

A systematic review of quantitative evidence about the impacts of Australian legislative reform on firearm homicide



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ABSTRACT

Developing legislative interventions to address firearm misuse is an issue of considerable public policy interest across many countries. However, systematic reviews of evidence about the efficacy of legislative change in reducing lethal firearm violence have only considered research examining the United States of America, a country that is unique among developed nations in its approach to firearm ownership. To inform international policy development, there is a need to consider other countries' experiences with gun law amendments. The current study used systematic literature search methods to identify evaluation-focused studies examining the impacts of legislative reform on firearm homicide in Australia, a country that made significant changes to its gun laws in the mid-1990s. Five studies met the inclusion criteria. These examined various different time periods, and used a range of different statistical analysis methods. No study found statistical evidence of any significant impact of the legislative changes on firearm homicide rates. The strengths and limitations of each study are discussed. Findings from this review provide insights into strategies and policies that may, and may not, be effective for reducing lethal firearm violence.

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1. Introduction

Reducing levels of intentional lethal injury is an ongoing international objective within the spheres of both public health and crime and

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justice policy development. Firearm misuse, and the possible application of legislative interventions to address misuse, is a topic of ongoing interest for scholars, practitioners, and policymakers alike. The majority of studies concerning legislative interventions and firearm misuse have been undertaken in the United States of America (USA) (although with exceptions – see for example Blais, Gagne, & Linteau, 2011; Gjertsen, Leenaars, & Vollrath, 2014). The ability for legislation to reduce firearm homicides in the USA remains a source of considerable debate and inconsistent research findings. For instance, research has demonstrated the existence of temporal and geographic variation in terms of observed relationships between firearm ownership, legislation, and lethal gun violence (e.g., Fan, 2015; Fleegler, Lee, Monuteaux, Hemenway, & Mannix, 2013; Irvin, Rhodes, Cheney, & Wiebe, 2014; Kleck & Patterson, 1993; Kwon, Scott, Safranski, & Bae, 1997; Ludwig & Cook, 2000; McDowall, Loftin, & Wiersema, 1995; O'Carroll et al., 1991; Rosenfeld, 1995; Rosenfeld, Fornango, & Baumer, 2005; Webster, Crifasi, & Vernick, 2014), and disentangling potentially causal relationships between legislation and changes in firearm related deaths presents a notable challenge.

In an effort to better understand types of interventions that do, and do not, appear to work effectively to prevent firearm violence, a growing number of authors have applied systematic review methodology. However, this approach has, to date, only been applied to research that examines the USA (most likely due to the substantial body of research around firearm misuse in that country). There has emerged insufficient evidence from those reviews to conclude that legislative interventions have been effective in controlling violence. Rather, it has been suggested that proactive policing strategies and community-based crime prevention appear to offer one of the most promising means of reducing firearm violence (see for example Hahn et al., 2005; Makarios & Pratt, 2012).

Although such findings are of great interest for policy development, it must also be highlighted that the USA is unique among developed nations in its approach to firearm ownership, and stands alone in enshrining in its Constitution the right to bear arms. Given this very specific cultural context, findings from the USA about firearm legislation and homicide may not, therefore, be applicable to (or relevant for) other countries. Despite this possibility, there has been little effort made within scientific literature to systematically review research from countries other than the USA. This is a notable gap in knowledge, given that a selection of developed countries have undertaken epochs of significant firearm legislative change over recent decades, thus providing grounds for study. A number of Commonwealth countries, including the United Kingdom, Canada, and Australia, fit this description.

The United Kingdom introduced significant changes to its firearm legislation in 1988 and 1997 (e.g., Parliament of the United Kingdom, 1997). The 1988 laws placed restrictions on ownership of certain semi-automatic and pump-action rifles, as well as introducing 'secure storage' provisions and increasing the legislative requirements that had to be met in order to possess shotguns over a particular capacity. The 1997 changes included, most notably, an almost total prohibition on private ownership of handguns. Canada undertook major revision of its firearm legislation in 1977, 1991, and 1995 (Royal Canadian Mounted Police, 2012). Key changes were the introduction of the requirement to hold a 'Firearms Acquisition Certificate' or FAC (which involved passing a police background check), a ban on fully automatic firearms, and penalties for unsafe storage (1977), mandatory safety tests for a FAC, 28-day waiting periods, registration of semiautomatic military-style firearms, and bans on 'high-capacity' magazines (1991), registration of all long-guns (rifles and shotguns) and bans on various handguns and "paramilitary" firearms (1995). Australia followed many of Canada's policies – particularly around licensing and long-gun registration requirements – when that country introduced strict firearm regulations in 1996 (discussed in further detail below).

