Check for updates

Received: 12 September 2022 | Revised: 18 September 2023 | Accepted: 19 September 2023

DOI: 10.1002/hec.4769

Health Economics WILEY

RESEARCH ARTICLE

The effect of gun buy-back law reform on homicides and suicides in Australia

Patrick Duenow[1,2] | **Luke B. Connelly**[2,3]

[1]School of Economics, The University of Queensland, Saint Lucia, Queensland, Australia

[2]Centre for the Business and Economics of Health, The University of Queensland, Saint Lucia, Queensland, Australia

[3]Department of Sociology and Business Law, The University of Bologna, Bologna, Italy

Correspondence
Patrick Duenow.
Email: patrick.duenow@uqconnect.edu.au

Abstract

In this paper we use the synthetic control method (SCM) to estimate the causal effects of a national legislative reform accompanied by mandatory gun buy-backs in Australia on both suicide and homicide rates. Using a rich international dataset, we are able to separate not only these two death types, but also to distinguish deaths by firearm and by other means, thereby enabling us to test substitution-of-means hypotheses. Specifically, we apply the SCM to determine whether any reductions in firearm-related death rates where wholly or partly offset by increases in the use of other means (e.g., bladed weapons, poisons) to commit suicides and perpetrate homicides. Our findings show that these gun control policies substantially reduced both homicides and suicides by firearm, but also some evidence of other-means substitution.

KEYWORDS
buyback, gun laws, homicide, mortality, suicide

JEL CLASSIFICATION
I18, K14

1 | INTRODUCTION

Some debates about gun control are inherently normative in nature and involve trade-offs between various individual rights and freedoms, and public safety. Nevertheless, some arguments that are advanced in gun control debates are empirically testable, even if the evidence to date has been fairly mixed. These include the stated reasons that greater gun prevalence may be associated with either higher or lower rates of violent crime. Proponents of gun control, for instance, may argue that lowering gun prevalence will lead to less violent crimes ending in deaths. The reasons advanced for this argument are that fewer criminals will have guns, and less access to guns in households also lowers the chance of successful suicide attempts. Opponents of gun control commonly counter-argue that lower gun prevalence has the effect of improving the odds for criminals that potential victims will be unarmed: this is predicted to give rise both to more frequent and to more deadly violent crime.

In this paper, we study the effect of a gun buyback policy (and related legislation) that was initiated in Australia in 1996, following a mass shooting at the Port Arthur historical site in Tasmania. The purpose of this policy was to prevent further mass shootings: it aimed to do this by removing semi-automatic rifles and semi-automatic or pump-action shotguns from the populous. Indeed, since these changes were enacted, Australia has had no mass shootings (Reuters, 2021). In this respect, the gun buyback does seem to have achieved its purpose. The question of whether the policy has had wider-reaching effects by reducing

This is an open access article under the terms of the Creative Commons Attribution-NonCommercial-NoDerivs License, which permits use and distribution in any medium, provided the original work is properly cited, the use is non-commercial and no modifications or adaptations are made.

© 2023 The Authors. Health Economics published by John Wiley & Sons Ltd.

gun deaths generally, though, is - despite a number of empirical studies of the question to date - still largely unresolved. In this paper, we therefore seek to investigate whether or not Australia's gun buyback policy affected homicide and suicide rates. We seek to determine whether these types of deaths, whether by firearm, or by all external causes (i.e., by both firearms and other means) fell as a result of the policy. This paper thus contributes to an international literature on gun control and its disputed effects. In relation to gun buybacks, in particular, much of that literature, which we review below, suggests that these initiatives are not an effective way of reducing firearm deaths in the population.

1.1 | The economics of gun control: Theory and evidence

The notions that gun control could either increase or decrease violence are supported by economic theory: Taylor (1995) develops a game-theoretic model in which gun control may either increase or decrease social welfare and Marceau (1997) produces a model in which the level of violence in a community may have multiple equilibria, so the effect of gun control on violent crime is ambiguous. Oliveira and Neto (2015) draw similar conclusions about the ambiguity of outcome of gun control on violence that depends on whether there is perfect or imperfect information. More fundamentally, the individual and collective tensions over gun control have been cast in the framework of the Prisoner's Dilemma (Mueller, 1979), in which society, collectively, would benefit from a reduction of criminals' access to guns, but individuals are nonetheless reluctant to cede their own weapons for fear that collective security measures (e.g., provided by the armed forces) either are incomplete, or at risk of failure (see, e.g., McDowall & Wiersema, 1995).

Empirical research on this topic has, historically, also lent support to both sides of the argument. In an early review of the evidence, Kleck and Patterson (1993) observed that "…taking prior research as a whole, it would be fair to say at this point that a consistent, credible case for gun control efficacy in reducing violence has not yet been made". As Duggan (2001) later noted, one reason that empirical work at that time was inconclusive was a reliance on methods and measures that were not entirely convincing. One branch of that literature used time-series estimates of the gun stock and crime rates at the national level (see, e.g., Kleck & Patterson, 1993, or Medoff & Magaddino, 1983), in the United states. This design was largely motivated by a paucity of reliable data on gun prevalence at sub-national levels. The second, more sophisticated, branch of literature attempted to estimate the prevalence of gun possession at sub-national (e.g., state, city, county) levels and then relate the gun stock to crime rates cross-sectionally (see, e.g., Cook, 1982, or Kleck & Patterson, 1993). The results of this literature were also mixed, with some studies purporting to show that higher gun prevalence reduced crime, and others that it increased crime. Both types of studies suffered from serious limitations, though, according to Duggan (2001) due to (i) the limited number of national observations available for the time-series work (and the high level of aggregation), (ii) the use of unreliable proxies of gun prevalence in sub-national studies, and (iii) the failure to rule out reverse causality or omitted variable bias as drivers of the main results.

One noteworthy spike in the academic literature on gun control was associated with the work of Mustard and Lott (1997), who purported to show that concealed carry weapons legislation caused a significant drop in violent crime in the United States. A number of subsequent studies were conducted to test these results (Ayres & Donohue, 1999; Black & Nagin, 1998; Ludwig, 1998 as cited in Duggan, 2001; Moody, 2001). Some of these studies produced results that supported the central conclusions of Mustard and Lott (1997), but other studies showed that the findings were sensitive to the econometric specification that was chosen. Duggan (2001) subsequently developed a new measure of gun prevalence in the US that was based on the sales of Guns and Ammo magazine and showed, quite convincingly, that the results obtained by Mustard and Lott (1997) were inaccurate. He found that increases in gun ownership led to substantial increases in the overall homicide rate and, importantly, that this was "…driven entirely by a relationship between firearms and homicides in which a gun is used, implying that the results are not driven by reverse causation or by omitted variables" (p.1112).

Turning specifically to the literature on gun buyback programs (GBPs), most of the international literature, despite claims to the contrary (see e.g., Hazeltine et al., 2019), shows that this type of policy has either a zero, or a very small, effect on either death rates or violent crimes. For instance, Callahan et al. (1994) studied the 1992 buyback in Seattle and found no statistically-significant effect of the policy on deaths or injuries (which decreased) and the study by Rosenfeld (1995) of the 1991 and 1994 St Louis buybacks also showed no significant effect on violent crime. Braga and Wintemute (2013), however, found that Boston's 2006 Operation Ceasefire program, which paid gun owners USD200 per firearm and resulted in the submission of 1019 handguns, was followed by a 30% decline in shootings over the following 4 years. The most comprehensive study of GBPs in the United States was conducted by Ferrazares et al. (2021). This study, which also used an synthetic control method (SCM) approach, focused on National Incident Reporting System data from 1995 to 2015 to study gun buybacks in the United States. Their results indicate that (i) in 29 out of 38 cases, the hypothesis of zero effect of gun buybacks on gun crime cannot be

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

rejected at the five per cent level in the year following buyback; (ii) when *p*-values are set at 0.05 in permutation tests, seven of these cases yielded increases in gun crime; and (iii) for only two cities (Cincinnati and Columbus, Ohio) was there a significant decline in gun crime following a buyback program. In both of the latter instances, though, the results become statistically insignificant when a second year of post-treatment data is included. The authors therefore conclude that "…U.S. GBPs have been ineffective at deterring gun crime, firearm-related homicides, or firearm-related suicides in the short- or long-run" (Ferrazares et al., 2021, p. 22).

1.2 | Literature on the Australian gun buyback

Australia's 1996 GBP followed a massacre at Port Arthur, Tasmania, in which a lone gunman killed 35 people and injured another 23 people. The murders were perpetrated with an AR-15 automatic rifle and a semi-automatic 0.308 FN rifle (manufactured by FN Herstal), both of which were legal in Australia at the time. Twelve days later, the Australian Prime Minister announced a sweeping package of gun reforms, subsequently enacted as the National Firearms Act (NFA), which sought to remove "military-style" automatic and semi-automatic firearms from Australian citizens. The NFA had the effect of making the possession of such weapons illegal, following a moratorium under which current owners of the nominated weapons could surrender their weapons to designated authorities and receive monetary compensation. Approximately 650,000 guns were peacefully returned and destroyed under the program (Beauchamp, 2018).

The effects of the NFA on gun deaths has already been studied extensively in the empirical literature, but the results of this literature have also been mixed. Further work on the Australian buyback is warranted for several reasons. First, the existing literature, which has produced mixed evidence of its effectiveness, suffers from numerous methodological shortcomings that may be overcome using the SCM approach. Second, very high quality data on disaggregated time-series are now available, via the Institute for Health Metrics and Evaluation (IHME). We are able to exploit those data in this paper and apply the SCM approach to their analysis. Third, while the purpose of the Australian legislative change was to prevent further massacres, there is also evidence that gun availability could affect the suicide rate (see, e.g., Balestra, 2018), and this notion has not been investigated in the extant literature on the Australian buyback. Finally, the Australian intervention differs in some important ways from buybacks in some other jurisdictions (Leigh & Neill, 2010), such as areas within the United States. Specifically, Australia is an island nation that is subject to a "hard border": this renders cross-border trade in restricted firearms difficult to achieve, and illegal to conduct. If gun buybacks were shown to their objectives under these circumstances, other nations with non-porous borders may also find gun buybacks to be effective, ceteris paribus.

The first empirical studies of the NFA's effect (Baker & Mcphedran, 2007; Chapman et al., 2016; Lee & Suardi, 2010; Reuter & Mouzos, 2003) were based on time-series analysis. Leigh and Neill (2010) subsequently used panel data methods to exploit variations in the number of firearms that were surrendered in Australia's states and territories and found large and statistically significant effects on suicide-by-firearm rates. Chapman et al. (2016), however, found that the results produced by Leigh and Neill (2010) are also detectable for non-firearm deaths suggesting either important spill-over effects or that their research design does not successfully isolate the causal effect of the GBP.

Taylor and Li (2015) adopted a difference-in-differences (DiD) approach, treating the unarmed robbery and sexual assault rates as placebo or control groups and testing these against two treatment groups: the armed robbery and attempted murder rates. They found that the NFA led to reductions in the armed robbery and attempted murder rates, relative to sexual assaults and unarmed robbery. A limitation of their study is that their panel of Australian states and territories commenced in 1993, which is very close to the implementation date of the NFA, in 1996.

Bartos et al. (2019) adopted an SCM design to study the effect of the NFA on overall homicides and suicides and used motor vehicle fatalities to conduct placebo tests. Their study uses data from the Mortality Database provided by the World Health Organisation (WHO). The utilised data includes Australia and 28 donor nations with multiple imputations to impute missing data-points. They report a large and statistically significant effect of the policy on homicides, no effect on the placebo (motor vehicle fatalities), and "…only an idiosyncratic effect" on suicides. Their analysis is limited to aggregate level suicides and homicides and as such did not allow for the study of potential substitution effects. The data we employ in our study differs from the data in Bartos et al. (2019) in several important ways. We discuss these differences in great detail in the following sections.

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

2 | DATA AND METHODS

To identify the causal effect of stricter gun laws on suicide and homicide rates, we exploit the substantial Australian firearm buyback program that was initiated in 1996. In a traditional econometric approach such as the Difference-in-Differences method, we would look for a treatment and a control unit and compare pre-intervention differences with post-intervention differences between both units. In an optimal setting, the treatment and control would behave identically in the absence of an intervention. Such a control unit is, however, usually challenging to find in a non-experimental environment. Thus, we utilize an alternative approach to traditional DiD, namely the SCM following Abadie and Gardeazabal (2003) and Abadie et al. (2010, 2015). This approach has, due to its desirable properties, now become a popular tool for testing the effects of policy on outcomes of interest. Following the SCM approach, instead of trying to find a single real-world control group (i.e., a country i.e., comparable to Australia), we derive an algorithmically-determined control that takes the form of a weighted average of donor units from a large donor pool. The donor weights are chosen such that the pre-intervention differences in the variables of interest between the treatment and control units are minimized. The trends of the resulting synthetic control unit (i.e., “synthetic Australia”) are then compared to the historical trends of the suicide and homicide rates that were actually witnessed in the treatment unit (i.e., Australia), following the policy change. In this study, we also apply an SCM design, but our application of it differs substantively to that of Bartos et al. (2019) and it yields quite different results. The most important differences between our study and Bartos et al. (2019) is that we use data from the Global Health Data Exchange (GHDx), supplied by the IHME, at The University of Washington, which is disaggregated into (i) homicide-by-firearm (ii) homicide-by-other-means (i.e., non-firearm); deaths due to homicide; (iii) suicide-by-firearm; and (iv) suicide-by-other-means. This enables us to test hypotheses not only the effects of the NFA on firearm-related deaths, but also on substitution-of-means hypotheses both in respect of homicide and suicide. Specifically, we ask whether or not the NFA decreased suicides and homicides by firearm, and also whether or not there was any increase in suicides or homicides via other means. Thus, our work is able to answer questions that previous work has been unable to answer and thereby provides greater insight into the causal effects of the NFA on four outcomes of interest.

2.1 | Deaths data and measurement error

In this study, we use data assembled by the Global Burden of Disease (GBD) Cause of Death (CoD) Collaborators (2017) to study the impact of the Australian gun buy-back program on homicides and suicides. In this section we provide a description of how these data are assembled and our rationale for preferring these data for the purposes of this study. Our primary reason for using these data (which are generally made available for CoD data, by country, via the IHME's GHDx website) is that there are known sources of measurement error associated with CoD data and the data series we use were assembled with the express purpose of minimizing that measurement error. In this section we provide a brief explanation of the main measurement error problems that the CoD collaborators have endeavored to address with the datasets we use in this study. It is important to understand that the main source of measurement error in mortality data, reported by developed countries, is not that some deaths are “missing” from the collection. Rather, the problem is generally that some deaths are misclassified, that is, are attributed to incorrect causes. To understand why this happens, it is useful to remark that, under the International Classification of Diseases system, only one CoD may be recorded in the collection of mortality statistics (Johnson et al., 2021). In principle, that CoD should correspond to the underlying cause of death (UCoD), meaning “the disease or injury that initiated the chain of events leading to death” (Johnson et al., 2021, p. 3). Yet a number of factors, including inadequate clinician training, lead to death certificates being incorrectly completed. This results in many deaths being ascribed “garbage codes”, which are codes that do not identify the true CoD. Cause of Death codes are labeled “garbage codes” when they constitute either an intermediate (rather than underlying), or impossible, CoD (Johnson et al., 2021). The intermediate CoD problem may be illustrated using the example of “sepsis” (Johnson et al., 2021), which is often recorded as the CoD. There are, in fact, hundreds of potentially-underlying causes of sepsis, and the attribution of CoD to “sepsis”, *per se*, masks the true distribution of the underlying causes of death (which may have led to sepsis, then death). Common causes of sepsis, depending on the local environment, may include malaria, diabetes and road traffic injuries, for instance. The true CoD (UCoD) in such cases is not, in fact, sepsis, but the CoD that preceded that intermediate step before death. The problem of impossible causes of death is illustrated by referring to symptoms that are sometimes recorded as causes of death—e.g., back pain, shortness of breath—but simply do not cause death. Rather, these are more correctly viewed symptoms of underlying diseases (e.g., cancer, heart failure) that constitute the UCoD. The latter classifications—attributions to impossible causes—are labeled “Major Garbage” classifications by the GBD CoD Collaborators (2017).

