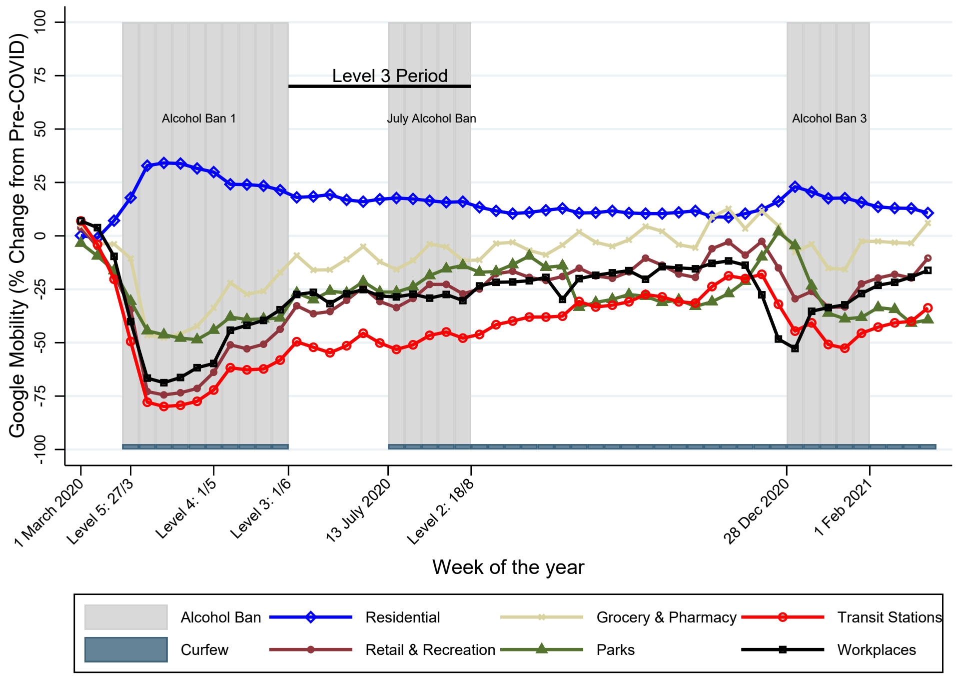
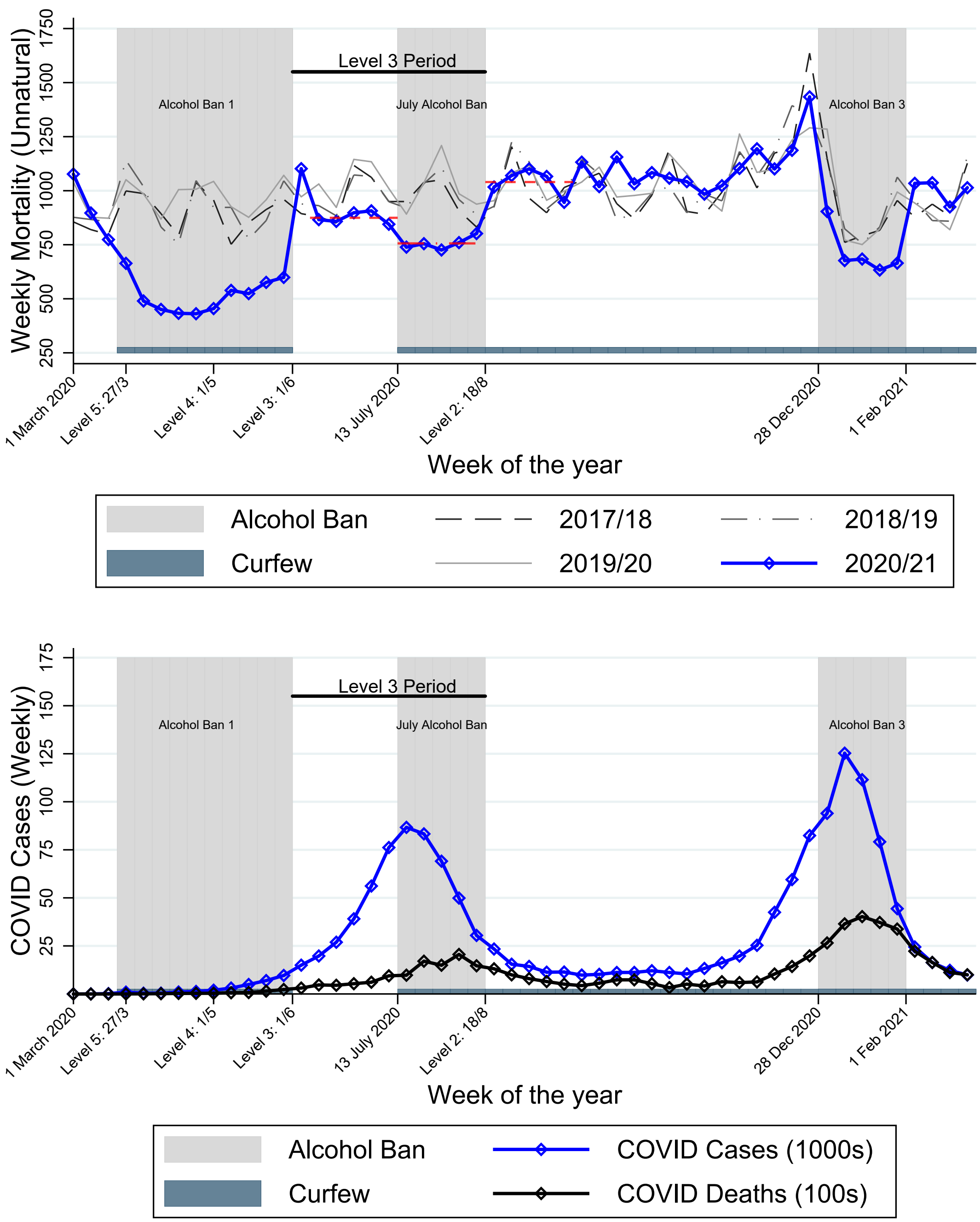
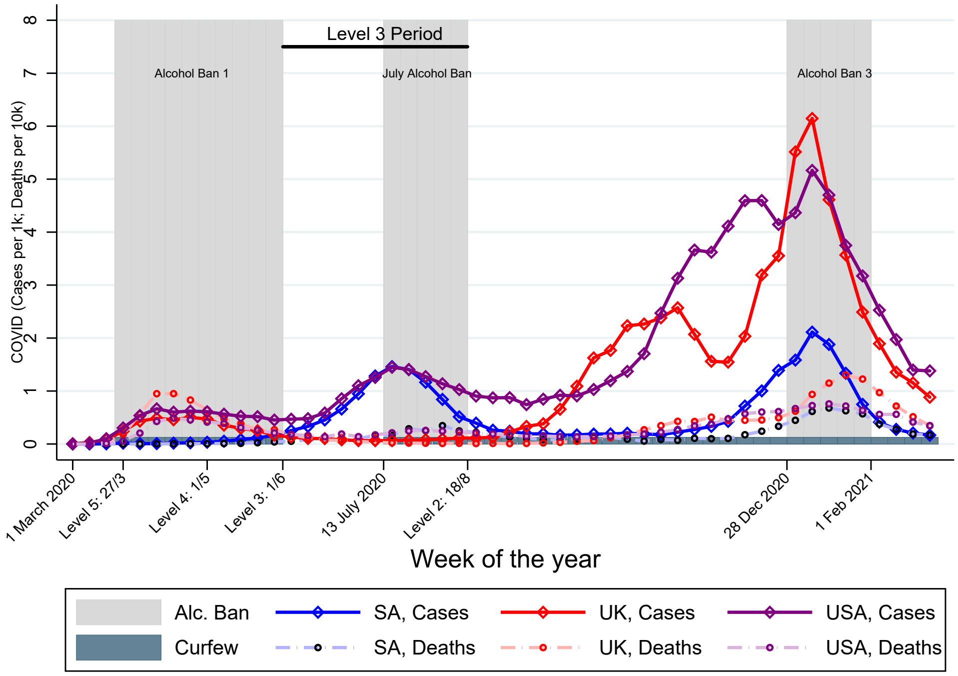