In keeping with the research inconsistencies found in the USA, there exists in those countries a level of disagreement about the impacts that firearm legislative reform can be expected to have. In Canada, for

instance, there has been dispute over what outcomes successive legislative changes (especially those made in the 1990s) could be expected to deliver in terms of public safety benefits. While some argue that significant public safety benefits may arise from increased firearm legislation, in the form of reduced accidental deaths and homicides (e.g., Chapdelaine & Maurice, 1996; Fisher & Drummond, 1999), others have questioned this and highlighted conceptual and methodological issues such as method substitution and endogeneity bias (e.g. Kates & Mauser, 2007; Kovandzic, Schaffer, & Kleck, 2005; Mauser, 2007). The United Kingdom, also, has not found consensus on this issue; Adsheed, Fonagy, and Sarkar (2007) and Smith (2006) provide thoughtful overviews of factors driving that debate. In particular, views about the prevalence of gun violence, and perceptions about what firearms 'symbolize', have been identified as contributors to ongoing disagreement.

Turning to the specific example of Australia, firearm laws in that country have been described by some as being among the most stringent in the Western world (e.g., Baker & McPhedran, 2007). Table 1 provides an overview of the most significant policy changes that occurred in Australia as part of its 1996 National Firearms Agreement (NFA). That Agreement was introduced in reaction to a public mass shooting in which 35 people were killed; however, its purpose was described in the much broader context of improving overall community safety with the then-Prime Minister of Australia stating the scheme was designed to "... reduce the number of guns in the community and make Australia a safer place to live" (Howard, 1996). This objective was echoed by the then-Attorney-General of Australia, who commented that the laws offered "... the real chance of a safer festive season and New Year" (Williams, 1996). It should be noted that the NFA was not a piece of legislation, but an agreement between all Australian states and territories to adopt a consistent set of firearm management principles into their own legislation and regulation. This reflects both Australia's federated system, and the constitutional division of powers through which states and territories rather than the federal government, have responsibility for firearm legislation.

Leading up to the time of the 1996 restrictions, it was generally argued that the legislative changes would have impacts on all types of firearm-related deaths. For instance, governments had for some years been urged to implement more restrictive firearm legislation as a suicide prevention measure (Cantor, 1992; Hassan, 1996; Public Health Association of Australia, 1992) with specific reference made to gun laws being a promising 'method restriction' approach to addressing rising youth suicide rates (Dudley, Waters, Kelk, & Howard, 1992). Also, it was broadly assumed that restricting legal firearm ownership would correlate meaningfully with increased public safety and reduced firearm

Table 1
Overview of Australian legislative change.

Jurisdiction	Policy change	Key features
All Australian states and territories	National Firearms Agreement (agreed to in 1996 – principles subsequently adopted into legislation by states and territories by the end of 1997)	Prohibition of self-loading rifles, and self-loading and pump-action shotguns Mandatory safety training 'Secure storage' of firearms when not in use Police background check Photographic license Registration of all firearms 28 day waiting period for issue of license License holders must have a 'genuine reason' for ownership (self defense prohibited) Licensing based on different "categories" of firearm All firearm acquisitions must be approved by police via a 'permit to acquire'

misuse, including in the context of homicide (National Committee on Violence, 1990). However, in the late 1990s research suggested that the NFA may have been successful in reducing firearm suicides, but ineffective for other firearm-related deaths (Reuter & Mouzos, 2003).

Since that time, various studies have drawn different conclusions about the impact of the changes (McPhedran & Baker, 2008 provide an overview of possible reasons for the divergent conclusions). In terms of broader public debate in the international sphere, however, it has been put forward in popular political discourse that Australia's approach to firearm management has been highly effective in reducing firearm homicide, and represents a policy model that other countries should follow if they seek to reduce the occurrence of firearm-related deaths (e.g., Obama, 2015). To date, though, there has not been rigorous examination of the extent to which existing evidence may support – or conflict with – these suggestions for international policy development.

It has been noted that firearm ownership has become a highly politicized issue in many countries (Smith, 2006), and that it can be challenging to develop genuinely effective interventions when legislation is drafted within an emotive and/or politicized context (Baker & McPhedran, 2007). Consequently, to support well informed policy decisions, it is desirable to identify points of commonality and difference between existing studies, and to discern whether any degree of consensus and consistency might be found. To facilitate this process of evidence-based policy development, it is therefore necessary to undertake a systematic review of studies that have evaluated the impacts of legislative change on firearm homicide deaths in Australia.