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

Rather than discarding deaths that are attributed to intermediate or impossible causes from the data collection, the GBD, via the CoD Collaborators sought to distribute these "garbage-coded" data to plausible CoD categories by grouping and assigning them according to their diagnostic relationship to the UCoD. The importance of the garbage-coding problem in the context of our study can be appreciated by using an example provided in Johnson et al. (2021). Here, the authors discuss the data for Brazil in which deaths due to both "physical firearms violence" and "suicide via other specified" means are ranked in the top-20 causes of death prior to the application of the GBD approach to correct for garbage-coding. After applying the garbage-coding correction, both causes of death change rank. Physical firearms violence falls from third to seventh rank, while suicide via other specified means falls from 19th to 24th rank. These are substantial changes in rankings, albeit for one country and not for Australia. Importantly, this example shows that the "garbage coding" problem may be influential for deaths from many underlying causes, and that this concern is not limited to the attribution of deaths to, for example, various chronic diseases.

In Section A.9, Figure A14 of the Appendix, we provide an example for the similarities and differences across datasets by plotting the time series for the aggregate homicide rate for Australia from three different sources. These include the WHO Mortality Database, the newer WHO Global Health Estimates and the IHME data underlying our study. The Appendix provides a detailed discussion of the relevance of differences in these datasets to the policy questions that are of central interest in this paper.

Since the SCM strategy involves creating a synthetic comparator unit from a donor pool of "comparable" nations, over time, the resolution of these measurement errors in respect of CoD data is appealing for our application of the method. Using data that are adjusted, in the foregoing way, to address a measurement problem is, in our view, superior both to (i) proceeding to analysis with series that are known to be problematic, or (ii) abandoning analytical efforts on the grounds of imperfect data series. Nevertheless, since the data are subject to imputation, we employ a number of sensitivity and placebo tests to reassure us that the results obtained from these series are robust to various assumptions about the quality of our CoD data. These include constructing donor pools by applying additional quality filters that have been assigned by the GBD CoD Collaborators (2017) to the data, as we describe below.

2.2 | Selecting a donor pool

In a first step of choosing the potential donor countries for our SCM Design we chose to focus on the 37 Organization for Economic Cooperation and Development (OECD) countries. In doing so we restrict our potential donor pool to countries that should, at a fundamental level, have some important similarities with Australia. The outcome variables we focus on are suicides and homicides. The IHME provided, at our request, disaggregated time series on each of the variables requested for the OECD countries, which include suicide (or, deaths due to "self-harm") and homicide (or, deaths due to "interpersonal violence") by firearms and by other means. In contrast with datasets that have been used in other empirical papers on this subject, we are able explicitly to estimate the impact of the policy of interest on the disaggregated rates of suicide-by-firearm, suicide-by-other-means, homicide-by-firearm and homicide-by-other-means. These distinctions allow us to focus not only on aggregate effects but also on potential substitution effects. For interpersonal violence (i.e., homicide), we also use the primary data to create 5-year moving averages (MAs) in order to smooth out spikes that are due to massacres or mass shootings, such as the massacre that prompted policy change of interest in our analysis. For suicides and homicides data are available for the period 1980–2019.

While our starting point was the 37 OECD countries, we chose not to allow all countries of the OECD to act as potential donors. Instead, we chose to employ a quality rating system that was developed by the GBD CoD Collaborators (GBD CoD Collaborators, 2017) to classify the quality of data that were used to derive the mortality series for each country in the OECD sample.

The main components of this quality rating framework are (i) a scale that runs from 1–5, in which 1 represents the lowest quality data and 5 represents the highest quality data; (ii) a "completeness" measure that computes the number of vital registrations (VRs) of deaths as a percentage of the total estimated ("true") deaths, and (iii) the "Major Garbage Percentage" which is the percentage of VRs recorded that falls into the "Major Garbage" categories (i.e., categories that are impossible causes of death). Each of these components, and the basis for assigning the "-star" quality ratings is described, in detail, in GBD CoD Collaborators (2017). We use these three data quality indicators, to limit the donor pool for the purposes of conducting the main analyses. Our primary estimates employ only the star-rating system used by GBD CoD Collaborators (2017): in this approach we select only those OECD countries with a star rating of 4- or 5-stars as the potential donor pool. This led to the exclusion of three countries—Turkey, Slovakia, and the Republic of Korea (each 3-stars)–from the OECD donor pool. Next, we applied a further restriction by dropping all 4- and 5-star countries that had (i) a completeness score of less than 98% or (ii) a Major Garbage Percentage of more than 20%. We chose these cut-offs somewhat

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE 1 Summary statistics for the restricted sample.

Variable	Obs	Mean	Std. dev.	Min	Max
Homicide-by-firearm rate	1200	0.664	1.030	0.016	7.105
Suicide-by-firearm rate	1200	1.858	1.666	0.026	8.222
Homicide-by-other-means rate	1200	1.889	2.491	0.356	19.122
Suicide-by-other-means rate	1200	13.306	7.527	2.315	44.479
Alcoholism rate	900	2129.614	578.272	555.090	3333.988
Mental disorder	900	13,847.930	1835.205	9687.729	17,609.200
Substance use disorder rate	900	3249.149	764.908	1312.534	5558.945
Anxiety rate	900	4994.440	1259.070	2322.435	7518.905
Depression rate	900	3783.191	654.761	1953.470	5950.096
GDP per capita (current US$)	1104	29,042.890	20,513.040	1444.280	123,678.700
Unemployment—(% of labor force)	870	7.678	4.083	1.100	27.470

arbitrarily, with the intention of generating and applying a higher quality threshold to determine the SCM donor pool. Applying these criteria resulted in the elimination of seven OECD countries from the potential donor pool. The eliminated countries are Turkey, Slovakia, Republic of Korea, Colombia, Mexico, Portugal, and Poland. The summary statistics for the restricted dataset are presented in Table 1 (An overview of the different criteria by country can be found in the Appendix in Table A6).

2.3 | Specification issues and the use of other variables

One strategy that may be employed in the application of SCM is to obtain matching variables that may be correlated, in some systematic way, to the outcome of interest. We hypothesized that the aggregate levels of substance use disorders and mental health disorders and more disaggregated levels of alcohol use disorders, anxiety and depression, may also be correlated with homicide and suicide rates. On that basis, we requested these data and were provided them by the IHME. These series were available, however, only from 1990 onwards. In each case, these variables are aggregated across genders, and as age-standardised rates.

In our main specification for homicides-by-firearms we utilize the outcome itself for the years 1980, 1987 and 1995, as well as the means of alcohol use disorders and mental disorders over the pre-intervention time-periods starting from 1990. Here, we make use of the more disaggregated measure of alcoholism, since the literature suggests that homicides often take place under the influence of alcohol. Dearden and Payne (2009) highlight, for example, that for the time-period mid 2000 - mid 2006, 47% of homicides in Australia could be classified as alcohol-related. Against this backdrop, we also use the mean rates of alcoholism and mental disorders for our main specification for homicides-by-other-means. However, in this case we include the outcome level for every fourth year from 1980 to 1996. We do so because the inclusion of the years 1980, 1987 and 1995 alone does not yield a reasonably close pre-intervention fit between Australia and synthetic Australia.

As with our specification for rates of homicide-by-firearm, in the matching for suicide-by-firearm we make use of the corresponding outcome levels for 1980, 1987 and 1995 and the mean for alcohol use disorders. However, we also include the means for anxiety and depression rather than the more aggregated variable on mental disorders. Using the more disaggregated variables here, it is important to highlight, in particular, the importance of depression in the context of suicides. Finally, we use the means for anxiety, depression and aggregated substance use disorders in our specification for suicide-by-other-means where they are combined with the outcome level for every fourth year from 1980 to 1996.

3 | RESULTS

3.1 | Homicide-and suicide-by-firearm

3.1.1 | Effect on firearm-related deaths

Figure 1 shows the development of homicide rates per 100,000 people for Australia and synthetic Australia over the time period 1980–2003.[1] The main specification that we present here utilizes 5-year MAs. The motivation for invoking MAs, in this instance, is precisely due to the occasional occurrence of mass shootings that lead to spikes in the annual time-series. To make sure that the use of MAs does not lead to an impact prior to the actual policy change we define the MA as the sum of the current time period and the four preceding time periods that is then divided by five.

As shown in Table 2, synthetic Australia consists of Spain with a weight of 0.815, Chile with a weight of 0.184, and the USA with 0.001. We observe a downward trend for both units that moves these rates from close to 0.8 in 1980 toward 0.4 before the intervention in 1996. The data for Synthetic Australia tracks the real Australian data closely during the pre-intervention period. Following the start of the buyback program, we see a substantial divergence between both units, indicating a significant effect of the treatment on firearm-related homicides. Quantitatively this is indicated by the mean of the gaps after the start of the buyback program which is −0.135. This suggests that the introduction of the buyback program had a substantial effect on homicide-by-firearm rates. A post-treatment drop of around 0.135 firearm-related homicides per 100,000 people is fairly large considering the relatively low levels of 0.4–0.8 that we observed prior to 1996. Given the current population of approximately 25.7 Million, the point-estimate reduction constitutes the prevention of an estimated 35 firearm-related deaths per annum.

Figure 2 shows the corresponding results for suicides via firearm. By contrast with the homicides analysis we do not expect to have an occurrence of outlier events with a large number of deaths (i.e., due to mass suicides) and we therefore abstain from using MAs in this case. Synthetic Australia consists of Denmark with a weight of 0.412, Norway with a weight of 0.323, Greece with a weight of 0.260 and Finland with a weight of 0.005. Similar to what was observed with respect to homicides by firearm, the result exhibits a strong downwards trend for both Australia and Synthetic Australia in the years leading to the start of the buyback program. Rates start from more than 3.5 per 100,000 population in 1980 and reach levels of below 2 per 100,000 population at the time of the treatment. Again, the tracking of Australia during the pre-intervention period by Synthetic Australia is fairly close. As with the homicide case, we observe a divergence, albeit a relatively-weaker one, between both units after the treatment in the expected direction. While both units stay on downwards trajectories, the treated unit reaches lower levels than the control does. Post treatment, the mean of the gaps takes a value of −0.299. This suggests that the introduction of the buyback program lowered the rate of suicides by firearm by an average of 0.299 per 100,000 population, per year, from a base rate that was already below 2 per 100,000. This reduction corresponds to a population estimate of 77 firearm-related suicide deaths per annum in Australia.

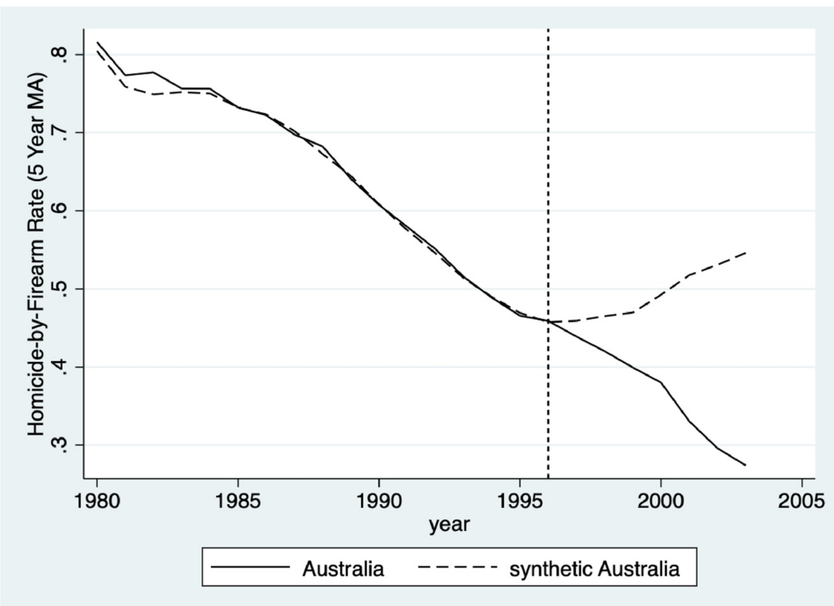


FIGURE 1 Homicide-by-firearm. [Colour figure can be viewed at wileyonlinelibrary.com]

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE 2 Derived donor country weights for Synthetic Australia—firearm related deaths.

Country	Suicide-by-firearm	Homicide-by-firearm
Chile	0	0.184
Denmark	0.412	0
Finland	0.005	0
Greece	0.260	0
USA	0	0.001
Norway	0.323	0
Spain	0	0.815

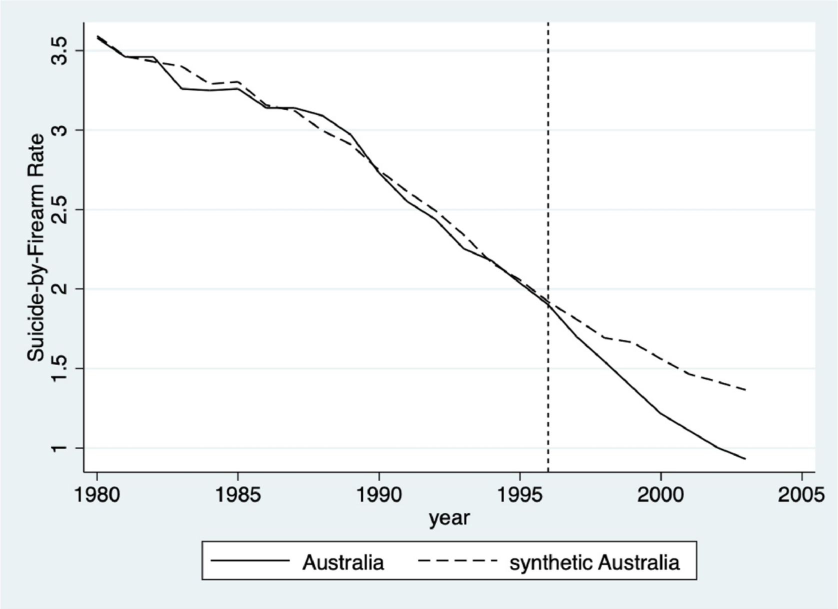


FIGURE 2 Suicide-by-firearm. [Colour figure can be viewed at wileyonlinelibrary.com]

Our main specifications for deaths-by-firearm produce results (see above) that are reasonably simple and constitute the starting point for our analysis. Tables 3 and 4 show the predictor balances for both suicides-by-firearm and homicides-by-firearm, respectively. In both instances we can see a good fit with respect to the pre-treatment outcomes, but a somewhat weaker fit with respect to the other variables. Nonetheless, with the exception of alcoholism in the suicide case, the differences between Australia and synthetic Australia remain under 10%. Overall, the aforementioned results show a relatively strong divergence between treatment and control in the wake of the intervention, suggesting an important reduction in firearm related deaths following the buyback program.

For an easier numerical analysis of the year-by-year trends and gaps between Australia and Synthetic Australia, we provide tables for all of our outcomes of interest in the Appendix in Section A.7, Tables A2–A5.

3.1.2 | Robustness checks

To test the robustness of our baseline results we adopt an approach that is analogous to that adopted by Abadie and Gardeazabal (2003) and Abadie et al. (2010). Specifically, we start by running a series of placebo tests. To do so, we iteratively treat the countries from the donor pool as if they were the treatment units, while excluding Australia from the corresponding donor pools. That means we use the same predictor variables we used before to find synthetic controls for each remaining unit from the original donor pool and analyze how treatment and control units behave pre- and post-1996, as if the donor pool countries were actually the treated units, one by one. We then compare the magnitude of divergence we observe for Australia to those of the placebos. The results for homicide-by-firearm can be found in Figure 3, those for suicide-by-firearm in Figure 4. For a

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE 3 Predictor balance homicide-by-firearm (5-year moving-averages).

Variables as rates per 100,000 population	Australia	Synth. Aus.
Homicide-by-Firearm (5-year moving-averages)		
1980	0.816	0.805
1987	0.698	0.702
1995	0.466	0.469
Other variables		
Alcoholism mean 1990–1995	1713.914	1855.866
Mental disorders mean 1990–1995	17,108.12	15,568.06

TABLE 4 Predictor balance suicide-by-firearm.