Figure 11: Comparing confirmed COVID-19 cases with unnatural mortality



Notes: (i) Source of COVID-19 data is the World Health Organization (https://covid19.who.int/), (ii) The red dashed horizontal lines in the figure in the top panel reflect the 5-week period averages before, during and after the July Alcohol Ban, (iii) The figure in the top panel is otherwise a replication of Figure 2 from the main text.

Figure 12: Weekly confirmed COVID-19 cases in South Africa, USA and the UK



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

C Robustness exercises

C.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 C.1.1 we consider the possibility that COVID-19 affected unnatural mortality through channels other than the alcohol ban. Second, in Section C.1.2 we discuss the possibility that other contemporaneous policy changes may have influenced unnatural mortality.

C.1.1 The role of COVID-19

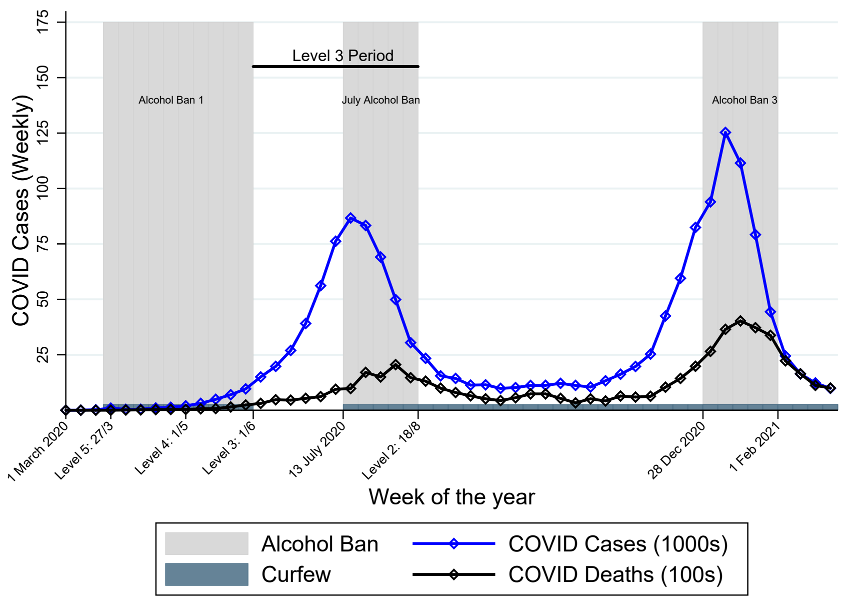
New policies are often a reaction to other events or changes in society. Therefore, the changes that precipitated the alcohol ban could also have caused the observed change in unnatural mortality rather than the alcohol ban being the causal factor itself. As indicated above, the stated aim of the alcohol ban was to reduce pressure on hospitals during the COVID-19 pandemic by reducing alcohol-related trauma admissions.[43] 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.[44] 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 13 reports the total number of weekly confirmed COVID-19 cases in South Africa over

[43]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).

[44]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).

Figure 13: Weekly confirmed COVID-19 cases in South Africa



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

time.[45] From this we see that the July Alcohol Ban was introduced when COVID-19 cases were already at their peak level. Thereafter, during the period in which the alcohol ban was in place, the number of new confirmed cases dropped rapidly. It seems unlikely that the level of fear was higher when the number of cases was falling (during the July Alcohol Ban) than when it was rising (before the July Alcohol Ban). In addition, if fear were the main driver of the change in unnatural mortality, we would not expect to see such a sharp drop at the start of the July Alcohol Ban and then a sharp rise at the end, rather we would expect to see a smoother decline and rise in unnatural mortality levels. We therefore view it as unlikely that fear of COVID-19 was responsible for much of the reduction in unnatural mortality in mid-July 2020 by directly influencing behavior.

[45] 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 13 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.

Table 16: Impact of the one hour curfew change on mortality (entire population)

	Men		Women	
	(1a)	(1b)	(2a)	(2b)
Level 3 Period = 1	2.03	1.98	-0.28	-0.30
(1/6-17/8)	(1.90)	(1.91)	(0.61)	(0.61)
Alcohol Ban Period = 1	0.46	2.84	0.44	1.15
(13/7-17/8)	(2.39)	(3.15)	(0.81)	(1.02)
Level 3 Period x Year=2020	0.40	0.41	-2.18*	-2.19*
	(3.77)	(3.77)	(1.19)	(1.19)
Alcohol Ban Period x Year=2020	-21.43***	-23.23***	-0.55	0.77
	(4.74)	(5.58)	(1.43)	(1.72)
Curfew Shortened Period = 1		-4.67		-1.38
		(3.46)		(1.21)
Curfew Shortened x Year=2020		3.58		-2.64
		(7.58)		(2.06)
Constant	100.89***	100.88***	29.87***	29.86***
	(3.58)	(3.57)	(1.34)	(1.33)
Weekend Controls	Y	Y	Y	Y
Day of Week FEs	Y	Y	Y	Y
Day of Month FEs	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y
Observations	1460	1460	1460	1460
Adjusted R^2	0.606	0.606	0.374	0.374

Notes: (i) Each observation contains unnatural mortality data for a single day, (ii) Heteroskedasticity robust standard errors (Huber, 1967; White, 1980, 1982) are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between January, 1 and December, 31 of each year, excluding February, 29, (iv) All columns report estimates 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 in Table 2, and specifications (*b) examine the influence of the curfew on unnatural mortality.

C.1.2 The role of the curfew

The second concern is that other policies may have changed at the same time as the July Alcohol Ban was introduced, 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 our 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.[46] 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. Being outside your home at night without a valid reason was also not legally permitted. Second, Figure 2 as well as our event study analyses show that unnatural mortality jumped up to pre-COVID levels when the July Alcohol Ban was lifted even though the curfew remained in place for several months. This suggests that the curfew did not reduce unnatural mortality levels. Third, 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.[47] 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 was effective in reducing mortality, one might expect this shift to a later start-time to be associated with an increase in mortality levels.