Australia particularly lends itself to a study of this nature. Not only were its laws changed in the same way in all jurisdictions during the same time period (1996–1997), but all legislative changes were implemented during that period rather than 'staggered' over time (as was the case in Canada, for example) or implemented at different times in different jurisdictions. Also, Australia's relative geographical isolation and level of border control suggest that factors which may act as policy and/or evaluation confounds in other nations – such as cross-border transfer of firearms from states or countries with less restrictive jurisdictions – are far less likely to apply. This means that the Australian situation may reasonably be conceived of as a comparatively 'pure' indicator of legislative impacts, relative to what may emerge in other countries.

The current paper sought, firstly, to systematically identify relevant studies, and consider whether any consistent observations about firearm homicide can be found across those different studies. Second, it undertakes a critical review of each paper to identify strengths and limitations, in order to inform future study.

2. Methods

2.1. Search strategy and inclusion/exclusion criteria

A systematic search of English-language peer-reviewed published articles considering Australian firearm homicide was undertaken in Web of Knowledge (all databases, and incorporating Medline), with supplementary searches made of the Cochrane Collaboration library and peer-reviewed 'grey literature' (e.g., government reports which are subject to peer review, but published through government outlets, rather than traditional academic outlets). For inclusion in this study, papers were required to:

- Contain original quantitative data analysis (i.e., not be a summary, re-presentation, or replication of previously published work, 'letter to the editor,' opinion piece, literature review, legal analysis, media analysis, or the like);
- Focus specifically on firearm homicide in Australia;
- Include time series data; and
- Use formal statistical methods to detect legislative impacts/change over time.

These requirements were designed to ensure that the review was able to directly interrogate the specific research question of whether Australia's legislative changes impacted on firearm homicide rates, from a quantitative, evaluation-focused perspective. Fig. 1 summarizes the search strategy and provides step by step details of papers excluded as the criteria above were successively applied.

From examination of the papers that met the inclusion criteria, it was immediately apparent that those papers typically presented insufficient statistical detail to enable formal meta-analysis. The level of statistical detail supplied in each paper differed substantially. In some instances, details such as effect sizes, confidence intervals, and mean firearm homicide rates were not included, nor was sufficient information made available to independently calculate those. Given the lack of detail and small number of studies, reliable meta-analysis could not feasibly be undertaken. However, it is important to note that the greatest value of formal meta-analysis lies with assisting to resolve uncertainty when individual studies yield inconclusive or conflicting statistical results (Bartolucci & Hillegass, 2010); as detailed in the following sections, this was not found to be the case with the studies placed under review. Nevertheless, given the inability to apply formal meta-analysis, a 'statistical overview' was used to augment the general critique, whereby each paper's key statistical findings for firearm homicide deaths were extracted and summarized, and categorized as either statistically significant or not statistically significant. This strategy enabled identification of general statistical consistencies and differences between studies. It must also be highlighted that this review follows accepted systematic procedures (Higgins & Green, 2011) recognized as sufficiently rigorous to allow research synthesis and comparative consideration of different studies (e.g., Campbell Collaboration, 2016; Cooper & Hedges, 2009; Eden, Levit, & Bird, 2011). As such, this study represents a formal evaluation that can be used to address the question of whether legislative changes have made a difference to firearm homicides in Australia.

3. Results

3.1. Overview

Table 2 summarizes the papers that were included in the review. All studies used the same dependent variable (firearm homicide rates) and the same, publicly available, data source (Australian Bureau of Statistics 'Causes of Death' data, which consists of data about all Australian deaths annually, from the Registrar of Births, Deaths and Marriages and the National Coronial Information System; for further detail, the reader is referred to the Australian Bureau of Statistics, Catalogue No. 3303.0). All studies adopted a primarily observational experimental design using a time series of cross-sectional records.

Although all papers attempted to introduce some form of proxy experimental controls (such as comparing trends in firearm and non-firearm homicide rates over time, or comparing one jurisdiction against other jurisdictions), as a result of the national implementation of legislative reform within a relatively short space of time, no study was able to compare long-term post-1996 firearm homicide trends in a jurisdiction which had, versus had not, adopted the NFA. Although an unavoidable consequence of the nature of the policy being tested, this limitation also means that no study has been able to fully test the impacts of Australia's legislative reforms from a case-control perspective. With regard to the quality of evidence available, Chapman, Alpers, Agho, and Jones (2006) reasonably note:

... as it would be politically almost inconceivable that any government would conduct a randomized controlled trial of gun law effects, the evidence presented must be among the best that could ever be available to deal with the effects of such law reform (p. 366).