Variables as rates per 100,000 population	Australia	Synth. Aus.
Suicide-by-firearm		
1980	3.58	3.593
1987	3.14	3.122
1995	2.038	2.057
Other variables		
Alcoholism mean 1990–1995	1713.914	2177.187
Anxiety mean 1990–1995	5544.436	5765.607
Depression mean 1990–1995	4460.964	4062.18

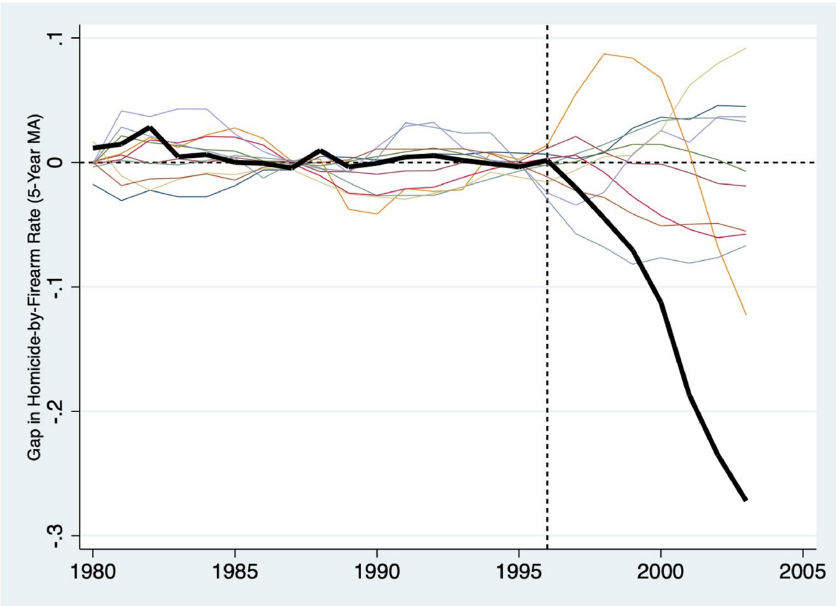


FIGURE 3 Placebos—homicide-by-firearm. [Colour figure can be viewed at wileyonlinelibrary.com]

clearer comparison we removed, in both instances, placebos with pre-treatment root mean-squared prediction errors (RMSPEs) that are more than three times those of Australia. For both outcomes we observe that the divergence between Australia and synthetic Australia is the strongest to the downside as highlighted by the curves in bold print. Consequently, this first test lends strong support to the conclusion that the observed effect is due to the treatment, rather than due to chance.

The corresponding placebo tests where those countries with a pre-treatment variation that is more than two times the one of Australia are excluded may be found in the Appendix in Section A.4–Figure A8.

Figures 5 and 6 present the results for the second test we conduct. Here, we plot the ratios of pre-treatment to post-treatment RMSPEs for Australia and all placebos. A higher ratio generally indicates a stronger effect and hence a higher probability that the observed change is a causal effect, rather than the result of chance. In both Figures, the bars in light blue highlight the RMSPE ratio for Australia. For homicide- and suicide-by-firearm, none of the 29 placebo tests yield larger RMSPE ratios than Australia. This lends further support to the conclusion that the buyback program led to a reduction in the rates of both homicide- and suicide-by-firearm.

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

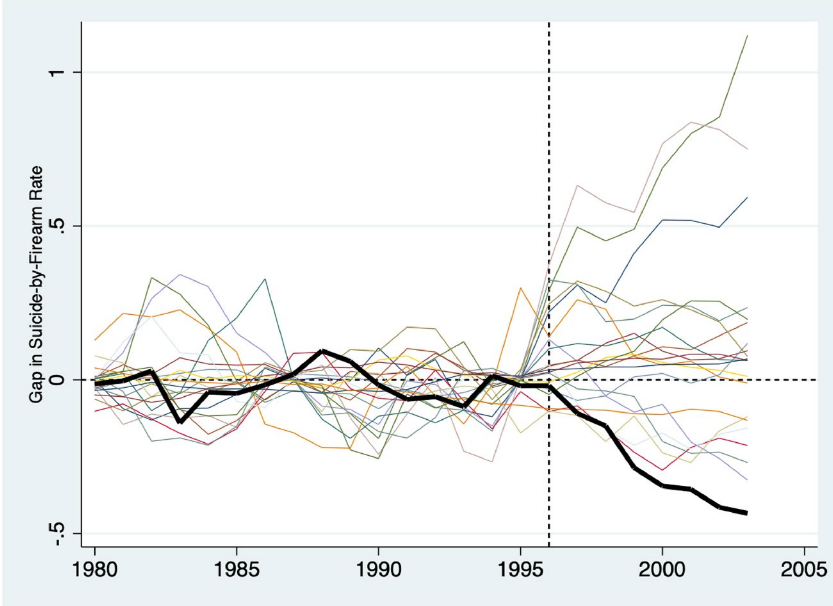


FIGURE 4 Placebos—suicide-by-firearm. [Colour figure can be viewed at wileyonlinelibrary.com]

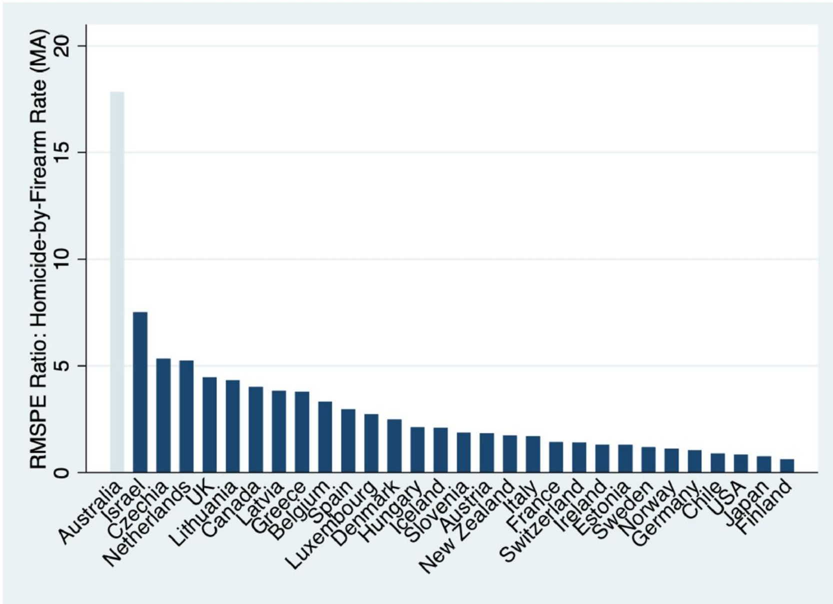


FIGURE 5 Root mean-squared prediction errors (RMSPEs) ranking—homicide-by-firearm. [Colour figure can be viewed at wileyonlinelibrary.com]

Beyond our main specifications we also adopt some alternative specifications to test if the observed divergence between Australia and synthetic Australia is driven by our choice of predictor variables. This appears particularly important for the homicide case as we observe a slight increase for synthetic Australia which seems to mark a trend change. With respect to homicides we therefore present Figure 7 with four alternative graphs tracking Australia and differing synthetic controls. On the top left we show the graph for Australia and synthetic Australia when including the outcome levels from 1980 to 1988 and the pre-intervention mean of the outcome alongside the other variables from our main specification. This graph looks very similar compared to our main specification both pre- and post-intervention, indicating empirically-robust results. What stands out regarding our main results is the very large weight of more than 0.8 that is put on Spain when creating synthetic Australia. One may say that we are largely comparing Australia to Spain. We address this aspect of our findings on the bottom left where we remove Spain from the donor pool. The graph shows that even when the large weight of Spain is distributed to other countries (in this instance Ireland and Japan take on larger weights), the post-intervention divergence does not appear to be significantly affected, either qualitatively or quantitatively. An aspect of our analysis that we address with our specification presented on the top right of Figure 7 is our decision to use MAs. The graph here results from the same choice of variables as in our main results, however, using the original outcome rather than MAs. As a result of the coarser ups and downs in the trend of Australia the pre-intervention tracking by synthetic Australia is not as close. At the same time the overall results and the

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

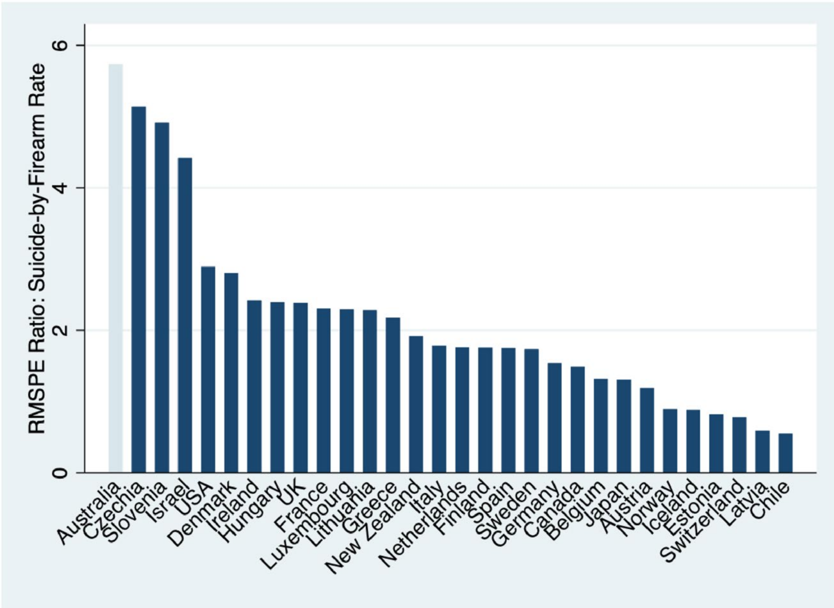


FIGURE 6 Root mean-squared prediction errors (RMSPEs) ranking—suicides-by-firearm. [Colour figure can be viewed at wileyonlinelibrary.com]

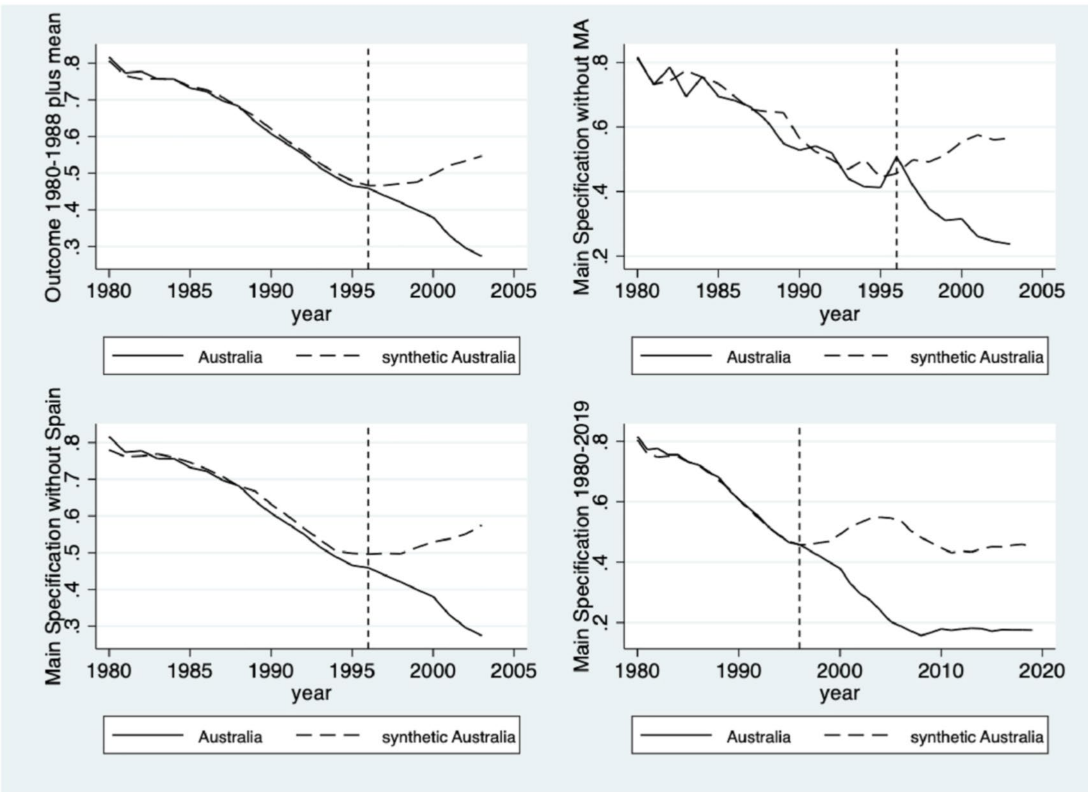


FIGURE 7 Alternative specifications—homicide-by-firearm. [Colour figure can be viewed at wileyonlinelibrary.com]

post-intervention divergence remain robust. As described earlier, we decided generally to restrict our sample to the time-period from 1980 to 2003 to prevent the handgun policy change of 2003 affecting our findings. Nonetheless, on the bottom-right of Figure 7 we show what the result of our main specification would be if we simply ignored the second policy change and plotted the results of our main specification over the full time-period that is covered by our data, i. e. from 1980 to 2019. Looking at the longer post-intervention period, we can see that the divergence between Australia and synthetic Australia remains robust and permanent, while the slight increase we observe for synthetic Australia appears to be of a more temporary nature. Overall, we

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

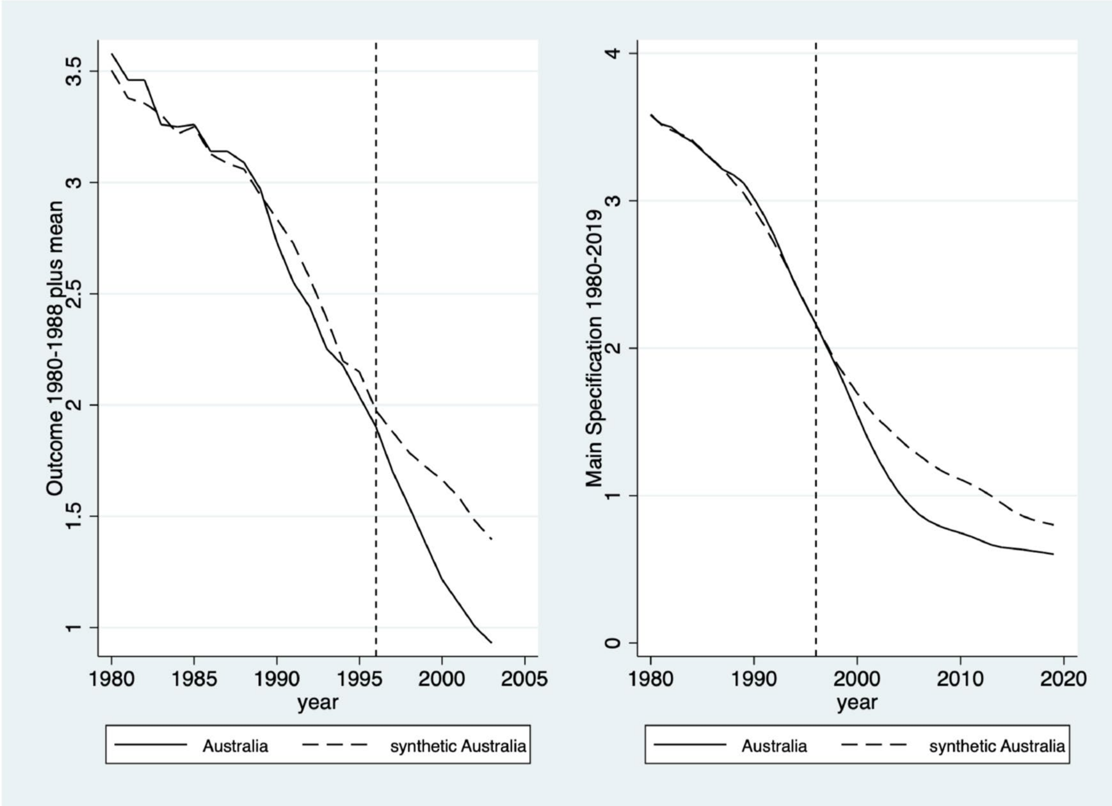


FIGURE 8 Alternative specifications—suicide-by-firearm. [Colour figure can be viewed at wileyonlinelibrary.com]

conclude that the observed gap that follows the buyback program is robust also across specifications and not due to our choice of predictor variables.