The regression results reported in Tables 16 and 17 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. These results, therefore, support our assertion that the curfew is unlikely to be the main driver of the reduction in mortality.[48]

[46] 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.

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

[48] 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).

Table 17: Impact of the one hour curfew change on mortality (15-34 years)

	Men		Women	
	(1a)	(1b)	(2a)	(2b)
Level 3 Period = 1	1.81	1.79	-0.75*	-0.75*
(1/6-17/8)	(1.32)	(1.32)	(0.40)	(0.40)
Alcohol Ban Period = 1	-1.36	-0.47	0.72	0.49
(13/7-17/8)	(1.64)	(2.13)	(0.50)	(0.55)
Level 3 Period x Year=2020	-0.30	-0.29	0.26	0.25
	(2.48)	(2.49)	(0.74)	(0.74)
Alcohol Ban Period x Year=2020	-11.78***	-13.03***	-1.64*	-0.70
	(3.21)	(3.91)	(0.91)	(1.06)
Curfew Shortened Period = 1		-1.75		0.46
		(2.30)		(0.68)
Curfew Shortened x Year=2020		2.50		-1.87
		(5.07)		(1.27)
Constant	46.85***	46.85***	11.56***	11.55***
	(2.09)	(2.09)	(0.67)	(0.67)
Weekend Controls	Y	Y	Y	Y
Day of Week FEs	Y	Y	Y	Y
Day of Month FEs	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y
Observations	1460	1460	1460	1460
Adjusted R^2	0.611	0.611	0.367	0.367

Notes: (i) Each observation contains unnatural mortality data for a single day, (ii) Heteroskedasticity robust standard errors (Huber, 1967; White, 1980, 1982) are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between January, 1 and December, 31 of each year, excluding February, 29, (iv) All columns report estimates 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 in Table 3, and specifications (*b) examine the influence of the curfew on unnatural mortality.

C.2 Additional robustness exercises

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

C.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 18 (all ages) and 19 (younger adults).

Table 18: Placebo regressions for impact of the alcohol ban on mortality entire population

	Men			Women		
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
Level 3 Period = 1	0.95	0.09	0.49	-0.33	-0.39	-0.21
(1/6-17/8)	(4.35)	(2.23)	(1.79)	(0.83)	(0.66)	(0.59)
Alcohol Ban Period = 1	-0.97	1.13	2.16	0.27	0.65	0.90
(13/7-17/8)	(5.85)	(2.77)	(2.28)	(1.17)	(0.86)	(0.81)
Level 3 Period x Year=2019	10.40	10.62**	5.75	2.33	1.80	-0.00
	(8.44)	(4.47)	(4.00)	(1.86)	(1.30)	(1.22)
Alcohol Ban Period x Year=2019	-3.69	-5.54	-5.54	-1.06	-1.23	-1.22
	(12.03)	(5.94)	(4.80)	(2.40)	(1.77)	(1.56)
Constant	107.48***	86.31***	102.00***	30.79***	27.06***	31.90***
	(1.32)	(0.91)	(3.01)	(0.30)	(0.25)	(1.12)
Weekend Controls		Y	Y		Y	Y
Day of Week FEs			Y			Y
Day of Month FEs			Y			Y
Year FEs			Y			Y
Observations	1460	1460	1460	1460	1460	1460
Adjusted R^2	-0.000	0.613	0.703	-0.001	0.384	0.477

Notes: (i) Each observation contains unnatural mortality data for a single day, (ii) Heteroskedasticity robust standard errors (Huber, 1967; White, 1980, 1982) are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2016 to 2019, between January, 1 and December, 31 of each year, excluding February, 29, (iv) All columns report estimates of the impact on unnatural mortality, and differ only in their specifications, with column (*a) the simplest specification, (*b) adding controls for the weekend, and (*c) adding fixed effects. The weekend controls include an indicator variable for a weekend day, an indicator for the first weekend of the month, and one for the last weekend of the month. They also include all three variables interacted with a 2020 indicator variable. In column (*c), the weekend day indicator is excluded due to the Day of Week FEs, (v) The table is essentially a replication of Table 2, except shifted one year backwards in time.

The results indicate that we observe no significant difference between mortality in 2019 and the preceding three years during the calendar period of the July alcohol sales ban. This helps to verify the validity of our results and alleviate concerns that they might be generated by the choice of empirical specification.

Table 19: 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	0.28	-0.45	-0.25	-1.21***	-1.29***	-1.24***
(1/6-17/8)	(2.80)	(1.39)	(1.20)	(0.45)	(0.37)	(0.36)
Alcohol Ban Period = 1	-0.62	0.75	1.17	0.66	0.87*	0.94*
(13/7-17/8)	(3.83)	(1.70)	(1.50)	(0.68)	(0.51)	(0.49)
Level 3 Period x Year=2019	6.17	7.30**	5.35*	2.22*	2.11**	1.63**
	(5.63)	(2.92)	(2.79)	(1.18)	(0.84)	(0.82)
Alcohol Ban Period x Year=2019	-4.12	-5.45	-5.41	-0.88	-0.99	-0.97
	(7.85)	(3.83)	(3.40)	(1.51)	(1.11)	(1.01)
Constant	54.64***	40.58***	48.41***	11.84***	9.77***	12.12***
	(0.88)	(0.59)	(1.96)	(0.16)	(0.13)	(0.62)
Weekend Controls		Y	Y		Y	Y
Day of Week FEs			Y			Y
Day of Month FEs			Y			Y
Year FEs			Y			Y
Observations	1460	1460	1460	1460	1460	1460
Adjusted R^2	-0.001	0.615	0.686	0.003	0.391	0.455

Notes: (i) Each observation contains unnatural mortality data for a single day, (ii) Heteroskedasticity robust standard errors (Huber, 1967; White, 1980, 1982) are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2016 to 2019, between January, 1 and December, 31 of each year, excluding February, 29, (iv) All columns report estimates of the impact on unnatural mortality, and differ only in their specifications, with column (*a) the simplest specification, (*b) adding controls for the weekend, and (*c) adding fixed effects. The weekend controls include an indicator variable for a weekend day, an indicator for the first weekend of the month, and one for the last weekend of the month. They also include all three variables interacted with a 2020 indicator variable. In column (*c), the weekend day indicator is excluded due to the Day of Week FEs, (v) The table is essentially a replication of Table 3, except shifted one year backwards in time.

C.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. As discussed in the main text of the paper, when examining the weekly and daily mortality patterns in Figures 2 and 15, 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 July Alcohol Ban in our analysis. This essen-

tially 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 20 and 21, considering time windows of between 5 weeks and 2 weeks in length.