The papers have varied in terms of specific analysis methods applied, time periods subject to analyses, and populations/samples examined.

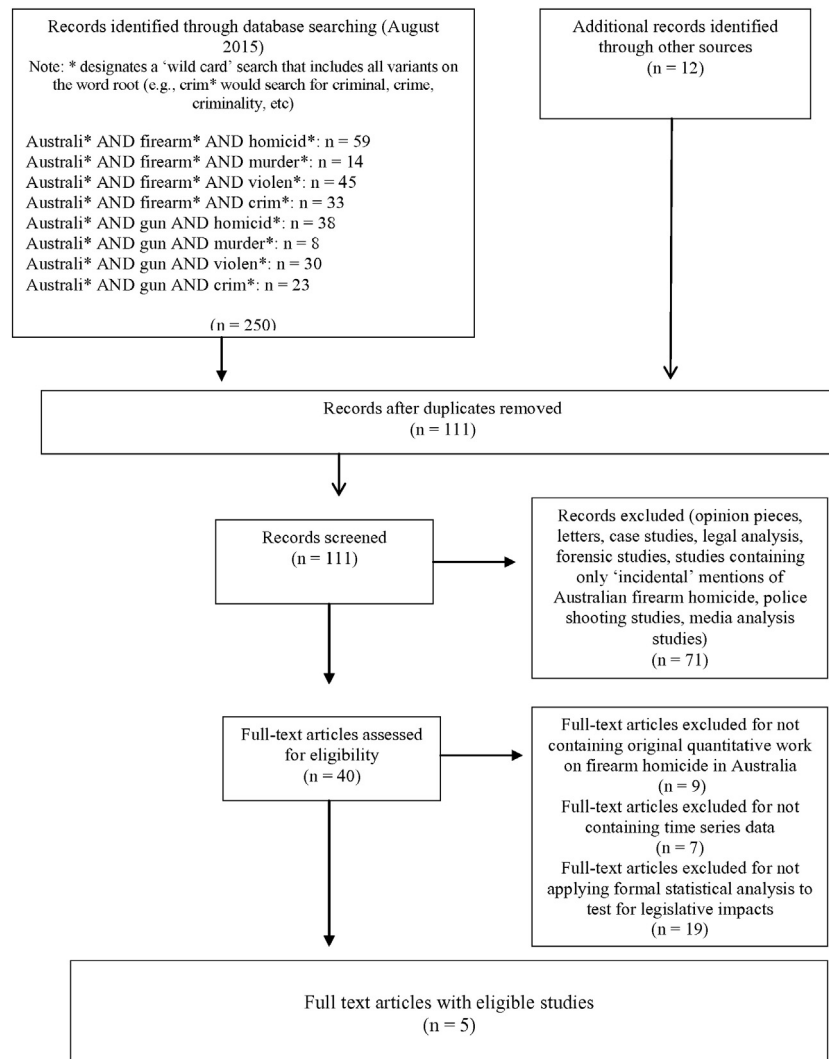


Fig. 1. Identification of studies for inclusion – flow chart.

With regard to analysis method selection, a range of justifications are provided. These include: avoiding assumptions of linearity with time (Baker & McPhedran, 2007), dealing with overdispersion (Chapman et al., 2006), the value of using a battery of different tests (Lee & Suardi, 2010), the need to exploit sub-national variations (Leigh & Neill, 2010), and for reasons not explicitly specified but potentially due to being “convenient in practice” (Ozanne-Smith, Ashby, Newstead, & Clapperton, 2004, p. 283). Although the analysis methods varied across studies, the included papers (with the possible exception of Ozanne-Smith et al., 2004) provide plausible justifications for the methods selected. Each different method is well established within research literature more generally, and each represents a reasonable means of approaching the study questions of particular interest within each individual paper. However, specific methodological limitations of each analytic strategy – and of each paper more generally – are discussed further in sections below.

3.1.1. Findings of each paper

As Table 2 demonstrates, none of the papers reviewed found evidence for a statistically significant impact of the legislative intervention on firearm homicide rates. Nor did any of the values reported in each paper approach the 0.05 level of statistical significance that was adopted across all studies. Baker and McPhedran (2007) reported little difference in mean predicted and observed firearm homicide rates post-1996, with rates of 0.28 and 0.27 deaths per 100,000 population, respectively, a standard error of 0.01 and a model R^2 value of 0.52. Similarly, Chapman et al.