As in the case of homicide-by-firearm rates, we also look at alternative specifications and time-periods for suicides-by-firearms. Here we present two alternative specifications: on the left of Figure 8 we show the specification where we make use of the annual values of the outcome variable from 1980 to 1988, the pre-intervention mean of the outcome variable as well as the other variables of the main specification. The graph on the right shows the main specification but for the full time-period the data is available, that is, 1980–2019. As in the case of homicides, the pre-intervention tracking is not as precise as those presented for our main specification, however, the results do not change, qualitatively. While we observe a slight convergence as the years go by following the intervention, a reduction in suicides-by-firearms remains observable at 2019.

Beyond those sets of robustness tests we add another layer of robustness that aims to rule out the possibility that the divergence we observe between Australia and the synthetic control is due to factors other than the policy change. Highlighting the importance of factors such as mental health or substance abuse in the context of homicides and suicides, we plot the trends in these factors for Australia and synthetic Australia with respect to both homicide-by-firearm and suicide-by-firearm. For homicide-by-firearm we plot the trends in alcoholism and mental disorders; for suicide-by-firearm we plot the trends for anxiety, alcoholism and depression. These selections accord with our selection of the variables that were included in our baseline specifications. For none of these variables do we observe any trend change around the time of the policy change, thus lending further support to our interpretation that the changes we observe in suicides and homicides are, in fact, due to the policy change. The corresponding Figures may be found in the Appendix in Section A.3.

In a similar fashion we seek to rule out the possibility that post-intervention divergence between the synthetic control and the actual data are driven by significant trend changes in the outcomes of interest in the SCM donor countries. To do so, we plot the trends in homicide rates and suicide rates for the main donor countries in both cases. As the main donors we consider those countries that contribute a weight of more than 25% in generating synthetic Australia. In the case of homicide-by-firearm this is only Spain, in the case of suicide-by-firearm those countries include Denmark, Greece and Norway. We do not observe any major trend change in these main donor, however, the slow downward trend in the homicide-by-firearm rate in Spain from the 80s appears to end in the early 90s. The Figures pertaining to these trends may be found in Section A.2. Overall, we don't see a

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

risk that the trends of the main donor countries could invalidate our findings. This holds particularly true since we observe very similar outcomes in the homicide-by-firearm case when we eliminate Spain from the potential donor pool as discussed above.

Finally, we run a number of sets of specifications where we (i) include economic predictors, in our baseline specifications, (ii) show the findings for a less restrictive set of potential donor countries, and (iii) conduct simple *t*-tests on the null hypothesis of no statistically significant differences in the pre- and post-treatment gaps between Australia and synthetic Australia. We conduct these tests for several outcomes. For (i) we add unemployment rates and GDP per capita to the baseline specifications. The additional variables come from the World Development Indicator Database provided by The World Bank (2023). Adding these measures does not affect our findings appreciably. The corresponding Figures can be found in 5.5. For (ii) we only restrict the subset of potential donors to those OECD countries that receive at least 4 stars rather than applying the set of all three quality measures. This leads to the addition of Colombia, Mexico, Portugal, and Poland to the set of potential donor countries. We do not make any adjustments to the selection of predictor variables or control variables. For the re-estimations based using this alternative set of potential donor countries we also include sets of placebo estimations and rankings on pre- versus post-treatment variations. In both instances our findings remain stable. The corresponding sets of Figures can be found in Section A.6 of the Appendix. For (iii) the null hypothesis of no statistically significant differences in pre- and post-treatment gaps between Australia and synthetic Australia in our outcomes of interest before and after the policy change on guns (i.e., the NFA reforms of 1996) can be rejected. The corresponding table can be found in the Appendix in Section A.1, Table A1.

3.2 | Homicide-and suicide-by-other-means

The preceding analyses provide fairly convincing evidence that Australia's gun buyback policy reduced suicide-by-firearm and homicide-by-firearm rates. In this section, we seek to determine whether these reductions resulted in reduced death rates, overall, for suicides and homicides in Australia; and hence whether there was a substitution of other means, for firearms. We examine this question by applying the SCM to data on rates of suicide- and homicide-by-other-means.

Table 5 shows the derived donor country weights for both other means outcomes that result from the specifications described in 2. For homicide-by-other-means positive weights are assigned to Chile, Finland, Greece, New Zealand, Netherlands and Sweden, with Greece taking the largest weight with 0.33. For Suicide-by-other-means positive weights are applied to Greece, New Zealand, Spain and Sweden, where the largest is put on New Zealand with a weight of 0.726.

Tables 6 and 7 show the corresponding predictor balances where, similar to the case of firearm related deaths, the lags are matched very closely, while the other variables are more loosely matched.

As in our analysis for homicide-by-firearm we use a 5-year MA as the outcome of interest in order to smooth out massacres (e.g., due to arson-related homicides). Figure 9 shows the result of our analysis. While our generated pre-intervention fit does not perfectly match synthetic Australia, the matching is reasonably close. Interestingly we do observe a trend change following the intervention leading to a divergence between Australia and the synthetic control, which indicates an increase in homicide-by-other-means and therefore a potential substitution effect.

As before we conduct a number of robustness tests which we summarize in Figure 10. The top panel and bottom right show panels show the RMSPE ratios and the gaps in the placebo specifications, respectively. We identify four countries with higher RMSPE ratios and two countries with a stronger post-intervention divergence to the upside by 2003. On the bottom left panel we also present the trends for Australia and synthetic Australia when using an alternative specification that is in line with what we introduced for firearm-related deaths. In this alternative we use the lags for 1980–1988, the mean of the outcome and the other variables from the main specification.

TABLE 5 Derived donor country weights for Synthetic Australia—deaths by other means.

Country	Suicide-by-other-means	Homicide-by-other-means
Chile	0	0.039
Finland	0	0.046
Greece	0.176	0.33
New Zealand	0.726	0.304
Netherlands	0	0.007
Spain	0.006	0
Sweden	0.091	0.273

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE 6 Predictor balance: Homicide-by-other-means (5-year moving-averages).

Variables as rates per 100,000 population	Australia	Synth. Aus.
Homicide-by-other-means (5-year moving-averages)		
1980	1.258	1.255
1984	1.283	1.279
1988	1.376	1.373
1992	1.476	1.472
1996	1.364	1.363
Other variables		
Alcoholism mean 1990–1995	1713.914	2238.626
Mental disorders mean 1990–1995	17,108.12	15,422.49

TABLE 7 Predictor balance: Suicide-by-other-means.

Variables as rates per 100,000 population	Australia	Synth. Aus.
Suicide-by-other-means		
1980	8.67	8.669
1984	8.67	8.699
1988	9.93	9.885
1992	10.230	10.240
1996	11.282	11.203
Other variables		
Substance use disorders mean 1990–1995	3888.746	3453.577
Anxiety mean 1990–1995	5544.436	6744.616
Depression mean 1990–1995	4460.964	3900.556

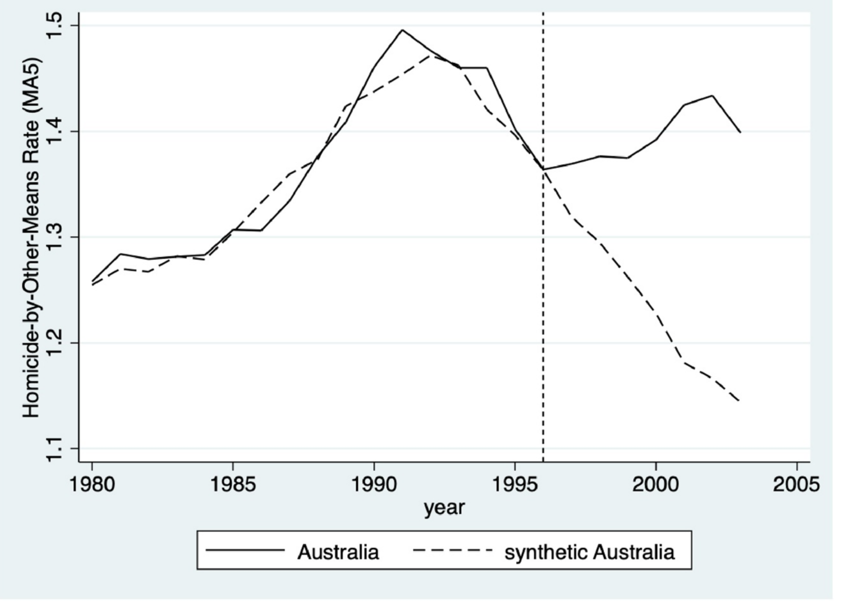


FIGURE 9 Homicide-by-other-means. [Colour figure can be viewed at wileyonlinelibrary.com]

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

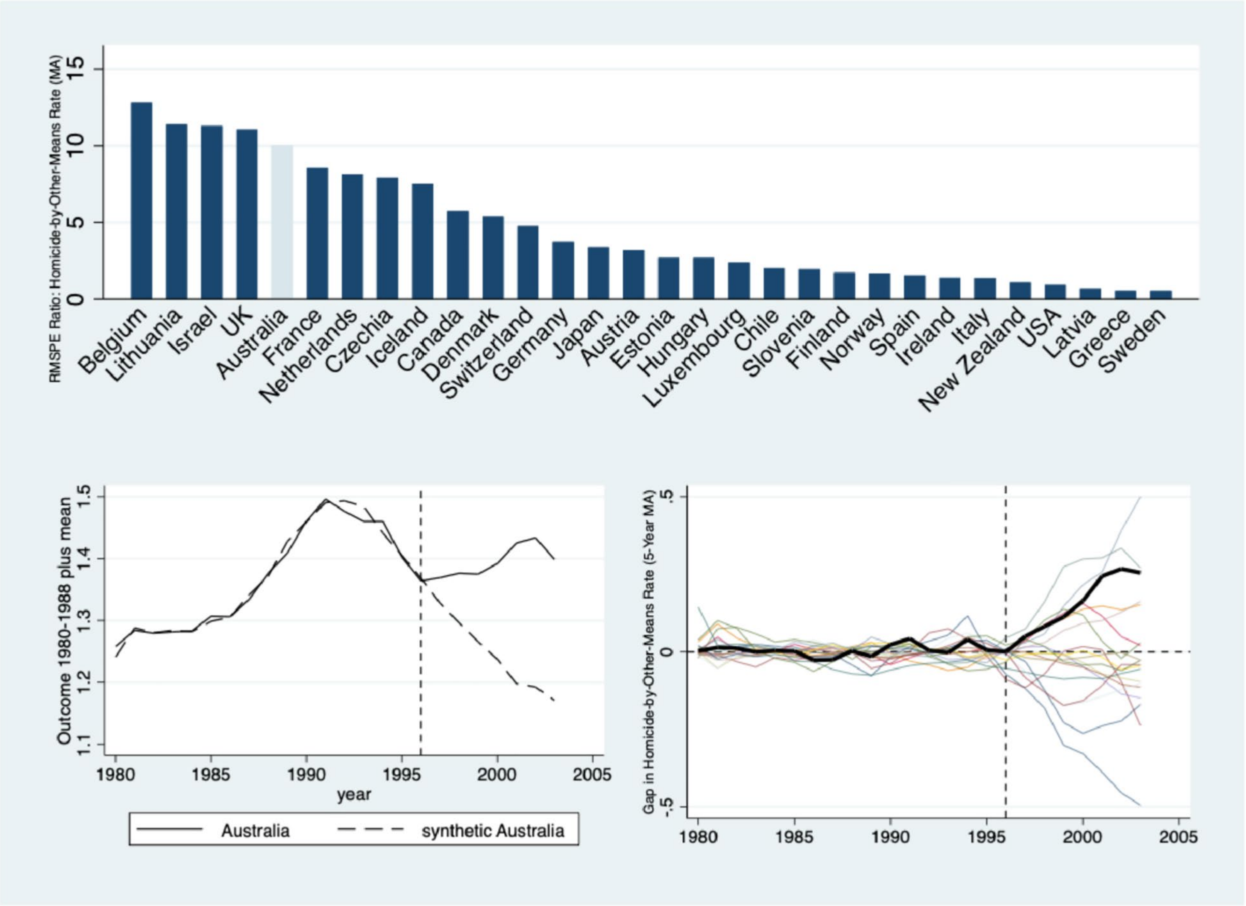


FIGURE 10 Robustness tests—homicide-by-other-means. [Colour figure can be viewed at wileyonlinelibrary.com]

At this point, we put the effects on homicide-by-firearm and homicide-by-other-means into relation. Figure 11 suggests an almost linear offset of the reduction in homicide-by-firearm through an equally or even more sizable increase in the homicide-by-other-means rate. While the gap in 2003 reaches 0.255 which is, in absolute terms, a little less than the gap we observe for homicide-by-firearm rates, the mean gap between Australia and synthetic Australia post-intervention takes a value of 0.168 and is therefore larger than the mean reduction in homicide-by-firearm.

Considering the results of these robustness tests, it is important to highlight that the evidence for a substitution effect is weaker than, and our findings on rates of homicide-by-other-means are less robust than those on firearm-related deaths. However, beyond the robustness tests presented in Figure 10, we also applied the SCM to the aggregated homicide time series using the specifications for homicide-by-firearm and homicide-by-other-means. In doing so, we do not find evidence for a net reduction in homicides during our time period of interest. In conclusion, while the policy reduced homicide-by-firearm rates, the corresponding increase in homicide-by-other-means potentially offsets the effect on overall homicide rates. The results for the aggregated homicide time series are available upon request.

Finally, we examine the empirical results for suicide-by-other-means. Figure 12 shows the trends for Australia and synthetic Australia based on our main specification, while Figure 13 shows the same robustness tests we provided for homicides-by-other-means. At first glance, we can see that also for suicides-by-other-means a post-intervention increase is observable. With respect to the robustness tests we first observe that the trends for the alternative specification presented on the bottom left of Figure 13 are similar to those from our main specification apart from the remaining gap in 2003. As before, the alternative specification makes use of the lags from 1980 to 1988, and includes the pre-intervention mean while the other variables remain the same. The top panel of the Figure shows that Australia has the fourth highest RMSPE ratio of all countries based on our main specification. On the bottom right of Figure 13 we once more plot the trends for Australia and several placebos whose pre-intervention variation is at most three times as high as the one of Australia. The divergence to the upside between treatment and control shortly after the introduction of the buyback program is the strongest for Australia reaching values of more than one. However, the effect decays quickly, reaching identical levels by 2003.