Table 20: 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]	2.03	2.85	4.05*	3.30	10.93***	-0.28	0.05	0.12	0.10	0.79
	(1.90)	(2.07)	(2.43)	(2.88)	(3.45)	(0.61)	(0.68)	(0.75)	(0.83)	(0.99)
After Ban [t,t+X]	0.46	-0.27	-0.59	-0.94	-11.08**	0.44	0.32	0.53	0.12	-0.01
	(2.39)	(2.56)	(3.04)	(3.74)	(4.35)	(0.81)	(0.86)	(0.94)	(1.11)	(1.38)
Before & After Ban x Year=2020	0.40	-3.22	-5.18	-4.80	-15.06***	-2.18*	-2.84**	-3.44**	-3.58**	-4.09***
	(3.77)	(3.63)	(4.07)	(4.63)	(5.41)	(1.19)	(1.24)	(1.35)	(1.47)	(1.58)
After Ban x Year=2020	-21.43***	-17.17***	-19.69***	-19.76***	-1.16	-0.55	0.08	0.79	1.57	2.85
	(4.74)	(4.63)	(5.40)	(6.48)	(7.53)	(1.43)	(1.48)	(1.66)	(1.93)	(2.26)
Constant	100.89***	100.93***	100.72***	100.64***	100.26***	29.87***	29.82***	29.73***	29.58***	29.43***
	(3.58)	(3.58)	(3.53)	(3.51)	(3.49)	(1.34)	(1.33)	(1.32)	(1.32)	(1.31)
Weekend Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
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	1460	1460	1460	1460	1460	1460	1460	1460	1460	1460
Adjusted R^2	0.606	0.606	0.607	0.606	0.605	0.374	0.374	0.374	0.373	0.372

Notes: (i) Each observation contains unnatural mortality data for a single day, (ii) Heteroskedasticity robust standard errors (Huber, 1967; White, 1980, 1982) are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between January, 1 and December, 31 of each year, excluding February, 29, (iv) All columns report estimates for the outcome variable, unnatural mortality, following the specificaion (*c) from Table 2 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, (vi) The reason why the estimated coefficients in columns (*b) here differ slightly from those in columns (*c) of Table 7 is because here we use five weeks on each side of the start of the July Alcohol Ban as the treatment and control periods, while Table 7 includes the full 36 days of the July Alcohol Ban as the treatment period along with a 35 day control period.

Table 20 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. However, the results discussed in our event study analyses in Sections 5 and 6 suggest that this is not likely

to be the correct explanation since we observe a sharp immediate drop in our outcomes of interest upon the implementation of the July Alcohol Ban. Second, Figure 2 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.[49] While the fixed effects that we include in our analyses 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 positive coefficient in the *Before and After Ban* variable as well as a substantially larger negative coefficient on the *After Ban* variable 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. Therefore, the result for the narrowest time window seems to be driven by the downward slope in unnatural mortality in previous years generated by the change of month, rather than due to the July Alcohol Ban not having an influence on unnatural mortality. Overall, we view these results as providing support for the validity of our main estimates.

Table 21: 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]	1.81	2.18	2.88*	2.47	7.32***	-0.75*	-0.52	-0.51	-0.65	-0.13
	(1.32)	(1.45)	(1.72)	(1.94)	(2.35)	(0.40)	(0.45)	(0.52)	(0.58)	(0.73)
After Ban [t,t+X]	-1.36	-1.83	-2.60	-3.05	-9.43***	0.72	0.65	0.58	0.11	-0.20
	(1.64)	(1.77)	(2.11)	(2.51)	(2.88)	(0.50)	(0.54)	(0.62)	(0.68)	(0.85)
Before & After Ban x Year=2020	-0.30	-1.90	-2.67	-2.24	-7.89**	0.26	-0.67	-1.04	-0.68	-0.52
	(2.48)	(2.50)	(2.82)	(3.22)	(3.89)	(0.74)	(0.74)	(0.78)	(0.89)	(1.07)
After Ban x Year=2020	-11.78***	-9.71***	-11.32***	-11.04**	-2.58	-1.64*	-0.90	-0.35	-0.42	0.68
	(3.21)	(3.25)	(3.74)	(4.45)	(5.48)	(0.91)	(0.92)	(1.03)	(1.21)	(1.45)
Constant	46.85***	46.85***	46.63***	46.62***	46.52***	11.56***	11.61***	11.57***	11.50***	11.38***
	(2.09)	(2.09)	(2.08)	(2.07)	(2.06)	(0.67)	(0.66)	(0.65)	(0.66)	(0.65)
Weekend Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
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	1460	1460	1460	1460	1460	1460	1460	1460	1460	1460
Adjusted R^2	0.611	0.611	0.612	0.610	0.611	0.367	0.367	0.367	0.367	0.364

Notes: (i) Each observation contains unnatural mortality data for a single day, (ii) Heteroskedasticity robust standard errors (Huber, 1967; White, 1980, 1982) are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between January, 1 and December, 31 of each year, excluding February, 29, (iv) All columns report estimates for the outcome variable, unnatural mortality, following the specificaion (*c) from Table **??** 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.

[49] 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 weekend controls and 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. It is plausible that this imbalance when considering the two-week window that only includes a payday weekend in the control period, but not in the treatment period could be the reason for this result.

C.2.3 Alternative methods of inference

There are essentially three main types of potential interdependence in the random error terms that one might be concerned about when using an empirical strategy of the type that we consider in this paper: (i) there may be a correlation across individuals or geographical regions, (ii) there may be serial correlation, or (iii) there may be seasonal effects (i.e., correlations across years within a given calendar period). If the assumptions made about the correlational structure of the errors is incorrect, this can result in biased standard error estimates, leading to incorrect inference.

Table 22: Replication of Table 2 (Standard errors clustered at the calendar week level).

		Men			Women	
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
Level 3 Period = 1	5.86	2.68	2.03	1.02	0.32	-0.28
(1/6-17/8)	(6.97)	(3.89)	(2.80)	(1.40)	(0.85)	(0.58)
Alcohol Ban Period = 1	-1.99	-0.44	0.46	-0.19	0.16	0.44
(13/7-17/8)	(8.78)	(3.73)	(2.59)	(1.81)	(0.89)	(0.62)
Level 3 Period x Year=2020	-8.24	0.97	0.40	-5.22***	-3.26**	-2.18
	(7.13)	(5.18)	(5.74)	(1.69)	(1.25)	(1.50)
Alcohol Ban Period x Year=2020	-20.62**	-21.07***	-21.43***	-0.31	-0.48	-0.55
	(10.22)	(6.70)	(6.34)	(2.12)	(1.38)	(1.26)
Constant	106.54***	87.90***	100.89***	30.42***	27.24***	29.87***
	(2.76)	(2.53)	(4.74)	(0.60)	(0.55)	(1.45)
Weekend Controls		Y	Y		Y	Y
Day of Week FEs			Y			Y
Day of Month FEs			Y			Y
Year FEs			Y			Y
Observations	1460	1460	1460	1460	1460	1460
Adjusted R^2	0.008	0.535	0.606	0.009	0.306	0.374

Notes: (i) Each observation contains unnatural mortality data for a single day, (ii) Standard errors are clustered at the calendar week level to account for potential interdependence on the time dimension (it also accounts for interdependence across years for a particular week of the year). These 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, 1 and December, 31 of each year, excluding February, 29, (iv) All columns report estimates of the impact on unnatural mortality, and differ only in their specifications, with column (*a) the simplest specification, (*b) adding controls for the weekend, and (*c) adding fixed effects. The weekend controls include an indicator variable for a weekend day, an indicator for the first weekend of the month, and one for the last weekend of the month. They also include all three variables interacted with a 2020 indicator variable. In column (*c), the weekend day indicator is excluded due to the Day of Week FEs.