(2006) reported a rate ratio of 0.955 (95% CI: 0.897–1.016). Lee and Suardi (2010) did find structural changes in the time series under examination, however those changes did not coincide with the implementation of the legislative changes, occurring instead at 1951–1953 (Quandt test; test statistic critical value 12.93) and 1951 and 1987 (Bai Sequential Multiple Breaks test; 5% significance level). Bai and Perron tests for multiple breaks at unknown points did not identify structural breaks in the firearm homicide time series. Leigh and Neill found a relationship coefficient of -0.044 between the number of firearms handed in and firearm homicide rates (t statistic = 0.54; p value = 0.608; $R^2 = 0.0464$; the average death rate for the period 1990–1995 was given as 0.43 deaths per million people, however the average death rate for the period 1998–2003 was not provided). Ozanne-Smith et al. (2004) provided only a p value (0.108) for their analyses of firearm homicide, with no further information made available.

3.2. Specific methodological strengths and limitations of individual papers

3.2.1. Baker and McPhedran (2007) and Chapman et al. (2006)

Using similar time series and methods, the studies of Baker and McPhedran (2007) and Chapman et al. (2006) also have similar limitations, and hence are considered together for the purpose of this review. Although both studies have the strength of examining and comparing pre- and post-1996/97 trends in firearm homicide – which accurately reflects the relevant policy question about legislative impacts – both

Table 2
Summary of papers, methods and available information used in each paper, and main findings.

Study	Geographic coverage	Statistical method/s	Research focus	Time period	Dependent variable	Experimental design	Data source/s	Available statistical information and main findings for firearm homicide
Baker and McPhedran (2007)	Whole of Australia	ARIMA, paired samples t-tests	Did trends differ pre- and post-1996?	1979–2004	Firearm homicide rates (per 100,000)	Observational/cross-section	Australian Bureau of Statistics	Mean predicted rate (per 100,000) post 1996: 0.28 Mean observed rate (per 100,000) post 1996: 0.27
Chapman et al. (2006)	Whole of Australia	Negative binomial regression	Did trends differ pre- and post-1997?	1979–2003	Firearm homicide rates (per 100,000)	Observational/cross-section	Australian Bureau of Statistics	p = 0.14 (n.s.) Trend before 1997: Rate Ratio (CI) = 0.971 (95% CI: 0.958–0.984) Trend after 1997: Rate Ratio (CI) = 0.925 (95% CI: 0.881–0.973) Ratio of slopes: Rate Ratio (CI) = 0.955 (95% CI: 0.897–1.016)
Lee and Suardi (2010)	Whole of Australia	ARIMA, Quandt (Chow), Bai, Bai and Perron	Were there changes in the time series structure?	1915–2004	Firearm homicide rates (per 100,000)	Observational/cross-section	Australian Bureau of Statistics	p = 0.15 (n.s.) Quandt: break at 1951–1953 (p < 0.05, sig.) Bai: structural breaks at 1951 and 1987. Bai and Perron: UDMax = 3.97, critical value = 8.88 (n.s.) WDMax = 4.72, critical value = 9.91 (n.s.)
Leigh and Neill (2010)	Whole of Australia, based on jurisdiction-level data	Linear regression Difference between averages for 1990–95 and 1998–2003.	What was the estimated effect of the number of guns handed in, on firearm homicide?	1990–2003	Firearm homicide rates (per million)	Observational/cross-section	Australian Bureau of Statistics; Reuter and Mouzos (2003)	1990–1995 average death rate (per million) = 0.43 Implied change in death rate 1998–2003 (per million) = –0.16 Lower limit of 95% CI for death rate = –0.9 Upper limit of 95% CI for death rate = 0.5
Ozanne-Smith et al. (2004)	Focus on one Australian state (Victoria); comparisons performed against rest of Australia	Poisson regression Three different time periods (1979–1987, 1988–1996, 1997–2000)	Did trends differ, between the different time periods?	1979–2000	Firearm homicide rates (per 100,000)	Observational/cross-section	Australian Bureau of Statistics	p = 0.608 (n.s.) p = 0.108 (n.s.) – no further statistical data provided.

Note: sig denotes that the result was statistically significant; n.s. denotes that the result was not statistically significant.

studies use the point of the intervention (1996/97) as a pre-determined break in the time series. In doing so, the analyses in both studies are unable to take into account the possibility that legislative impacts may only have become apparent at a point more distant into the future (i.e., a 'lag' between implementation and impacts). While the analyses used were appropriate for time series data, they lacked the necessary sensitivity to detect such a lag, if it was present in the data. In addition, neither study was able to differentiate between short-term impacts (i.e., a sudden 'one off' change), versus sustained change over time.