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

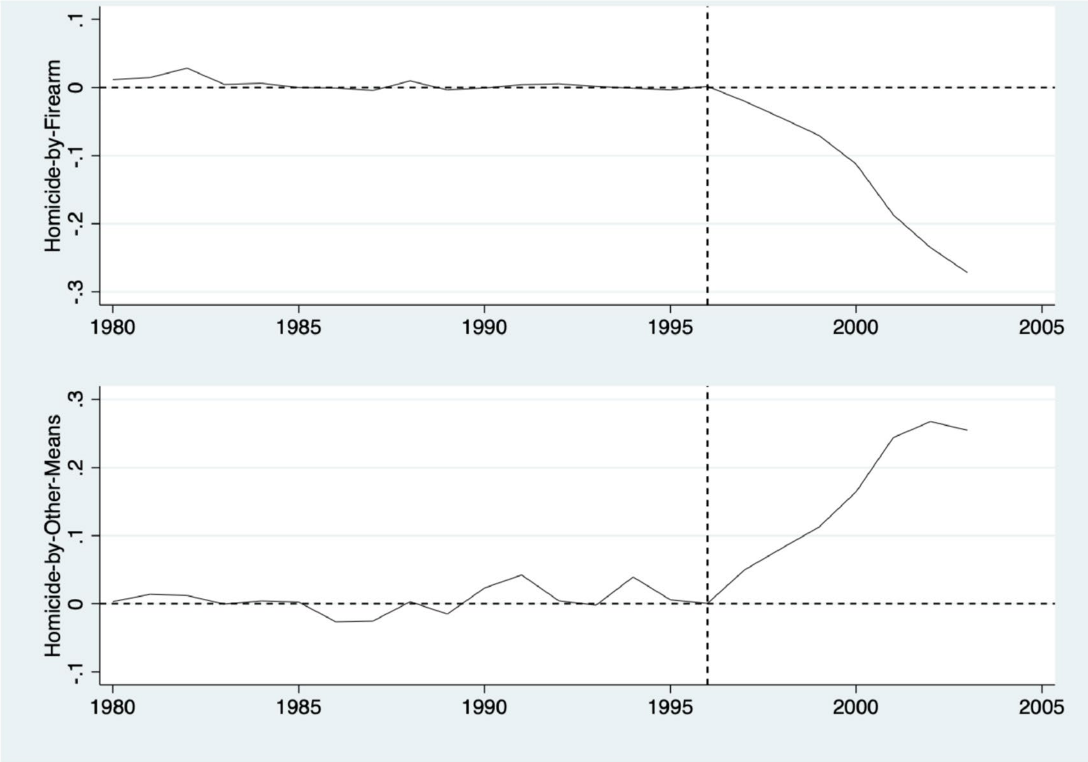
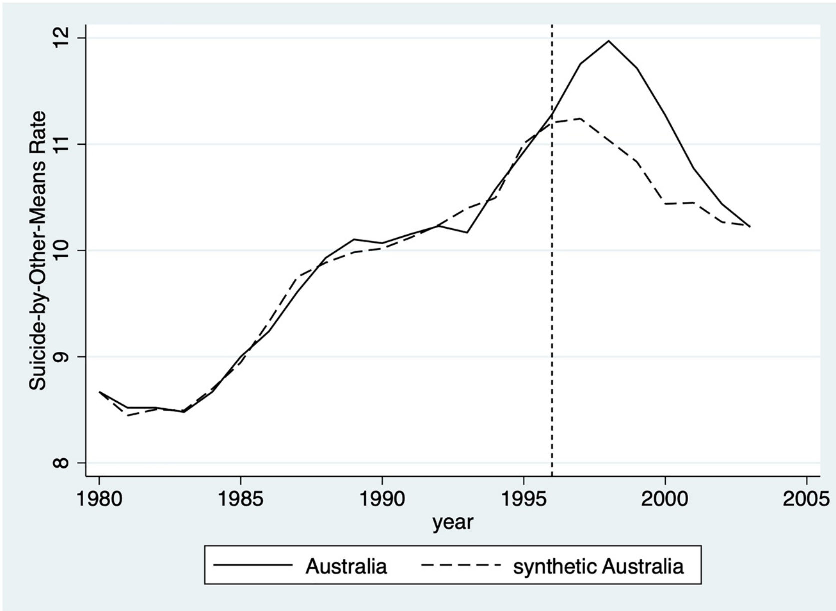


FIGURE 11 Substitution effects homicides. [Colour figure can be viewed at wileyonlinelibrary.com]

FIGURE 12 Suicide-by-other-means. [Colour figure can be viewed at wileyonlinelibrary.com]



10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

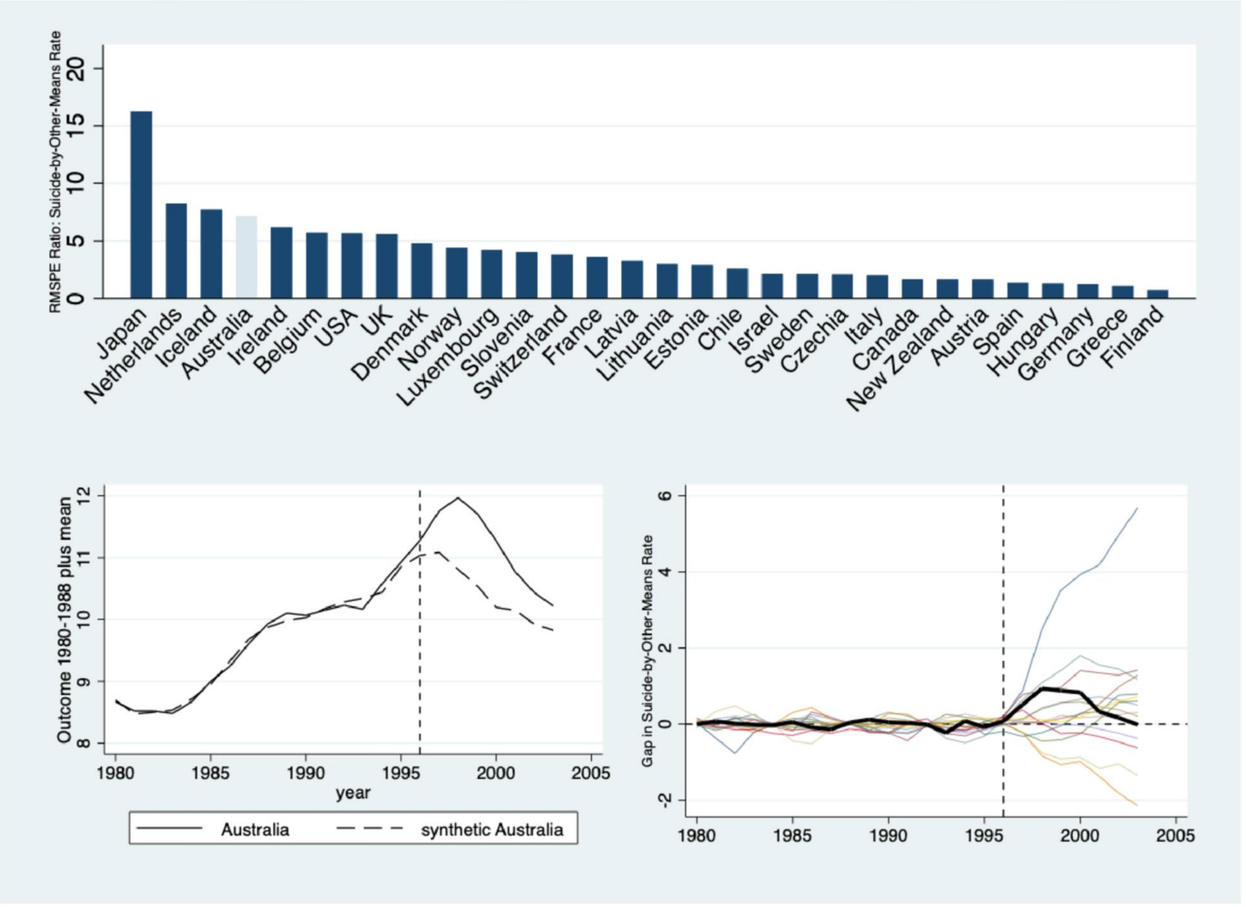


FIGURE 13 Robustness tests—suicide-by-other-means. [Colour figure can be viewed at wileyonlinelibrary.com]

Taking the observed effects of our suicide analyses at face value, we find some evidence for a substitution effect. While we observe a reduction in suicides-by-firearm, we simultaneously see an increase of suicides-by-other-means in the first years after the policy change. This is summarized in Figure 14. What is noteworthy, however, is that in contrast with our other outcomes of interest we do not observe a clear post-intervention trend for suicides-by-other-means, but instead a sharp, but temporary, increase. As such, the findings for suicides suggest a reduction in firearm related suicides, but potentially rising rates in suicides using other means and hence lend some support for the existence of substitution effects. However, the volatility in trends makes it difficult to convincingly quantify the extent to which these effects take place.

4 | CONCLUSION

This paper provides convincing evidence that Australia's gun buyback policy, and related legislation, did produce substantial reductions in firearm deaths, both for suicides and for homicides. It also provides some evidence that, although deaths-by-firearms decreased as a result of the policy, homicide rates overall were not reduced in Australia following the introduction policy due to a substitution between homicides perpetrated by firearms, and homicides perpetrated by other means. The evidence of the policy's effect on suicides is less clear: in this instance we also find at least circumstantial evidence of a substitution between suicide-by-firearm and suicide-by-other-means. In the latter case, however, the initial spike in substitution appears to have attenuated more recently, so that the long-run effect is ambiguous.

The central policy conclusion to be drawn from this work is that gun-buyback initiatives may not only prevent mass shootings, but may reduce homicides- and suicides-by-firearm. The important reason that Australia's experience is likely to differ from that in many other jurisdictions is that its borders are much less porous than those of some jurisdictions that have used gun

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

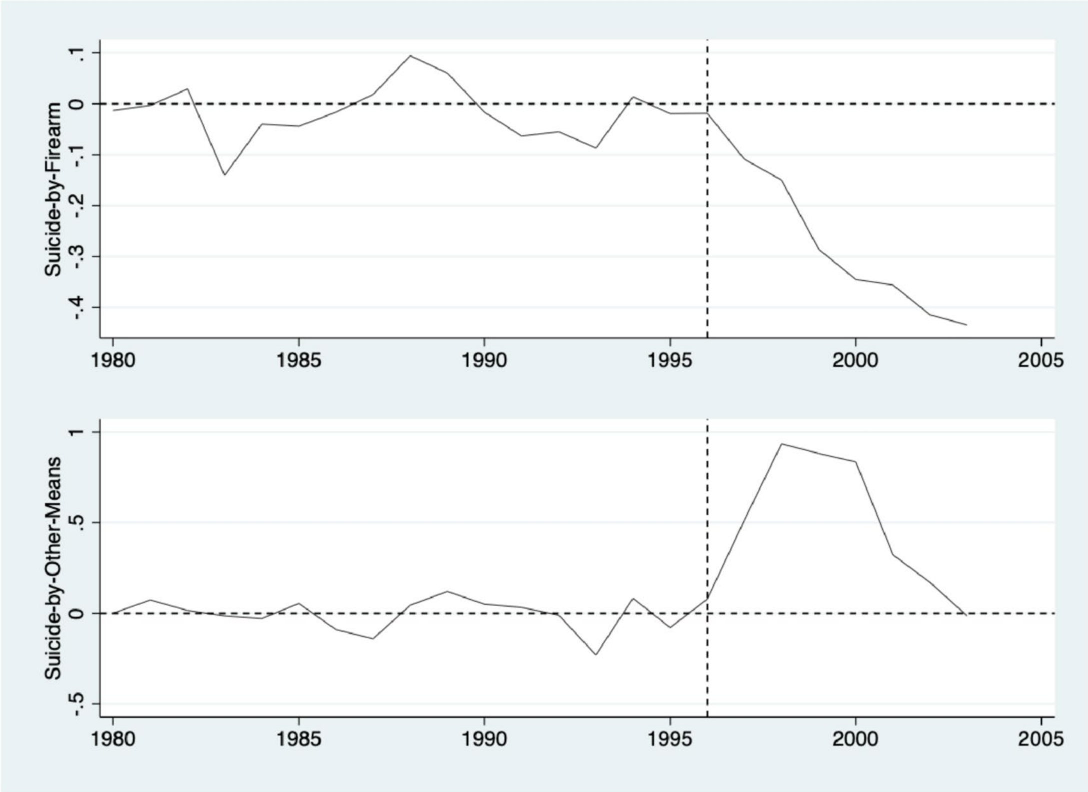


FIGURE 14 Substitution effects suicides. [Colour figure can be viewed at wileyonlinelibrary.com]

buy-backs to apparently little effect. The results reported in this study may suggest that countries with similarly "hard borders" may also benefit from reductions in firearm deaths by invoking similar measures.

Another conclusion that may be drawn is that while gun buy-backs may be successful under the foregoing circumstances, they are no panacea to the complex social problems that give rise to homicide and suicide deaths. Our empirical results suggest that the falling rates of suicide- and homicide-by-firearm attributable to the policy were offset by an other-means substitution effect.

ACKNOWLEDGMENTS

We wish to thank the IHME for providing us with the disaggregated panel data necessary to undertake this work. We are grateful to Dr Angela Maguire for useful discussions on this topic, especially concerning suicidality and the role of "access-to-means" and lethality-of-means" in risk of suicide.

Open access publishing facilitated by The University of Queensland, as part of the Wiley - The University of Queensland agreement via the Council of Australian University Librarians.

CONFLICT OF INTEREST STATEMENT

The authors declare no conflicts of interest.

DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available from IHME. Restrictions apply to the availability of these data, which were used under license for this study.

ORCID

Patrick Duenow https://orcid.org/0000-0002-5425-2559
Luke B. Connelly https://orcid.org/0000-0002-1734-4809

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

ENDNOTE

[1] We restrict the end-point of the sample in this way since, in 2003, another but significantly smaller buyback was introduced that targeted handguns in the possession of citizens.

REFERENCES

Abadie, A., Diamond, A., & Hainmueller, J. (2010). Synthetic control methods for comparative case studies: Estimating the effect of California's Tobacco control program. *Journal of the American Statistical Association*, *105*(490), 493–505. https://doi.org/10.1198/jasa.2009.ap08746

Abadie, A., Diamond, A., & Hainmueller, J. (2015). Comparative politics and the synthetic control method. *American Journal of Political Science*, *59*(2), 495–510. https://doi.org/10.1111/ajps.12116

Abadie, A., & Gardeazabal, J. (2003). The economic costs of conflict: A case study of the Basque country. *The American Economic Review*, *93*(1), 113–132. https://doi.org/10.1257/000282803321455188

Ayres, I., & Donohue, J. J. (1999). Nondiscretionary concealed weapons laws: A case study of statistics, standards of proof, and public policies. *American Law and Economics Review*, *1*(1 and 2), 436–470.

Baker, J., & Mcphedran, S. (2007). Gun laws and sudden death: Did the Australian firearms legislation of 1996 make a difference? *British Journal of Criminology*, *05*, 47.

Balestra, S. (2018). Gun prevalence and suicide. *Journal of Health Economics*, *08*, 61–177. https://doi.org/10.1016/j.jhealeco.2018.08.003

Bartos, B., McCleary, R., Mazerolle, L., & Luengen, K. (2019). Controlling gun violence: Assessing the impact of Australia's gun buyback program using a synthetic control group experiment. *Prevention Science*, *11*(1), 21–136. https://doi.org/10.1007/s11121-019-01064-8

Beauchamp, Z. (2018). *Australia confiscated 650,000 guns. Murders and suicides plummeted*. Vox. Online Retrieved from https://www.vox.com/2015/8/27/9212725/australia-buyback. accessed 19 May 2022.

Black, D., & Nagin, D. (1998). Do right-to-carry laws deter violent crime? *The Journal of Legal Studies*, *27*(1), 209–219. https://doi.org/10.1086/468019

Braga, A., & Wintemute, G. (2013). Improving the potential effectiveness of gun buyback programs. *American Journal of Preventive Medicine*, *45*(5), 668–671. https://doi.org/10.1016/j.amepre.2013.08.002

Callahan, C. M., Rivara, F. P., & Koepsell, T. D. (1994). Money for guns: Evaluation of the Seattle gun buy-back program. *Public Health Reports*, *109*, 472–477. Washington, D.C. : 1974.

Chapman, S., Alpers, P., & Jones, M. (2016). Association between gun law reforms and intentional firearm deaths in Australia, 1979-2013. *Journal of the American Medical Association*, *316*(3), 291–299. https://doi.org/10.1001/jama.2016.8752

Cook, P. J. (1982). The role of firearms in violent crime. In M. E. Wolfgang & N. A. Weinger (Eds.), *Criminal violence*. SAGE Publications Inc.

Dearden, J., & Payne, J. (2009). Alcohol and homicide in Australia. *Trends and Issues in Crime and Criminal Justice*, *07*, 372.

Duggan, M. (2001). More guns, more crime. *Journal of Political Economy*, *109*(5), 1086–1114. https://doi.org/10.1086/322833

Ferrazares, T., Joseph, S., & Mark Anderson, D. (2021). *Have U.S. Gun buyback programs misfired?* SSRN Electronic Journal.

GBD CoD Collaborators. (2017). Global, regional, and national age-sex specific mortality for 264 causes of death, 1980–2016: A systematic analysis for the global burden of disease study 2016. *The Lancet*, *390*(10100), 1151–1210. https://doi.org/10.1016/s0140-6736(17)32152-9

Hazeltine, M., Green, J., Cleary, M., Aidlen, J., & Hirsh, M. (2019). A review of gun buybacks. *Current Trauma Reports*, *12*(4), 5–177. https://doi.org/10.1007/s40719-019-00180-8

Institute for Health Metrics and Evaluation. (2021). *Global health data Exchange*. The University of Washington.

Institute for Health Metrics and Evaluation. (2023). *IHME Starviz - for causes of death data*. IHME. Online Retrieved from https://ihmeuw.shinyapps.io/gbd_starviz_shiny/. accessed 19 May 2023.