In most of our analysis in the main text, we adopt a conservative approach to addressing (i) by collapsing the data to a single observation for each day (e.g., the total number of injury-induced deaths observed on that particular day in the country as a whole). To account for (iii), we generally

include an array of time-related fixed effects in our preferred empirical specification.[50] In all of these analyses we report White-Huber heteroskedasticity robust standard errors (see, e.g., White, 1980).

Table 23: Replication of Table 2 (Newey-West Standard Errors).

	Men			Women		
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
Level 3 Period = 1	5.86	2.68	2.03	1.02	0.32	-0.28
(1/6-17/8)	(4.66)	(3.21)	(2.41)	(0.98)	(0.75)	(0.57)
Alcohol Ban Period = 1	-1.99	-0.44	0.46	-0.19	0.16	0.44
(13/7-17/8)	(5.69)	(3.52)	(2.42)	(1.18)	(0.82)	(0.69)
Level 3 Period x Year=2020	-8.24	0.97	0.40	-5.22***	-3.26**	-2.18
	(5.54)	(5.34)	(6.77)	(1.37)	(1.32)	(1.79)
Alcohol Ban Period x Year=2020	-20.62***	-21.07***	-21.43***	-0.31	-0.48	-0.55
	(6.97)	(5.58)	(5.58)	(1.68)	(1.41)	(1.40)
Constant	106.54***	87.90***	100.89***	30.42***	27.24***	29.87***
	(2.02)	(1.68)	(4.76)	(0.50)	(0.42)	(1.70)
Weekend Controls		Y	Y		Y	Y
Day of Week FEs			Y			Y
Day of Month FEs			Y			Y
Year FEs			Y			Y
Observations	1460	1460	1460	1460	1460	1460

Notes: (i) Each observation contains unnatural mortality data for a single day, (ii) Newey-West (1987) standard errors reported in parentheses, accounting for an error structure that is assumed to be heteroskedastic and potentially correlated up to a lag of 9 days * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between January, 1 and December, 31 of each year, excluding February, 29, (iv) All columns report estimates of the impact on unnatural mortality, and differ only in their specifications, with column (*a) the simplest specification, (*b) adding controls for the weekend, and (*c) adding fixed effects. The weekend controls include an indicator variable for a weekend day, an indicator for the first weekend of the month, and one for the last weekend of the month. They also include all three variables interacted with a 2020 indicator variable. In column (*c), the weekend day indicator is excluded due to the Day of Week FEs.

One remaining concern is that there may exist an unaccounted for serial correlation in the errors.[51] To address this potential issue, we have conducted several additional exercises that reproduce the results reported in Table 2, with each exercise adopting a different approach to calculating the standard errors by allowing for some form of interdependence on the temporal dimension. Below we report the results from each of these exercises.[52] First, in Table 22, we cluster the standard

[50] In Section 6 we also report the results from an empirical strategy that only uses data from 2020. In this empirical specification, we avoid the issue of across-year seasonal interdependencies influencing our standard error calculations, since we are only considering a single year. This provides additional support for the conclusion that our results are not driven by the influence of unaccounted for seasonal correlations in our standard error calculations.

[51] It is worth noting that it is unclear from an a priori perspective whether one might expect this correlation to be negative or positive. Mortality as an outcome is different from many other behavior-induced outcomes because a single individual can only die once. Therefore, a change in the underlying determinants of injury-induced mortality that leads to more mortality today might also result in more mortality tomorrow (through the same underlying process) or less mortality tomorrow (since the people who might have died tomorrow already died today). In addition, when considering mortality as an outcome, other typical mechanisms that result in persistent changes in behavior, such as habit formation, are less relevant due to the once-off nature of dying.

[52] In the interest of brevity, we have not reported the results from conducting these exercises for the other regressions in the main text here. These results are available upon request.

errors at the calendar-week level. This allows for interdependence within a given week of a specific year, helping to address (ii). It also allows for interdependence across years within a particular calendar week, providing an additional way to account for (iii). Second, we use the Newey-West (1987) variance estimator, which allows for autocorrelation up to a lag of a certain pre-specified length. In Table 23, we replicate the full set of results of Table 2 for a lag length of 9 days. In Table 24, we reproduce our preferred specification from Table 2, namely column (*c), for different lag lengths ranging from 0 to 9 days.[53] Third, we take the most conservative approach to addressing interdependence on the temporal dimension and cluster at the year level, using the wild cluster bootstrap to correct for the small number of clusters (Cameron and Miller, 2015). This is implemented using the 'boottest' command in STATA (Roodman et al., 2019) and provides a p-value for the coefficient of interest. This exercise provides an estimate of $p < 0.01$ using Rademacher weights and $p = 0.06$ using Webb (2013) weights for the corrected standard errors for the coefficient of interest, namely **Alcohol Ban Period x Year=2020**, from our preferred specification (1c) from Table 2.[54]

Collectively, the results of this set of exercises are highly consistent with the conclusions drawn in the main text, thereby providing support for the assertion that the statistically significant effect that we find is not generated by the specific assumptions that we have made in calculating our standard errors.

[53] Stock et al. (2012) argue that there is a tension between including too few and too many lags and on p. 599 propose a rule-of-thumb formula (equation 15.17) for calculating the number of lags that optimally balances this tension. In our context, using this formula with T=1460 gives a suggested lag length of 8.51, while using T=365 gives a suggested lag length of 5.36. Both lie within the range of lag lengths that we consider, and provide estimates that are consistent with the main results.

[54] In the interest of completeness, the corresponding estimates for specification (2c) from Table 2 (i.e., the coefficients for the sample of women) are $p = 0.72$ using Rademacher weights and $p = 0.73$ using Webb (2013) weights. It is unsurprising that the coefficient of interest for women is not statistically significant after the standard error correction, since it is also not statistically significant prior to the correction procedure.

Table 24: Replication of col (*c) of Table 2 (Newey-West Standard Errors, varying lag length).

