Evaluation of Ontario’s Drinking & Driving Countermeasures

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This report was prepared by Tracey Ma, Patrick Byrne, Nathalie Chernoff, and Yoassry Elzohairy.
EXECUTIVE SUMMARY

This report comprises a complete summary of a two-year quantitative evaluation undertaken by the Ministry of Transportation’s Road Safety Research Office to assess the impact of legislative and regulatory countermeasures implemented by the Government of Ontario to reduce alcohol-impaired driving in the province. A variety of methodologies were used, each designed to isolate the effects of a particular countermeasure from other concurrent and/or overlapping measures. Together, these analyses provide a comprehensive assessment of the strengths and weaknesses of each measure. Although much research has been done on individual drinking and driving countermeasures across the globe, this assessment of every countermeasure enforced by a single large jurisdiction is the first of its kind. We hope that these findings will provide a thorough evidenced-based framework to guide future research and policy considerations.

OVERVIEW OF COUNTERMEASURES

Ontario’s drinking and driving countermeasures target novice-classed drivers, young drivers, and members of the general driving population who drive with unsafe blood alcohol concentrations (BAC). Some of these countermeasures involve sanctions that are administered upon detection of an offence, while others are contingent upon conviction for an offence. The evaluated countermeasures are:

- 90-day Administrative Driver’s Licence Suspension
- Warn Range Sanctions
- Long Term Vehicle Impoundment
- Seven-day Vehicle Impoundment
- Zero BAC Requirements
- Alcohol Education and Treatment Remedial Measures Program
- Ignition Interlock Program
- Reduced Suspension with Ignition Interlock Conduct Review Program

OVERVIEW OF EVALUATION

For each of the eight countermeasures, specific research questions were defined based on the intended and/or expected impact of that countermeasure. The resulting questions can be divided into two broad categories: those investigating general deterrence (i.e., effects of a countermeasure’s existence on the behavior of all drivers) and those investigating specific deterrence (i.e., effects of a countermeasure on the
behaviour of those drivers who have been subject to it). Questions of general deterrence were typically addressed using collision outcomes, such as the number of drivers involved in alcohol-related collisions, or the number of injuries and fatalities resulting from such collisions. Questions of specific deterrence were typically addressed with behavioural outcomes, such as repeated drinking and driving incidents (roadside sanctions and/or later convictions) and driving while suspended incidents. Research questions were addressed using a mix of study designs and their corresponding analytic techniques. Data were derived primarily from the Ministry of Transportation’s Licensing Control System and Accident Data System.

OVERVIEW OF RESULTS

90-day Administrative Driver’s Licence Suspension

- Since November 1996, a roadside 90-day administrative driver’s licence suspension is given to a driver who has a measured BAC > 0.08%, in violation of Criminal Code of Canada (CCC) s. 253, or who refuses to submit to screening, in violation of CCC s. 254.
- If existing trends were to have continued from the pre-countermeasure period, there would have been 90.4 road users per month\(^1\) presenting with major or fatal injuries resulting from an alcohol-related collision in the immediate post-implementation period. Instead, 71.6 such road users were observed per month post-implementation\(^2\), a 21% decrease.
- Before countermeasure implementation, there were 2.45 drivers committing a CCC s. 253 or 254 re-offence\(^3\) in the 90 days immediately after their initial offence per 100 offending drivers. Post-implementation, this number decreased rapidly by 66% to 0.82 re-offending drivers per 100 offending drivers.

Warn Range Sanctions

- Since May 2009, a roadside 3, 7, or 30-day administrative driver’s licence suspension is given to drivers with a measured BAC > 0.05%. Additional sanctions apply as well (see main report).
- If existing trends were to have continued from the pre-countermeasure period, there would have been

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1 Averaged over a five year post-implementation window.
2 All results reported in this executive summary are “statistically significant” at the \(\alpha = 0.05\) level, indicating they are not likely due to random chance.
3 For the purposes of this report, the term “offence” is defined as an action that lead to conviction, while “re-offence” is defined as the commission of an offence subsequent to a previous commission of the same offence, where both incidents eventually lead to conviction.
o 179 drinking drivers per month\textsuperscript{4} involved in an alcohol-related collision\textsuperscript{5} in the immediate post-implementation period. Instead, 152 such drinking drivers were observed per month post-implementation, a 15% decrease.

o 261 road users per month presenting with injuries and fatalities from an alcohol-related collision in the immediate post-implementation period. Instead, 217 such road users were observed per month post-implementation, a 17% decrease.