3.2.2. *Lee and Suardi (2010)*

Using the lengthiest time series out of all studies included in the review, and a battery of different statistical tests, this study provides detailed econometric analysis of aggregated Australian firearm homicide trends. The methods are appropriate for detecting any structural breaks in those trends, rather than presupposing that impacts would occur at any particular time point; this enables identification of impacts that may have occurred as a possible result of the 1996 legislative reforms, but which may not have been apparent if that point was arbitrarily imposed as a break in the data. Among all studies considered, this paper arguably provides the most detailed analysis of firearm homicide rates in Australia over time, including identification of pre-1996 breaks in the time series. However, like Baker and McPhedran (2007) and Chapman et al. (2006), the study considers death rates within Australia as a whole rather than by individual jurisdictions. While not detracting from the analyses, this limitation means that geographically specific impacts of legislation could have occurred, but not been large enough to have been detected when only national homicide rates were considered.

3.2.3. *Leigh and Neill (2010)*

The study provides detailed econometric modeling and has the strength of considering individual jurisdictions, rather than only looking at Australia as an aggregated whole. Relative to other studies, the paper uses the shortest amount of pre-1996 data (1990–1995) meaning that its estimates of trends in firearm homicide before the legislative changes are also the most likely to be affected by short-term fluctuations. Unlike other papers, the study did not focus on impacts of legislative reform, as such, but examined associations between numbers of firearms handed in to state authorities during the 1996–1997 'buyback' program (which represented one component of the overall changes), and firearm homicide rates. The study relies on assumptions that the number of firearms surrendered equates proportionally to changes in firearm ownership levels (that is, it is assumed that higher numbers of firearms handed in equals a greater reduction in total levels of ownership in a given jurisdiction). Testing that assumption necessarily requires knowledge about pre- and post-1996 levels of ownership, however, reliable administrative data concerning pre-1996 firearm ownership levels are not used in the study, with data instead drawn from point-in-time self-report survey data from the International Crime Victimization Survey, as provided in Reuter and Mouzos (2003), for the years 1989 and 1992 (this may reflect an absence of pre-1996 record keeping by state authorities). Hence, the analyses lack information about what the raw number of firearms handed in translates to, in terms of actual percentage ownership reduction. Consequently, the study is unable to control for differences between jurisdictions in terms of the percentage reduction in firearm ownership following the legislative changes.

3.2.4. *Ozanne-Smith et al. (2004)*

This paper adopts a novel and useful approach – taking advantage of some pre-1996 legislative changes in Victoria in order to use three time periods rather than two as most other studies did, and attempting through this to approximate a case-control approach. However, the lack of detail provided about firearm homicide trends within each of the three time periods poses a significant challenge for interpretation

of the results. While examining a single state rather than Australia as a whole provides more geographically-specific detail than most other studies included in this review, the absence of comparative data from other individual jurisdictions means that any effects that may have been unique to Victoria during its first period of legislative change cannot be reliably identified. The aggregation of all other Australian jurisdictions into one group provides not only a far greater total number of firearm homicides than that obtained for Victoria (i.e., very unequal sample sizes), but also draws on assumptions about the homogeneity of each other jurisdiction in regards to historical firearm homicide trends. It is therefore open to question whether comparing Victoria with the 'rest of Australia' provides an appropriate comparative approach for examining legislative impacts on firearm homicide.

3.3. *Justification of time periods selected for analysis*

As Table 2 shows, studies included in this review analyzed death rates over a variety of different time periods. In addition to identifying any methodological implications of particular note (above), studies were also scrutinized for any rationale they may have provided for the particular time periods selected for analysis. With regard to the pre-law change periods selected, three studies (Baker & McPhedran, 2007; Chapman et al., 2006; Ozanne-Smith et al., 2004) used a time series commencing with the year 1979. Although no explicit justification is given for that choice by any of the three studies, two studies (Chapman et al., 2006 and Ozanne-Smith et al., 2004) mention that cause of death codings were based on the International Classification of Diseases ninth edition (ICD-9), which commenced in 1979. Lee and Suardi (2010) state that the commencement point of their time series (the year 1915) was selected to maximize their sample size, while Leigh and Neill (2010) do not state any particular reasons for their selection of 1990 as the commencement point.