Johnson, S. C., Cunningham, M., Dippenaar, I. N., Sharara, F., Wool, E. E., Agesa, K. M., Han, C., Miller-Petrie, M. K., Wilson, S., Fuller, J. E., Balassyano, S., Bertolacci, G. J., Davis Weaver, N., Arabloo, J., Badawi, A., Bhagavathula, A. S., Burkart, K., Cámera, L. A., Carvalho, F., …, Naghavi, M. (2021). Public health utility of cause of death data: Applying empirical algorithms to improve data quality. *BMC Medical Informatics and Decision Making*, *21*(1), 1–175. https://doi.org/10.1186/s12911-021-01501-1

Kleck, G., & Patterson, E. B. (1993). The impact of gun and gun ownership levels on violence rates. *Journal of Quantitative Criminology*, *9*(3), 249–287. https://doi.org/10.1007/bf01064462

Lee, W.-S., & Suardi, S. (2010). The Australian firearms buyback and its effect on gun deaths. *Contemporary Economic Policy*, *28*(1), 65–79. https://doi.org/10.1111/j.1465-7287.2009.00165.x

Leigh, A., & Neill, C. (2010). Do gun buybacks save lives? Evidence from panel data. Institute for the Study of Labor (IZA). *IZA Discussion Papers*, *11*(2), 12–557. https://doi.org/10.1093/aler/ahq013

Ludwig, J. (1998). Concealed-gun-carrying laws and violent crime: Evidence from state panel data. *International Review of Law and Economics*, *18*(3), 239–254. https://doi.org/10.1016/s0144-8188(98)00012-x

Marceau, N. (1997). Self-selection and violence in the market for crime. *International Review of Law and Economics*, *17*(2), 193–201. https://doi.org/10.1016/s0144-8188(97)00008-2

McDowall, D., & Wiersema, B. (1995). The incidence of defensive firearm use by US crime victims, 1987 through 1990. *American Journal of Public Health*, *84*(12), 1982–1984. https://doi.org/10.2105/ajph.84.12.1982

Medoff, M., & Magaddino, J. (1983). Suicides and firearm control laws. *Evaluation Review*, *7*(3), 357–372. https://doi.org/10.1177/0193841x8300700305

Moody, C. (2001). Testing for the effects of concealed weapons laws: Specification errors and robustness. *The Journal of Law and Economics*, *44*(S2), 799–813. https://doi.org/10.1086/323313

Mueller, D. C. (1979). *Public choice*. Cambridge University Press.

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

Mustard, D., & Lott, J. (1997). Crime, deterrence, and right-to-carry concealed handguns. *The Journal of Legal Studies*, *26*, 1–68. https://doi.org/10.1086/467988

Oliveira, C., & Neto, G. (2015). The deterrence effects of gun laws in games with asymmetric skills and information. *Review of Law & Economics*, *05*, 11.

Reuter, P., & Mouzos, J. (2003). Australia: A massive buy back of low-risk guns. In P. J. Cook & J. Ludwig (Eds.), *Evaluating gun policy: Effects on crime and violence*. Brookings Institute Press.

Reuters. (2021). *Australia marks 25 Years since worst mass shooting*. Reuters. Online Retrieved from https://www.reuters.com/world/asia-pacific/australia-marks-25-years-since-worst-mass-shooting-2021-04-28/. accessed 30 May 2022.

Rosenfeld, R. (1995). Gun buybacks: Crime control or community mobilization. In M. R. Plotkin (Ed.), *Under fire: Gun buy-backs, exchanges and amnesty programs*. Police Executive Research Forum.

Taylor, B., & Li, J. (2015). Do fewer guns lead to less crime? Evidence from Australia. *International Review of Law and Economics*, *01*, 42–78. https://doi.org/10.1016/j.irle.2015.01.002

Taylor, R. (1995). A game theoretic model of gun control. *International Review of Law and Economics*, *15*(3), 269–288. https://doi.org/10.1016/0144-8188(95)00018-4

The World Bank. (2023). World development indicators. Online. Retrieved from https://databank.worldbank.org/source/world-development-indicators. accessed 10 February 2023.

World Health Organization. (2023a). *Global health estimates*. WHO. Online Retrieved from https://www.who.int/data/gho/data/indicators/indicator-details/GHO/estimates-of-rates-of-homicides-per-100-000-population. accessed 12 August 2023.

World Health Organization. (2023b). *WHO mortality database*. WHO. Online Retrieved from https://platform.who.int/mortality/themes/theme-details/topics/indicator-groups/indicator-group-details/MDB/violence. accessed 12 August 2023.

How to cite this article: Duenow, P., & Connelly, L. B. (2024). The effect of gun buy-back law reform on homicides and suicides in Australia. *Health Economics*, *33*(2), 248–279. https://doi.org/10.1002/hec.4769

APPENDIX A

A.1 | Average policy effects and T-tests

In Section 3.1.2 of the paper, we reported that simple *t*-tests on the null hypothesis of no statistically significant differences in the pre-and post treatment gaps between Australia and synthetic Australia have been conducted for our outcomes of interest. Table A1 reports the detailed results of those tests, showing that the null hypothesis may be rejected at the 10 per cent level, or better.

TABLE A1 Average policy effects and *T*-tests.

	Difference post and pre-treatment	Pre-treatment	Post-treatment
Homicide-by-firearm	−0.1388**	0.0043	−0.1345
	(0.0370)		
Suicide-by-firearm	−0.2812***	−0.0177	−0.2990
	(0.0495)		
Homicide-by-other-means	0.1628**	0.0050	0.1678
	(0.0340)		
Suicide-by-other-means	0.5228*	−0.0022	0.5205
	(0.1438)		
N	24	17	7

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

A.2 | Outcome trends for major donor countries in main specification (weight greater 0.25)

In Section 3.1.2 of the paper we discuss that we plot the trends of the major donor countries for both firearm-related outcomes to test if a significant trend change occurred around the time of treatment period. Figure A1 shows the diagram for the homicide-by-firearm case, Figure A2 for suicide-by-firearm.

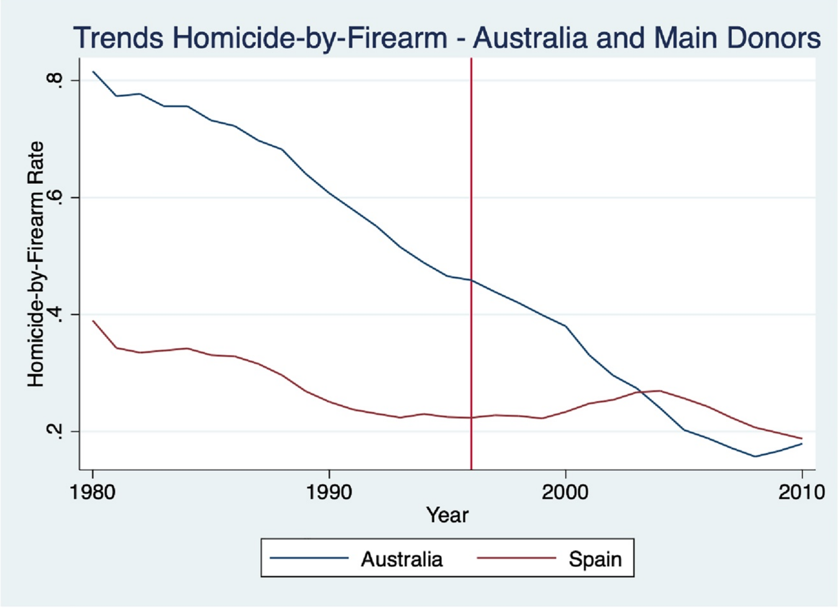


FIGURE A1 Trends homicide-by-firearm. [Colour figure can be viewed at wileyonlinelibrary.com]

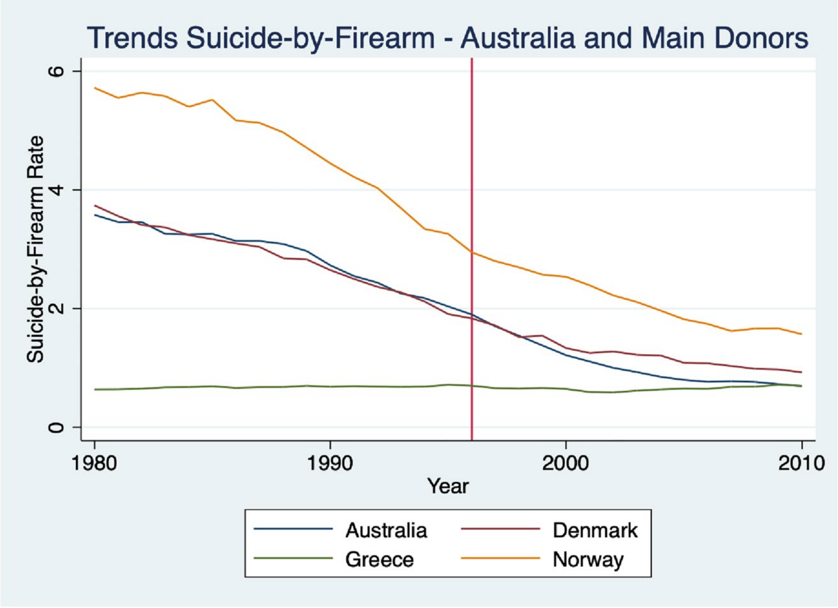


FIGURE A2 Trends suicide-by-firearm. [Colour figure can be viewed at wileyonlinelibrary.com]

A.3 | Trends in control-variables—Australia versus Synthetic Australia

As highlighted in Section 3.1.2 of the paper, we would like to rule out that major trend changes in our control variables such Substance Abuse or Mental Disorders drive our findings. We therefore plot the trends of our selected control variables for the firearm-related outcomes for Australia and synthetic Australia. For homicide-by-firearm we show the trends in alcoholism and mental disorders in Figures A3 and A4, respectively. The trends we capture on suicide-by-firearm are presented in Figures A5–A7 and focus on anxiety, alcoholism and depression rates. We do not observe any major changes around the time of the policy change of interest.

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

A.3.1 | Homicide-by-firearm

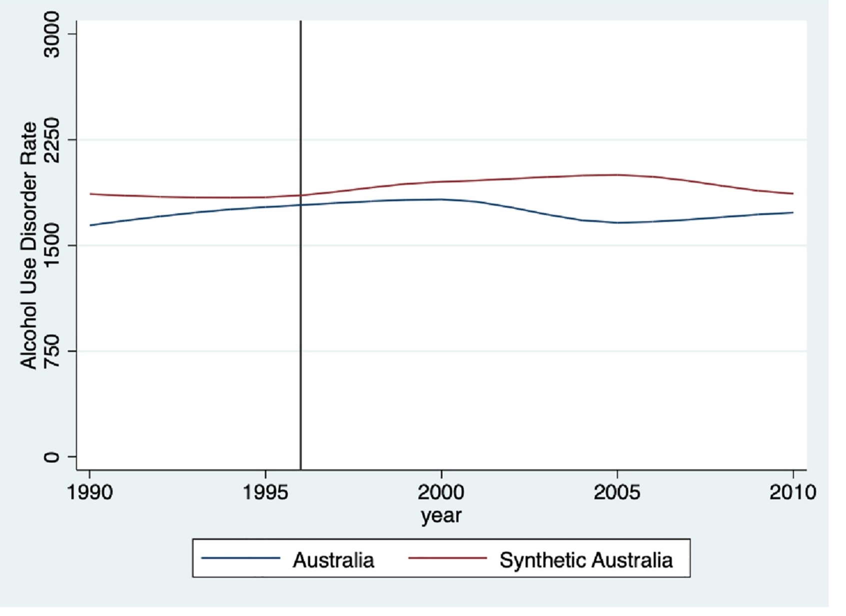


FIGURE A3 Trends alcoholism—homicide-by-firearm specification. [Colour figure can be viewed at wileyonlinelibrary.com]

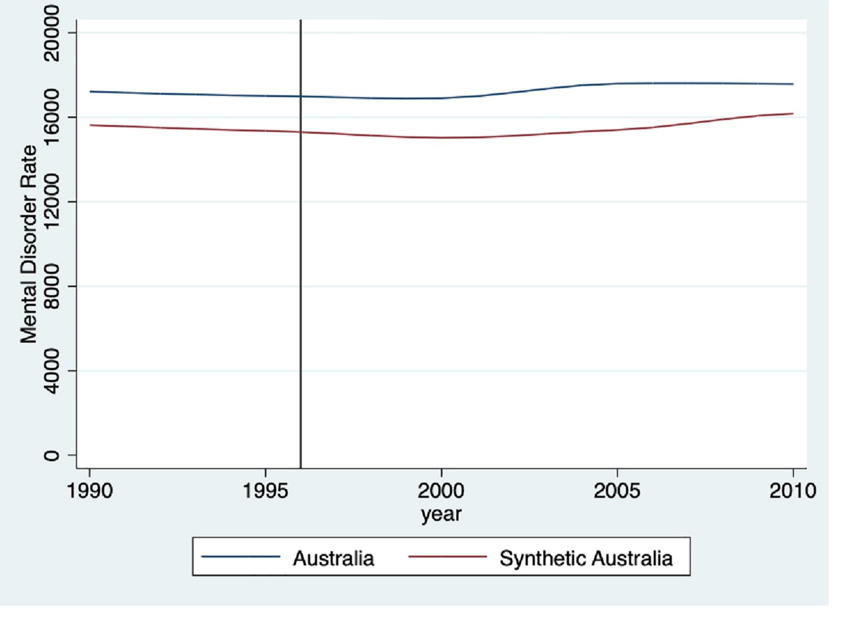


FIGURE A4 Trends mental disorders—homicide-by-firearm specification. [Colour figure can be viewed at wileyonlinelibrary.com]

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

A.3.2 | Suicide-by-firearm

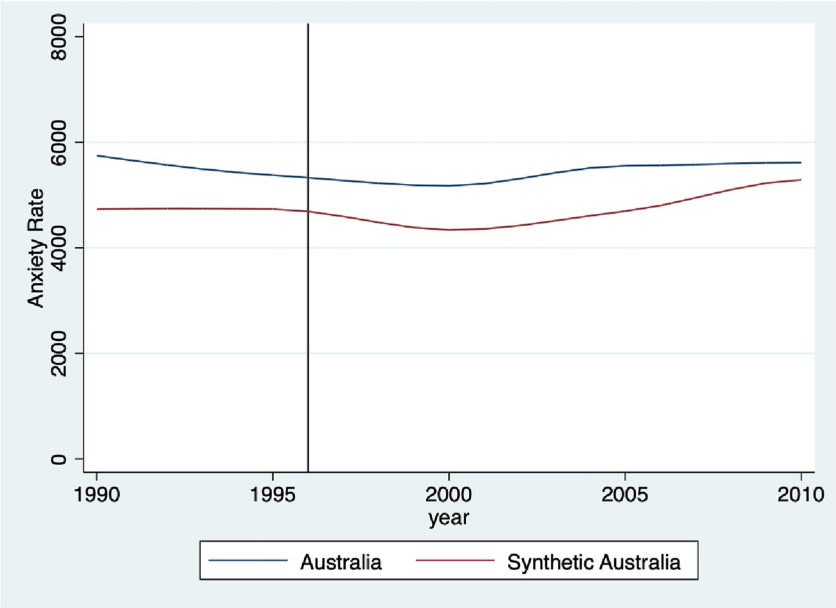


FIGURE A5 Trends anxiety—suicide-by-firearm specification. [Colour figure can be viewed at wileyonlinelibrary.com]