	Men						Women					
	M.Lag(0)	M.Lag(1)	M.Lag(3)	M.Lag(5)	M.Lag(7)	M.Lag(9)	F.Lag(0)	F.Lag(1)	F.Lag(3)	F.Lag(5)	F.Lag(7)	F.Lag(9)
Level 3 Period = 1	2.03	2.03	2.03	2.03	2.03	2.03	-0.28	-0.28	-0.28	-0.28	-0.28	-0.28
(1/6-17/8)	(1.90)	(2.17)	(2.37)	(2.46)	(2.44)	(2.41)	(0.61)	(0.64)	(0.63)	(0.60)	(0.59)	(0.57)
Alcohol Ban Period = 1	0.46	0.46	0.46	0.46	0.46	0.46	0.44	0.44	0.44	0.44	0.44	0.44
(13/7-17/8)	(2.39)	(2.71)	(2.80)	(2.64)	(2.49)	(2.42)	(0.81)	(0.84)	(0.78)	(0.74)	(0.72)	(0.69)
Level 3 Period x Year=2020	0.40	0.40	0.40	0.40	0.40	0.40	-2.18*	-2.18*	-2.18	-2.18	-2.18	-2.18
	(3.77)	(4.48)	(5.28)	(5.85)	(6.34)	(6.77)	(1.19)	(1.28)	(1.45)	(1.57)	(1.69)	(1.79)
Alcohol Ban Period x Year=2020	-21.43***	-21.43***	-21.43***	-21.43***	-21.43***	-21.43***	-0.55	-0.55	-0.55	-0.55	-0.55	-0.55
	(4.74)	(5.47)	(5.73)	(5.42)	(5.44)	(5.58)	(1.43)	(1.51)	(1.57)	(1.49)	(1.44)	(1.40)
Constant	100.89***	100.89***	100.89***	100.89***	100.89***	100.89***	29.87***	29.87***	29.87***	29.87***	29.87***	29.87***
	(3.58)	(3.72)	(3.99)	(4.28)	(4.52)	(4.76)	(1.34)	(1.37)	(1.47)	(1.56)	(1.63)	(1.70)
Weekend Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Day of Week FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Day of Month FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1460	1460	1460	1460	1460	1460	1460	1460	1460	1460	1460	1460

Notes: (i) Each observation contains unnatural mortality data for a single day, (ii) Newey-West (1987) standard errors reported in parentheses, accounting for an error structure that is assumed to be heteroskedastic and potentially correlated up to a lag of x days, where the lag length is varied across columns and indicated in the column header * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between January, 1 and December, 31 of each year, excluding February, 29, (iv) All columns report estimates of the impact on unnatural mortality, and differ only in their specifications, with column (*a) the simplest specification, (*b) adding controls for the weekend, and (*c) adding fixed effects. The weekend controls include an indicator variable for a weekend day, an indicator for the first weekend of the month, and one for the last weekend of the month. They also include all three variables interacted with a 2020 indicator variable. In column (*c), the weekend day indicator is excluded due to the Day of Week FEs.

C.2.4 Restricting the data to only the Level 3 period

Fourth, 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:

$$M_{y,t,g} = \alpha_0 + \alpha_1 \cdot T_{y,t} + \beta \cdot T_{y,t} \times Y_{2020} + \boldsymbol{\lambda}_{y,t} + \epsilon_{y,t,g} \tag{6}$$

where $M_{y,t,g}$ refers to the number of daily unnatural deaths in year y on day-of-the-year t in group g (i.e. for a specific gender or age group). 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 weekend controls and 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 25 (all ages) and 26 (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 25: 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	0.07	-2.46	0.38	1.12	0.91	0.49
(13/7-17/8)	(5.35)	(2.52)	(2.09)	(1.08)	(0.83)	(0.82)
Alcohol Ban Period x Year=2020	-28.86***	-13.65***	-21.73***	-5.53***	-3.22***	-0.68
	(4.98)	(3.06)	(4.01)	(1.19)	(1.05)	(1.34)
Constant	110.35***	89.61***	111.16***	30.13***	27.00***	29.37***
	(3.28)	(1.68)	(4.94)	(0.70)	(0.54)	(2.29)
Weekend Controls		Y	Y		Y	Y
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	0.043	0.776	0.859	0.029	0.447	0.554

Notes: (i) Each observation contains unnatural mortality data for a single day, (ii) Heteroskedasticity robust standard errors (Huber, 1967; White, 1980, 1982) are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between June, 1 and August, 17 of each year, (iv) All columns report estimates of the impact on unnatural mortality, and differ only in their specifications, with column (*a) the simplest specification, (*b) adding controls for the weekend, and (*c) adding fixed effects. The weekend controls include an indicator variable for a weekend day, an indicator for the first weekend of the month, and one for the last weekend of the month. They also include all three variables interacted with a 2020 indicator variable. In column (*c), the weekend day indicator is excluded due to the Day of Week FEs.

Table 26: Impact on mortality of 15-34 year olds (only considering the Level 3 period)

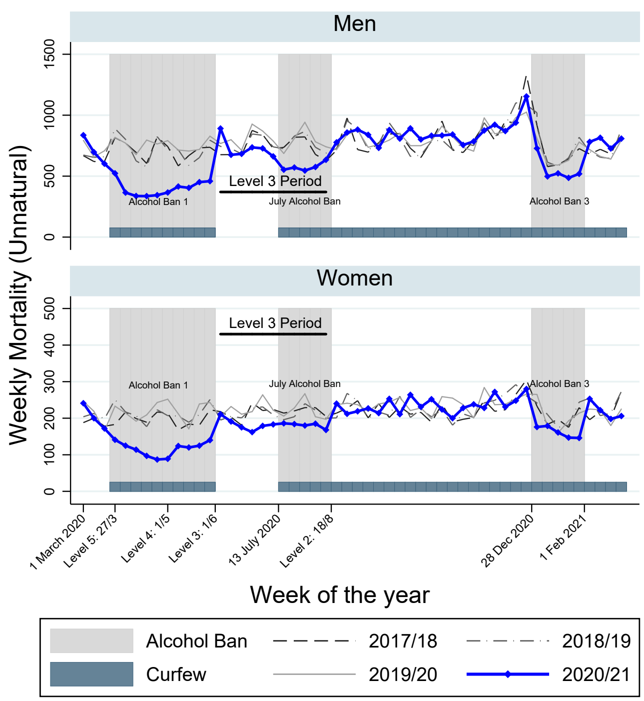
	Men			Women		
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
Alcohol Ban Period = 1	-0.96	-2.70*	-1.52	0.68	0.49	0.82
(13/7-17/8)	(3.45)	(1.59)	(1.49)	(0.64)	(0.51)	(0.51)
Alcohol Ban Period x Year=2020	-18.23***	-7.93***	-11.89***	-2.67***	-1.15*	-1.69*
	(3.07)	(1.75)	(2.60)	(0.67)	(0.65)	(0.88)
Constant	55.63***	42.15***	49.55***	11.10***	9.40***	11.96***
	(2.16)	(1.01)	(3.31)	(0.40)	(0.32)	(1.46)
Weekend Controls		Y	Y		Y	Y
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	0.043	0.791	0.849	0.018	0.394	0.501

Notes: (i) Each observation contains unnatural mortality data for a single day, (ii) Heteroskedasticity robust standard errors (Huber, 1967; White, 1980, 1982) are reported in parentheses $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$, (iii) The estimation uses data from 2017 to 2020, between June, 1 and August, 17 of each year, (iv) All columns report estimates of the impact on unnatural mortality, and differ only in their specifications, with column (*a) the simplest specification, (*b) adding controls for the weekend, and (*c) adding fixed effects. The weekend controls include an indicator variable for a weekend day, an indicator for the first weekend of the month, and one for the last weekend of the month. They also include all three variables interacted with a 2020 indicator variable. In column (*c), the weekend day indicator is excluded due to the Day of Week FEs.