With regard to post-intervention data, three out of the five studies (Baker & McPhedran, 2007; Chapman et al., 2006; Ozanne-Smith et al., 2004) appear to have made their selection based solely on the practical consideration of what was the longest time series available from the Australian Bureau of Statistics, at the time each study was conducted. Lee and Suardi (2010) selected the year 2004 as the analysis end-point, to replicate the end-point used in prior studies. Leigh and Neill (2010), although one of the most recently published papers in this review, analyzed only a short post-reform time series (1998–2003); the reasons for this are not stated.

In a more general sense, it could be reasonably suggested that the different choice of time period adopted by different studies may have affected the results obtained. However, the range of different time periods examined by the studies under consideration did not appear to materially impact on the consistency of the findings that emerged.

3.4. *Identification of possible external/non-legislative influences on results obtained*

Studies included in this review did not routinely identify potential explanations for the general absence of significant findings. The study of Baker and McPhedran (2007) notes that a wide range of external factors may contribute to homicide rates (firearm and non-firearm), and suggests that those factors may explain the lack of significant impacts observed for firearm homicide following legislative reform. Other studies are generally silent on this issue.

3.4.1. *Multivariate analyses/inclusion of control variables*

Two studies – Lee and Suardi (2010) and Leigh and Neill (2010) attempted limited multivariate analyses, controlling for (respectively) size of police force and incarceration rates, and unemployment rate, percentage of urban population, and percentage of population in certain age groups. Both studies identify the difficulty of obtaining comprehensive and appropriate control variable data, particularly at the state –

rather than national – level (Leigh & Neill, 2010), and both apply strong interpretive caution to their analyses as a result of that difficulty.

3.5. Treatment of questions concerning displacement/method substitution

Four out of the five studies included in this review (the exception being Ozanne-Smith et al., 2004) considered non-firearm homicide deaths, and acknowledge the possibility of displacement (or substitution) from the use of firearms to other methods of homicide. None of those four studies found strong evidence for the occurrence of displacement; Baker and McPhedran (2007) observed little change in non-firearm homicide rates post-1996 (predicted death rate = 1.39, observed death rate = 1.30, $p = 0.08$), Chapman et al. (2006) found a marginally significant decline in non-firearm homicide post-1997 (rate ratio = 0.965; $p = 0.05$), Lee and Suardi (2010) reported a single structural break in the non-firearm homicide time series (at 1950, using the Bai test, no further details provided), and Leigh and Neill (2010) found an association of -0.115 between guns handed in and non-firearm homicide rates ($t = 0.45$; $p = 0.671$; $R^2 = 0.0322$). Leigh and Neill (2010) flag the possibility that those findings may have occurred due to the larger numbers of non-firearm, relative to firearm, homicides (that is, it is possible that a small increase in non-firearm homicides may have occurred due to the firearm legislative changes, but as a result of sample sizes and variability in non-firearm homicide rates, that may not have been readily discernible).

4. Discussion

Using systematic review methods, it emerged that a relatively small number of studies to date have specifically examined the impacts of Australia's legislative change on firearm homicide, using time series analyses that lend themselves to good quality policy evaluation. However, irrespective of the differences between papers in methodology, time periods examined, and level of geographical disaggregation, none found evidence for a statistically significant impact of Australia's 1996 legislative changes on firearm homicide rates.

Although each study contained limitations, given the variety of different statistical methods and time periods used in the studies examined it seems unwise to dismiss the general replication of statistical findings across all studies as being a result of statistical artefact or Type II error associated with one particular method. This is also reflected in the individual studies' discussions of limitations; none of the studies reviewed flagged a lack of statistical power as a probable reason for their findings. While authors of some USA-based studies into the impacts of firearm legislation have, when interpreting non-significant findings, adopted the stance that it is more probable that effects are present but not detected, than not present at all (e.g., Wintemute, Hemenway, Webster, Pierce, & Braga, 2010), this approach overlooks an absolutely fundamental premise of scientific practice and inferential statistics: the assumption that the null hypothesis – or hypothesis of no effect – is 'true' until sufficient statistical evidence indicates otherwise (e.g. Fisher, 1956; Neyman & Pearson, 1933). The present review therefore follows these long-accepted scientific principles, and concludes that there is no evidence within the accumulated studies to reject the assumption of no effect and support the belief that the legislative interventions had an impact.