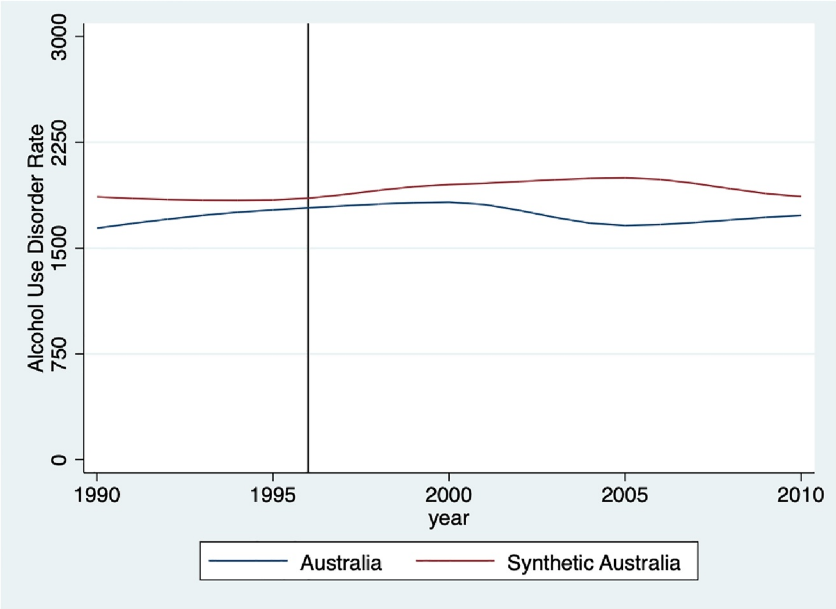
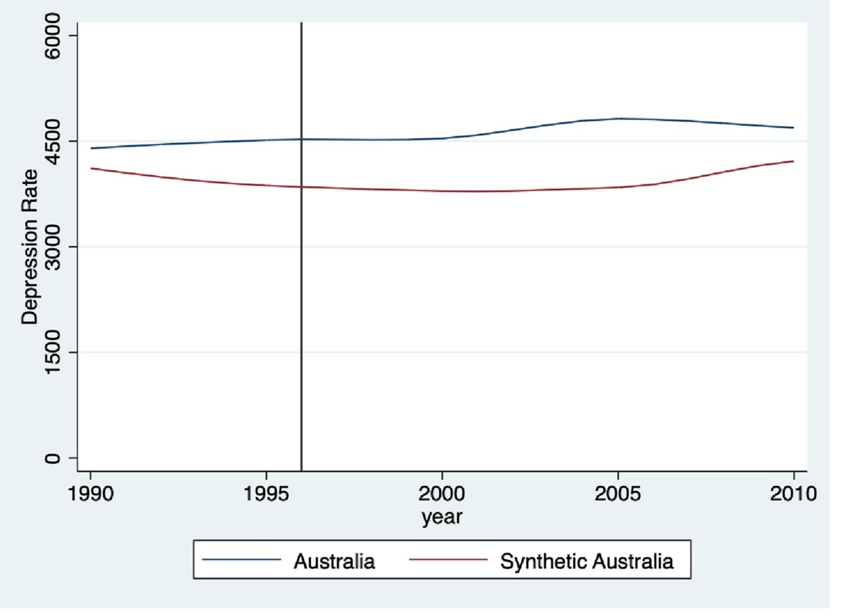


FIGURE A6 Trends alcoholism—suicide-by-firearm specification. [Colour figure can be viewed at wileyonlinelibrary.com]

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

FIGURE A7 Trends depression—suicide-by-firearm specification. [Colour figure can be viewed at wileyonlinelibrary.com]



A.4 | Additional placebo tests main specification

We discuss in Section 3.1.2 that we also conduct the placebo analysis for our main specifications eliminating those countries that show a pre-treatment variation that is more than double the one of Australia. The corresponding diagram is Figure A8. As one would expect this placebo with stronger inclusion requirements generally would strengthen our findings. In particular, the outlier to the upside in the case of suicide-by-other-means disappears.

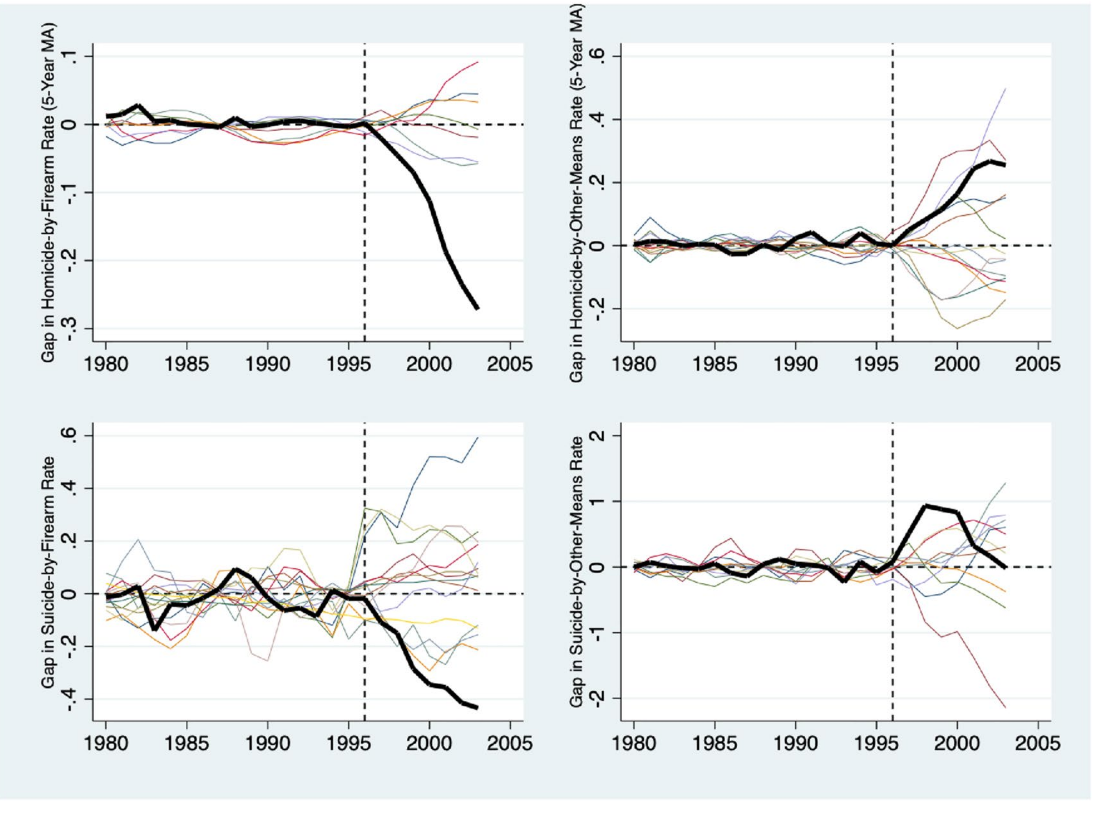


FIGURE A8 Placebo tests baseline specificaton—Countries with more than two times pre-treatment variation dropped. [Colour figure can be viewed at wileyonlinelibrary.com]

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

A.5 | Main specification including GDP and unemployment data

In Figure A9 we show the output for the SCM analysis on all of our outcomes of interest. As highlighted in Section 3.1.2 of the paper, the outputs show no significant differences when compared to our main specifications.

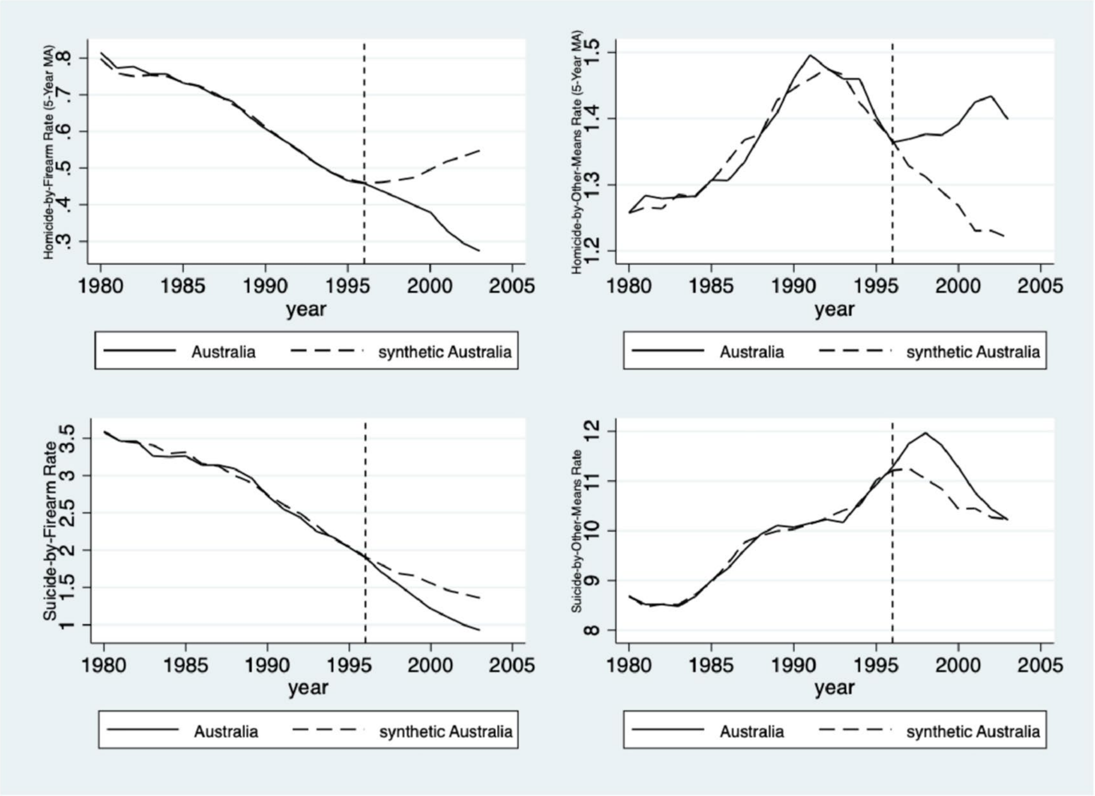


FIGURE A9 Outcomes main specification with added means for GDP per capita and unemployment. [Colour figure can be viewed at wileyonlinelibrary.com]

A.6 | Robustness-tests—Results when only countries with data quality under 4 Stars are dropped

We discussed in Section 3.1.2 that we also conduct the main analysis on all outcomes on a less restrictive set of potential donor countries where we only exclude those countries that have a IHME rating under 4-stars from our set of OECD countries. Figure A10 shows the main output, Figure A11 shows the ranking of RMSPE ratios. Figures A12 and A13 show the more and less restrictive placebo tests. Overall, the results remain, in our opinion, qualitatively and quantitatively robust. While the placebo tests and RMSPE ratios for the firearm related deaths are slightly weaker compared to our main specification, they are still convincing. At the same time the corresponding robustness for our other-means outcomes appear slightly more robust than in our main specification.

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

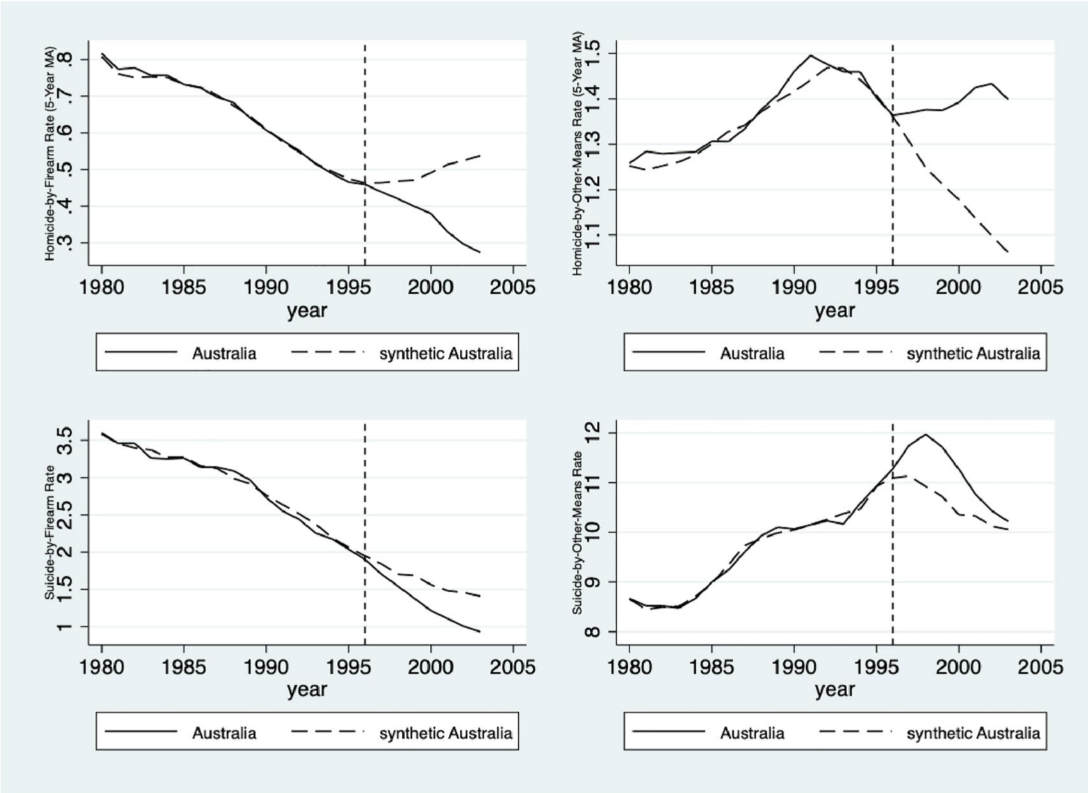


FIGURE A10 Main specifications 4 stars. [Colour figure can be viewed at wileyonlinelibrary.com]

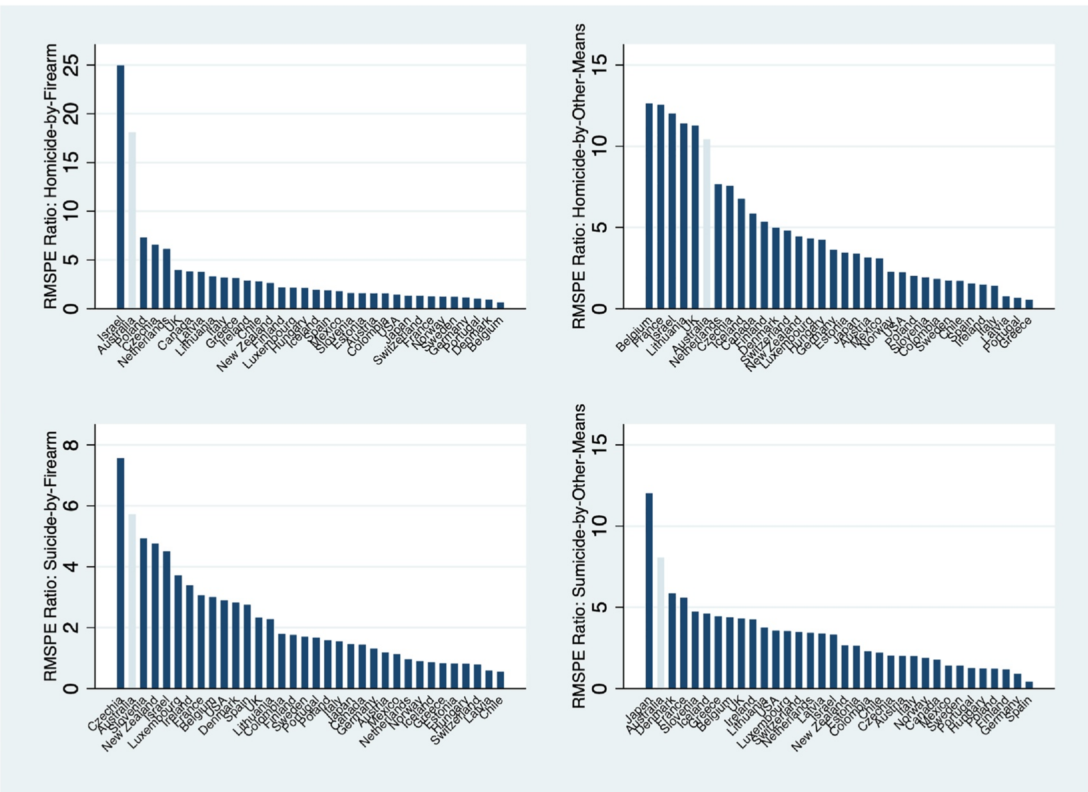


FIGURE A11 Root mean-squared prediction error (RMSPE) Ratios 4 stars. [Colour figure can be viewed at wileyonlinelibrary.com]