D Empirical regularities in unnatural mortality in South Africa

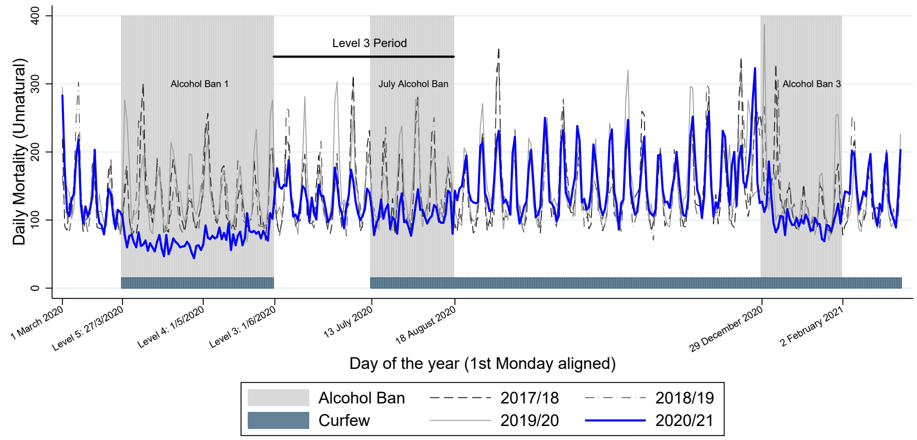
In order to study the change in unnatural mortality generated by the alcohol ban, it is important to understand the baseline patterns present in the mortality data. In this section we document several important regularities observed in the data. First, the number of daily deaths due to unnatural causes is markedly different for men and women. Figure 14 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 14: Weekly mortality (unnatural deaths, by gender)



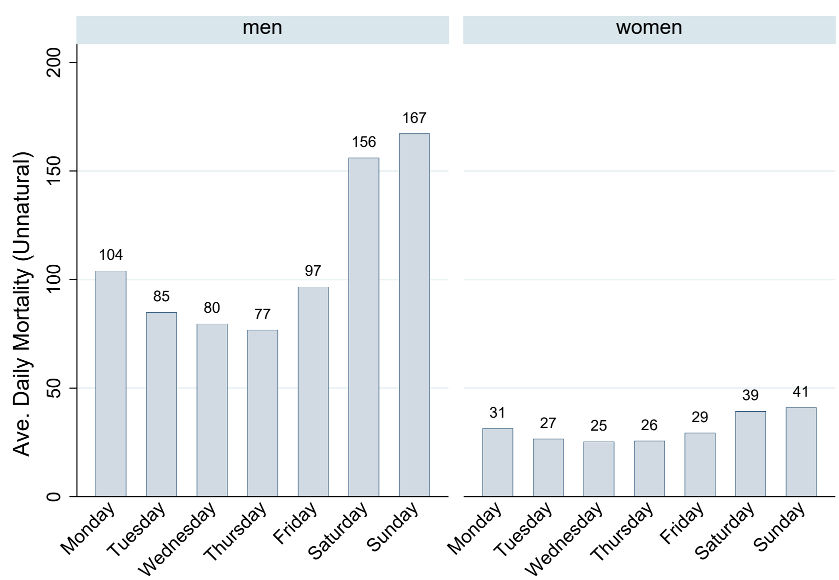
Second, unnatural mortality in South Africa follows a strong and systematic weekly pattern. Figure 15 shows this by reporting daily mortality levels, aligning days of the week across the four years by counting from the first Monday of the year. The peaks at regular intervals in the 2017, 2018, and 2019 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 15: Daily mortality (March 2017 to March 2021)



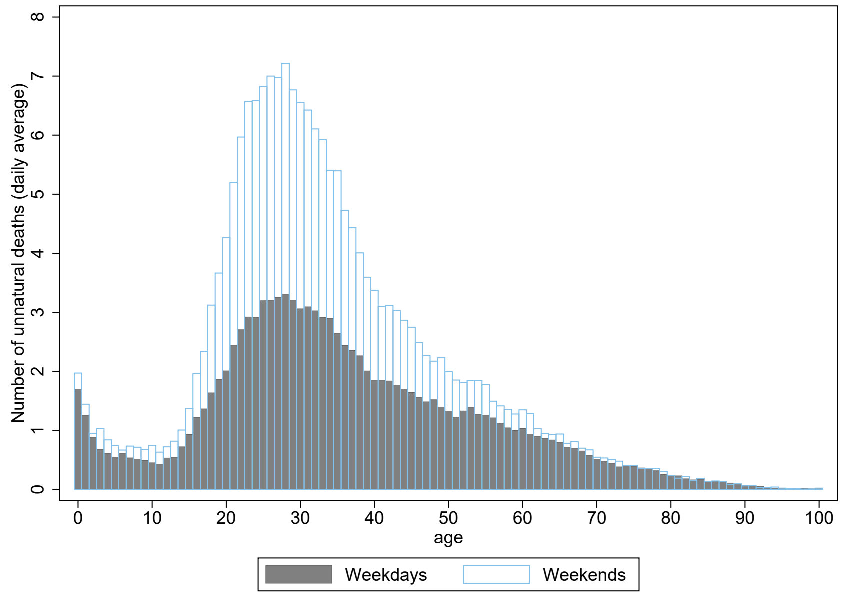
This weekly cyclical pattern is also clearly seen in Figure 16, which shows that mortality is at least 50% higher on Saturdays and Sundays for men, and at least 25% higher for women.[55] Further, Figure 17 shows that this weekend effect is strongest for individuals aged between 20 and 40 years old, who display a sharp increase in unnatural mortality on weekends.

Figure 16: Ave. Daily mortality by day of the week (by gender, 2017-2019)



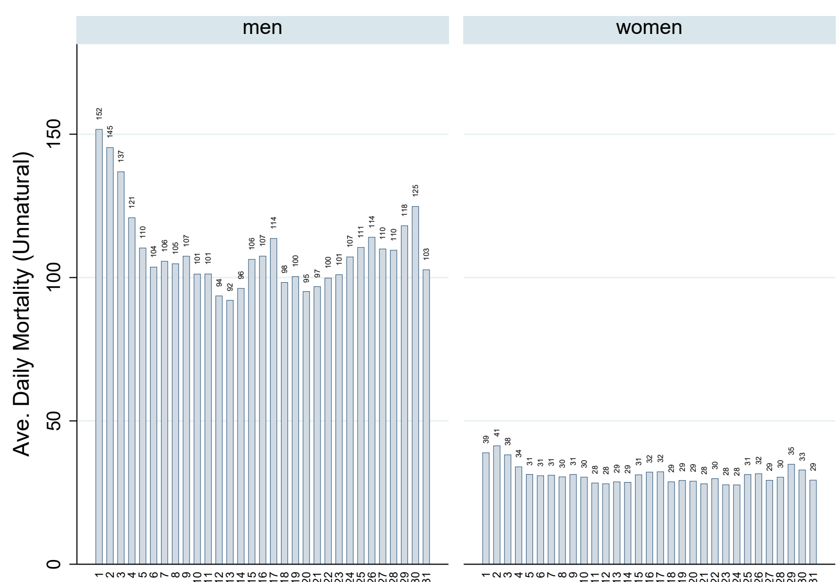
[55]Note, one reason for the higher mortality levels recorded on Saturday and Sunday (as opposed to Friday and Saturday) is that deaths that result from injuries obtained during Friday [Saturday] night are often recorded on Saturday [Sunday].