This outcome has noteworthy implications. While there have been suggestions made in popular debate that legislative reforms in Australia have been very effective in reducing firearm misuse (e.g. Beazley, 2013; Obama, 2015), it appears that evaluation-focused empirical studies do not validate those assertions. It may be reasonable to suggest that policy changes may have been 'effectively achieved', in the sense of implementing changes in processes around regulating firearm ownership, but seems prudent to avoid equating the *process* of legislative and policy change with *outcomes* resulting from that process of change.

The current findings raise two key questions. The first question is why a very significant set of legislative interventions have had no clearly demonstrable impact on firearm homicide rates in Australia. From a theoretical perspective, the possibility of misspecification of 'high risk' populations and/or inappropriately designed policy should be considered. For instance, legislative reform has typically had at its foundation the premise that reducing levels of access to firearms will lead to reductions in firearm misuse. However, research has indicated strong connections between illegal firearm ownership and firearm homicide (Dauvergne & De Socio, 2008; Davies & Mouzos, 2007; Dearden & Jones, 2008; Mouzos & Houliaris, 2006), and between illicit ownership, firearm homicide, and the illicit drug trade (Fitzgerald, Briscoe, & Weatherburn, 2001), as well as identifying particular 'high-risk' typologies for lethal domestic violence that predict homicidal behavior irrespective of firearm access (Folkes, Hilton, & Harris, 2013). This appears to validate suggestions by Reuter and Mouzos (2003) that interventions focused on restricting legal firearm ownership overall may primarily target 'low-risk' populations. If this is correct, then policies designed to increase restrictions on 'low-risk' populations, rather than to specifically address firearm access among 'high-risk' populations for firearm violence (such as disadvantaged young males involved in drug-related activity), or 'high-risk' populations for homicide offending regardless of method (such as highly maritally violent males), would not be expected to greatly affect firearm homicide rates.

The second question is that if legislative reform is limited in its ability to reduce firearm homicide rates, then what interventions are most likely to be successful? To date, this question has not been empirically addressed within the Australian context. The studies included in this review, although at times acknowledging the complex network of factors associated with violence, included no evaluation of alternative policies to legislative reform. Indeed, there appear to have been few strategies implemented in Australia, other than legislation, which are specifically aimed at curbing firearm violence.

Potentially, given associations between various measures of social disadvantage and firearm homicide rates (e.g., Langmann, 2012), interventions to address social and economic disadvantage and to improve social equality may be an effective way of reducing firearm homicide (and, indeed, homicide more broadly). To this end, future work evaluating legislative changes should ideally incorporate demographic, socio-economic and related indices that can reasonably be expected to relate to firearm violence, as well as measures of societal and individual wellbeing. As noted previously, however, the majority of work examining effective interventions has been undertaken in the United States, and it is unclear whether findings about strategies such as proactive policing and community-based crime prevention programs (Hahn et al., 2005; Makarios & Pratt, 2012) are transferable to other countries. Again, there is little international systematic and/or meta-analytic research in this area.

A limitation of the current work was that it was based on a relatively small number of studies. This can, in some cases, be linked with gaps in data collection. Australian research has historically suffered from significant limitations in available data (such as an absence of time series homicide data disaggregated by victim-offender relationship status, which would be useful in examining relationships between firearm legislation and domestic homicides). Although an unavoidable limitation, the relatively small number of papers reviewed does not detract from the ability of this paper to provide novel insights into the experiences of countries outside the USA, that have undertaken significant periods of firearm legislative reform.

This review also highlights the general absence of studies which undertake detailed consideration of whether specific elements of legislative change – rather than legislative change overall – may have had effects that were not apparent from the overall firearm homicide trends. For example, it is possible that measures such as exclusion from lawful firearm ownership for persons with a history of domestic violence may have affected rates of female-victim firearm homicide. Alternatively,

there may be other legislative measures which have not had any particular effects on firearm homicide. This flags the value of conducting more in-depth examination of which elements of Australia's overall legislative regime may and may not have been effective, in relation to which sub-categories of firearm homicide (e.g., intimate partner homicides, versus homicides between acquaintances). There would also be clear value in undertaking detailed multivariate analyses of firearm homicide rates, which attempt to control for the influence of multiple social and economic correlates of lethal violence.

4.1. Conclusion

The current review – although based on a relatively small number of papers – adds to the body of international criminological evidence concerning different legislative and policy approaches to reducing firearm homicide. The information provided in this paper suggests that when the current evidence base is considered in a systematic fashion, there may be a notable discrepancy between empirical findings about the efficacy of Australian firearm legislation in regard to reducing firearm homicide, compared with what has been proposed within popular discourse about the impacts of those laws. This observation can in turn inform a more evidence-based debate and support rigorous policy development.

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