- The effect of escalating sanctions on a driver’s propensity to drink and drive could not be evaluated due to data limitations at this time.

\textit{Long Term Vehicle Impoundment}

- Since February 1999, a 45-, 90-, or 180-day vehicle impoundment is applied to a vehicle driven by a driver when that driver is detected driving during a criminal driving prohibition.

- The implementation of this countermeasure was associated with a 19% decrease in the number of criminal driving while prohibited (for CCC s. 253 or 254) offences committed by previous offenders\textsuperscript{6}. This corresponds to a reduction from 23.0 offences per 100 years of criminal driving prohibition immediately before implementation to 18.6 immediately after.

\textit{Seven-day Vehicle Impoundment}

- As of December 2010, a seven-day vehicle impoundment is applied to the vehicle driven by a person when that person receives a 90-day Administrative Driver’s Licence Suspension.

- The implementation of this countermeasure was associated with a
  o 33% decrease in the proportion of drivers who were convicted for driving during (and in violation of) their first 90-day administrative driver’s licence suspension. This corresponds to a reduction from 2.12 drivers committing a driving while suspended offence per 100 drivers receiving a first 90-day suspension--immediately before implementation--to 1.42 immediately after.
  o 29% decrease in the proportion of drivers who received a second 90-day suspension in the three months immediately after the end of their first 90-day suspension. This corresponds to a reduction from 0.98 drivers incurring a second 90-day suspension per 100 drivers who had not yet been (or never were) convicted of the original charge, to 0.71 such drivers per 100.

\textsuperscript{4} Averaged over a 2.5 year post-implementation window.
\textsuperscript{5} Property damage only collisions were excluded
\textsuperscript{6} A previous offender in this instance is someone who was convicted of a driving while prohibited offence in the last 5 years.
Zero BAC Requirements

- As of August 2010, drivers under 22 years of age, or who are novice-classed drivers are required to maintain a zero BAC while driving.
- The implementation of this countermeasure was associated with a significant decrease in the number of 90-day administrative driver’s licence suspensions and Warn Range suspensions incurred by young drivers.
- Those subject to the Zero BAC suspension as a result of a conviction were less likely to incur a subsequent warn range or 90-day suspension compared to equivalent drivers who were charged but not convicted, at least for a specific group of young/novice drivers.

Alcohol Education and Treatment Remedial Measures Program

- Since September 1998, drivers convicted of a criminal drinking and driving offence are required to undertake alcohol education and/or treatment courses before their licence can be re-instated.
- The remedial measures program, including indefinite suspension for those who did not complete the program, was associated with decreased drinking and driving recidivism that could not be explained by pre-existing recidivism trends.
- The more-involved program (encompassing assessment, education or treatment, and follow-up components) was associated with a larger effect than the single-component program (education-only) in reducing recidivism.
- Program effectiveness was not observed for drivers over 45 years old, suggesting a need to tailor the program to the demographic characteristics of participants.

Ignition Interlock Program

- As of December 2001, after the end of a driving prohibition period, drivers convicted of a criminal drinking and driving offence are required to install an approved ignition interlock device in any vehicle they wish to drive for a prescribed period of time.
- The number of drivers who were detected and charged criminally for drinking and driving during the period of their ignition interlock condition was 2.49 times higher for those who did not instal the device compared to those who did install, even after groups were made equivalent.
- No effect on recidivism was observed after the end of the ignition interlock condition.
Reduced Suspension with Ignition Interlock Conduct Review Program

- Starting August 2010, by agreeing to install an ignition interlock device and not “sit out” during an interlock condition (along with other conditions), a driver convicted of a criminal drinking and driving offence for the first time can have their driving prohibition period reduced.
- Installation of the ignition interlock device increased by 54% at the time of program implementation.
- The time period between detection of a drinking driver and subsequent conviction (for those drivers who were convicted) decreased by 146 days at the time of program implementation.