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

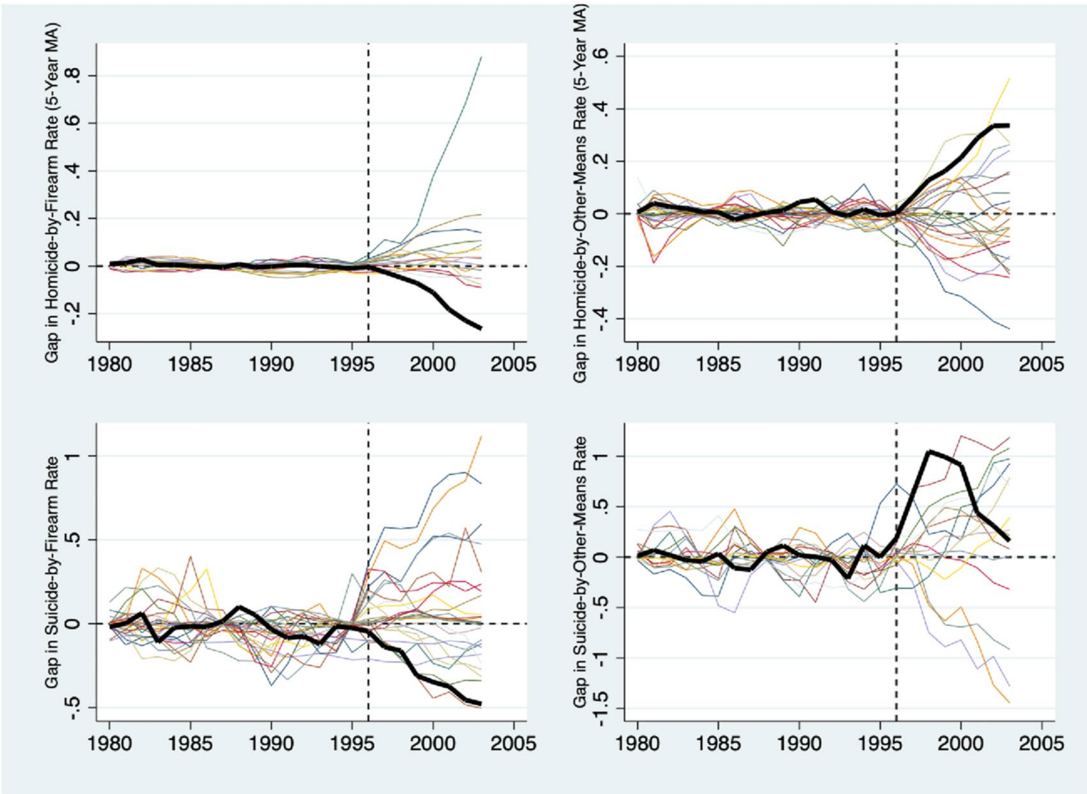


FIGURE A12 Placebo tests—Countries with more than three times pre-treatment variation dropped 4 stars. [Colour figure can be viewed at wileyonlinelibrary.com]

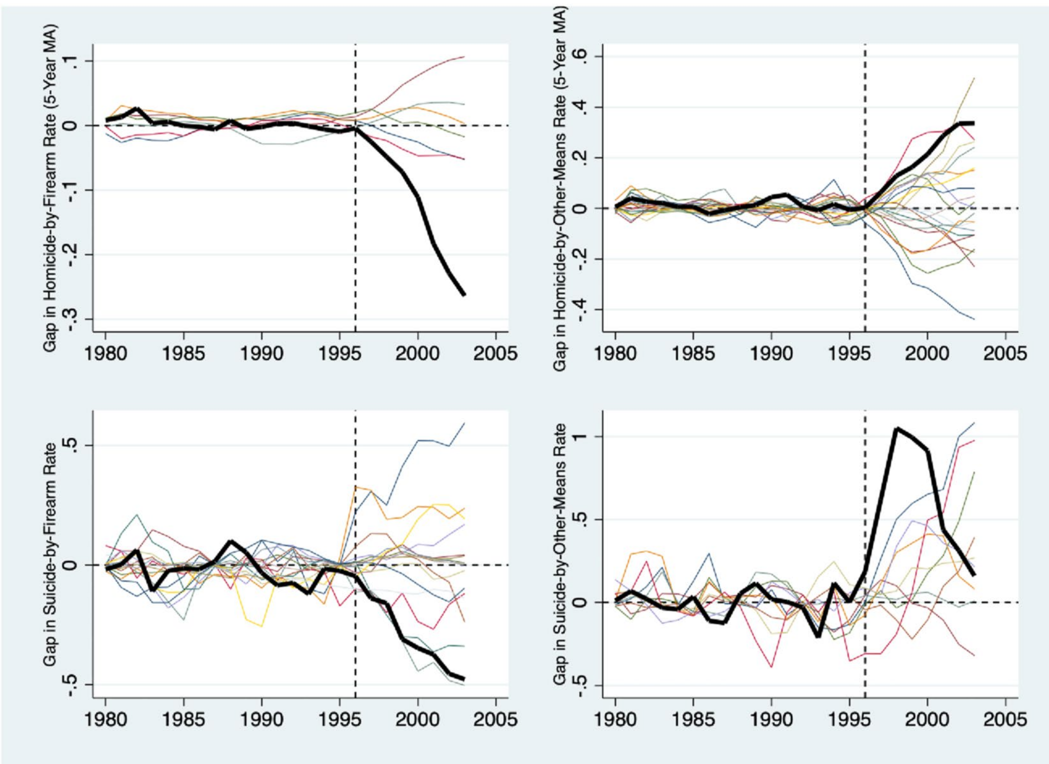


FIGURE A13 Placebo tests—Countries with more than two times pre-treatment variation dropped 4 stars. [Colour figure can be viewed at wileyonlinelibrary.com]

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

A.7 | Tables with trends and gaps

As discussed in Section 3 we provide Tables A2–A5 which capture the trends of our outcomes of interest for Australia and Synthetic Australia. We also include the gaps on a year-by-year basis.

TABLE A2 Homicide-by-firearm.

Year	Australia	Synthetic Australia	Gap
1980	0.816	0.805	0.011
1981	0.773	0.759	0.015
1982	0.777	0.749	0.028
1983	0.757	0.752	0.005
1984	0.756	0.750	0.006
1985	0.732	0.732	0.000
1986	0.722	0.723	−0.001
1987	0.698	0.702	−0.004
1988	0.682	0.673	0.010
1989	0.641	0.645	−0.004
1990	0.608	0.608	−0.001
1991	0.579	0.575	0.004
1992	0.551	0.546	0.005
1993	0.516	0.514	0.002
1994	0.489	0.490	−0.001
1995	0.466	0.469	−0.004
1996	0.459	0.457	0.002
1997	0.439	0.459	−0.020
1998	0.420	0.465	−0.045
1999	0.399	0.470	−0.070
2000	0.380	0.492	−0.112
2001	0.331	0.518	−0.187
2002	0.296	0.531	−0.235
2003	0.274	0.546	−0.272

TABLE A3 Suicide-by-firearm.

Year	Australia	Synthetic Australia	Gap
1980	3.580	3.593	−0.013
1981	3.460	3.463	−0.003
1982	3.460	3.431	0.029
1983	3.260	3.401	−0.141
1984	3.250	3.290	−0.040
1985	3.260	3.304	−0.044
1986	3.140	3.156	−0.016
1987	3.140	3.122	0.018
1988	3.090	2.996	0.094
1989	2.970	2.910	0.060
1990	2.731	2.747	−0.017

(Continues)

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE A3 (Continued)

Year	Australia	Synthetic Australia	Gap
1991	2.551	2.614	−0.063
1992	2.439	2.493	−0.055
1993	2.254	2.341	−0.087
1994	2.177	2.164	0.014
1995	2.038	2.057	−0.019
1996	1.901	1.920	−0.019
1997	1.702	1.810	−0.109
1998	1.543	1.693	−0.150
1999	1.378	1.664	−0.285
2000	1.215	1.560	−0.345
2001	1.109	1.465	−0.356
2002	1.003	1.417	−0.414
2003	0.931	1.365	−0.434

TABLE A4 Homicide-by-other-means.

Year	Australia	Synthetic Australia	Gap
1980	1.258	1.255	0.003
1981	1.284	1.270	0.014
1982	1.279	1.267	0.012
1983	1.282	1.282	−0.001
1984	1.283	1.279	0.004
1985	1.307	1.304	0.003
1986	1.306	1.333	−0.027
1987	1.334	1.360	−0.025
1988	1.376	1.373	0.003
1989	1.409	1.424	−0.015
1990	1.461	1.438	0.023
1991	1.496	1.454	0.042
1992	1.476	1.472	0.004
1993	1.460	1.462	−0.002
1994	1.460	1.421	0.039
1995	1.402	1.396	0.006
1996	1.364	1.363	0.001
1997	1.369	1.319	0.050
1998	1.376	1.295	0.081
1999	1.375	1.262	0.113
2000	1.392	1.228	0.164
2001	1.425	1.181	0.244
2002	1.434	1.166	0.267
2003	1.399	1.144	0.255

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE A5 Suicide-by-other-means.

Year	Australia	Synthetic Australia	Gap
1980	8.670	8.669	0.001
1981	8.520	8.447	0.073
1982	8.520	8.504	0.016
1983	8.480	8.494	−0.014
1984	8.670	8.699	−0.029
1985	9.000	8.945	0.055
1986	9.240	9.330	−0.090
1987	9.610	9.750	−0.140
1988	9.930	9.885	0.045
1989	10.104	9.983	0.121
1990	10.069	10.019	0.050
1991	10.154	10.121	0.033
1992	10.230	10.240	−0.010
1993	10.168	10.398	−0.229
1994	10.577	10.495	0.083
1995	10.928	11.007	−0.079
1996	11.282	11.203	0.078
1997	11.755	11.241	0.514
1998	11.972	11.037	0.935
1999	11.715	10.833	0.882
2000	11.273	10.438	0.835
2001	10.773	10.450	0.323
2002	10.437	10.267	0.170
2003	10.220	10.234	−0.014

A.8 | Data quality

As referred to in Section 2.2, Table A6 shows the assigned quality criteria by country. The criteria presented are based on the full time-series as presented by the Institute for Health Metrics and Evaluation (2023).

TABLE A6 Country ratings—from main analysis excluded countries are in bold.

Country	Star rating	Completeness %	Major garbage percentage %
Australia	5	100	7.19
Austria	5	100	7.16
Belgium	4	100	16.23
Canada	5	100	9.81
Chile	4	99.61	15.35
Colombia	4	97.33	15.14
Czechia	4	100	11.94
Denmark	5	100	13.95
Estonia	5	100	7.27
Finland	5	100	7.19
France	4	100	18.92
Germany	4	100	15.00
Greece	4	100	17.75
Hungary	5	100	7.94

(Continues)

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE A6 (Continued)

Country	Star rating	Completeness %	Major garbage percentage %
Iceland	5	100	6.28
Ireland	5	100	7.76
Israel	4	100	17.47
Italy	5	100	10.27
Japan	5	100	14.64
Latvia	5	100	8.47
Lithuania	5	100	6.77
Luxembourg	5	99.91	13.79
Mexico	4	91.72	15.29
Netherlands	5	100	13.45
New Zealand	5	100	3.91
Norway	5	100	10.63
Poland	4	100	29.80
Portugal	4	100	20.33
Republic of Korea	3	97.74	22.63
Slovakia	3	100	13.19
Slovenia	4	100	8.76
Spain	4	100	15.30
Sweden	5	100	10.35
Switzerland	4	100	17.63
Turkey	3	57.96	46.91
United Kingdom	5	100	7.39
United States of America	5	100	10.91

A.9 | Data comparison

Below in Figure A14, we extracted the time series for aggregate homicides in Australia from different datasets to provide an example of the differences in fatality rate estimates across three data sources. The data is "raw" in the sense, that we did not employ any MAs for this illustration. The time-period covered for the data from the WHO Mortality Database (in blue) is 1967–2007. This is, in our understanding, the time-period and data source used in Bartos et al. (2019). The time series from the more recently released WHO Database on Global Health Estimates (in orange) is only available from 2000 but was added for comparison. With respect to the Global Health Estimates database it is notable that the WHO makes use of similar methods as the IHME to adjust the data. The GBD time series that forms the aggregate basis for the homicide part of our study starts in 1980 and is presented in gray.

Looking at the "older" WHO data and the IHME time series we observe that both time series indicate a downward trend starting around 1990, which highlights the importance of implementing a method such as the SCM that allows to account for this pre-intervention trend. While the time series appear similar in overall trends there are distinct differences. In the WHO Mortality Database, we observe its lowest value of 0.78 in 2004, whereas, in the Global Health Estimates the value for 2004 is 1.28 and at this point in time slightly higher than in the IHME data. One noteworthy aspect of the WHO Mortality Database is, that there is a discontinuity in 2005 and that immediately prior to the data gap there is a precipitous fall in the trend, while, when the series resumes, it appears to do so at a similar level to that prior to the marked decrease. The "new" WHO series, in orange, appears to involve a substantial correction to the previous data and the fall witnessed in the "old" series appears to be no more than half the magnitude in the series utilized by Bartos et al. (2019).

The new approach of the WHO regarding their Global Health Estimates database and the data we extracted from the WHO databases supports, in our opinion, the use of the IHME data, which explicitly address the "garbage coding" problem we noted in the paper. The differences across the time series in the 2000s are, in particular, a potential explanation for the considerable differences in our findings from those reported by Bartos et al. (2019).

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

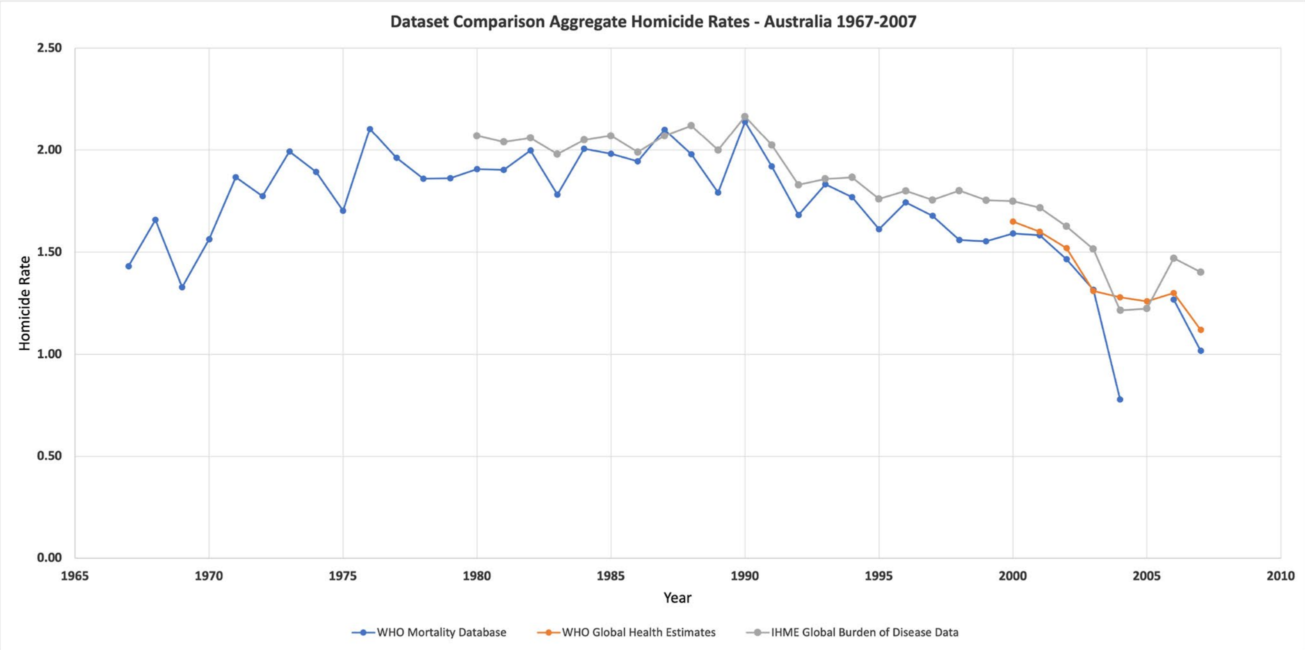


FIGURE A14 Comparison of World Health Organisation (WHO) and Institute for Health Metrics and Evaluation (IHME) data on aggregate homicides for Australia. [Colour figure can be viewed at wileyonlinelibrary.com]

10991050, 2024, 2, Downloaded from https://onlinelibrary.wiley.com/doi/10.1002/hec.4769 by Info Access & Delivery Service, Wiley Online Library on [19/12/2024]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License