Figure 17: Age distribution of unnatural mortality, weekdays and weekends (2017-2019)



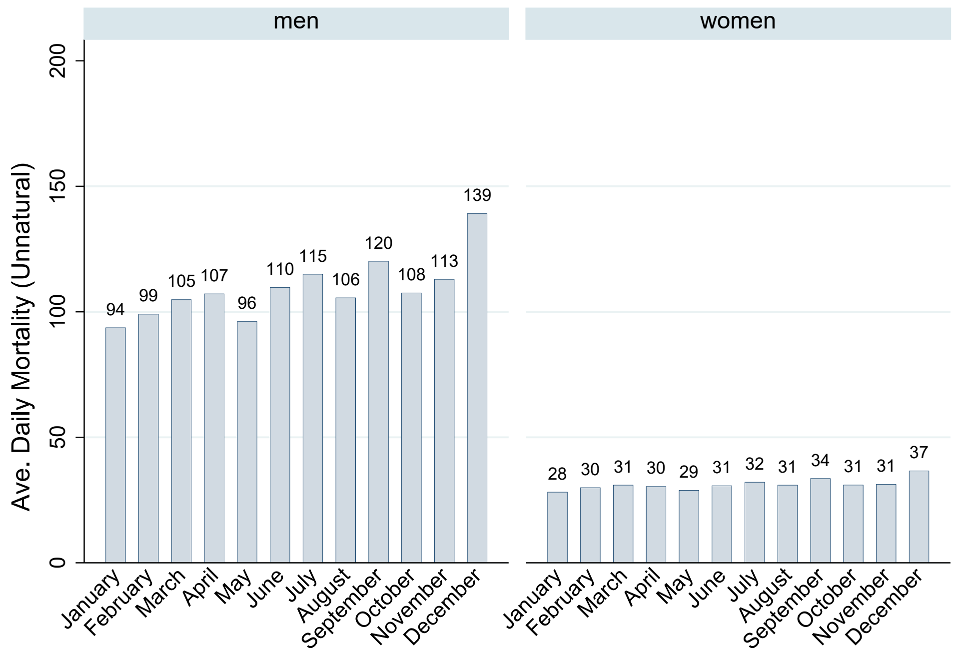
Third, Figure 18 shows that there is also variation in mortality according to the day of the month, with higher mortality levels observed at the beginning and end of the month. One potential explanation for these monthly peaks is that they are associated with wage payment days.

Figure 18: Ave. Daily mortality by day of the month (by gender, 2017-2019)



Fourth, Figure 19 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.[56]

Figure 19: Ave. Daily mortality by month of the year (by gender, 2017-2019)

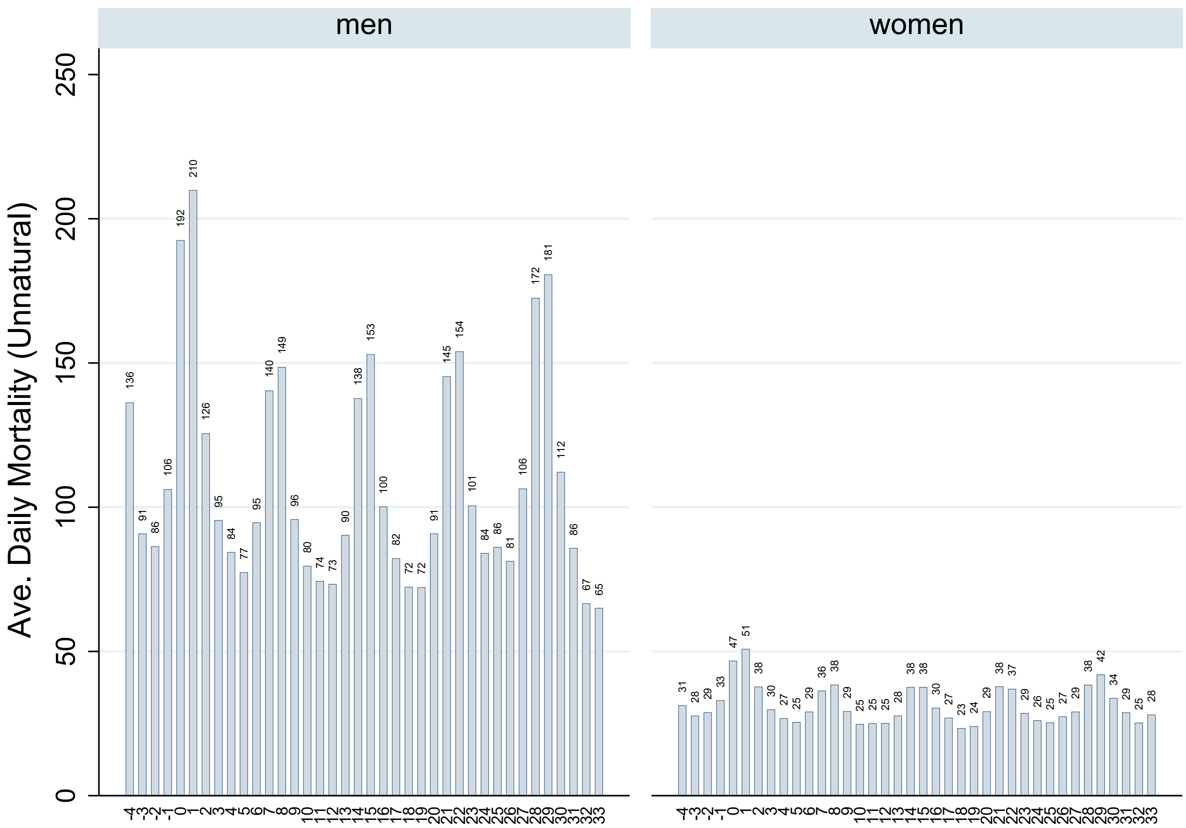


Last, to illustrate the influence of weekends and the monthly regularity in mortality together, Figure 20 aligns the days of the week in different months by counting the first Saturday that follows the last Monday of the previous month as day $t = 0$. For example, if the first day of a month is a Sunday, this is counted as day $t = 1$, but if the first day of the month is a Friday, it is counted

[56]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. Detailed information on other dimensions of alcohol consumption regularities is provided in, e.g., Shisana et al. (2013); Probst et al. (2017) and WHO (2019).

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.[57]

Figure 20: Ave. Daily mortality by day of the month (aligned by day of the week; 2017-2019)



[57]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.

Discussion Papers of the Research Area Markets and Choice 2022

Research Unit: **Economics of Change**

Kai Barron, Charles D.H. Parry, Debbie Bradshaw, Rob Dorrington, Pam Groenewald, Ria Laubscher, and Richard Matzopoulos SP II 2022-301

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