Our results echo the transportation safety literature in demonstrating that immediate and certain countermeasures, which, in Ontario, are administratively applied under the Highway Traffic Act (HTA) by the police at the time that the driver is stopped at roadside, are effective at preventing drinking and driving in both the general population and in the population of previously detected drinking drivers. Increasing countermeasure severity, on the other hand, is effective primarily in preventing the repetition of drinking and driving behavior for those who have been detected.

Approximately 89% of drinking drivers involved in injury/fatality collisions in Ontario in 2012 had no history of criminal drinking and driving convictions over the previous ten years. Moreover, 87% of such drivers had received no administrative roadside suspension for BAC > 0.08%. This strongly suggests that the largest road safety gains can be made by focusing on the general deterrence of drinking and driving in the population of persons who have not previously been detected driving while impaired. Our evaluation suggests that administrative sanctions, which are immediate, certain, and target novel driver subgroups and/or circumstances, are effective general deterrents when combined with public awareness campaigns and high visibility enforcement. Any new drinking and driving countermeasures should satisfy these criteria. Similarly, any changes to existing countermeasures should be focused on increasing the immediacy and perceived certainty of consequences.

Remedial-type countermeasures such as the Alcohol Education and Treatment Remedial Measures program, Ignition Interlock Program, and the Reduced Suspension with Ignition Interlock Conduct Review Program should be simplified and harmonized with each other as much as possible for at least two reasons. First, programs with only a few well-communicated requirements that do not interact in complex ways with other
programs would simplify operational processes and likely increase participant adherence, a key requirement for program effectiveness. Second, the effectiveness of a program that interacts in multiple ways with other existing programs is difficult to evaluate. New gains in road safety will be more difficult to achieve without an accurate understanding of the effectiveness of current policies and programs, as ascertained by proper evaluation.

Finally, we suggest that the development and implementation of road safety countermeasures should include and prioritize an evaluation component, and, therefore, ensure that the proper infrastructure (e.g., reliable and valid data measures, data sources, and databases) are in place to enable such work. This is required to continue moving toward a culture of evidence-based policy making.
ABBREVIATIONS

ADLS   Administrative Driver’s Licence Suspension
ADS    Accident Data System (database)
BAC    Blood Alcohol Concentration
BIC    Bayesian Information Criterion (statistic criterion)
BOT    Back on Track (program)
CAMH   Centre for Addiction and Mental Health
CCC    Criminal Code of Canada
DUI    Driving Under the Influence
DWI    Driving While Impaired
DWP    Driving While Prohibited
DWS    Driving While Suspended
GLS    Graduated Licencing System
HTA    Highway Traffic Act
LCS    Licence Control System (database)
LCBO   Liquor Control Board of Ontario
MAG    Ministry of Attorney General
MTO    Ministry of Transportation
PDO    Property damage only (type of collision)
US     United States
Zero BAC Zero Blood Alcohol Concentration
ZT     Zero Tolerance
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INTRODUCTION

Drinking and driving imposes significant human costs on society. Between 2003 and 2012, 1670 people were killed and 33,628 were injured on Ontario’s roads in alcohol-related collisions (“Accident Database System,” n.d.). In 2012 alone, 143 lives in Ontario were claimed by alcohol-related collisions, which represents over one-fourth of total lives lost from all collisions (“Ontario Road Safety Annual Report” 2012).

Cognizant of this burden on the health and safety of the population, Ontario has committed to combating drinking and driving through the introduction and enforcement of a number of countermeasures. See Figure 1. These countermeasures are considered as some of the toughest laws and programs in North America, and have contributed to Ontario’s reputation as a leader in road safety.

Implementation of Ontario’s Countermeasures

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Figure 1. Timeline of drinking and driving countermeasure implementation.

In addition to enforcing standards for driving behaviour as defined by the Criminal Code of Canada (CCC), Ontario has independent authority to regulate driver conduct on provincial roads through the Ontario Highway Traffic Act (HTA). Since 1995, the Ministry of Transportation of Ontario (MTO) has introduced a number of countermeasures aimed...
at reducing drinking and driving behaviour, and their related fatal and injury collision outcomes. These countermeasures target novice-classed drivers, young drivers, and the general driving population who have a blood alcohol concentration (BAC) at various thresholds—0.00, 0.05, and 0.08 (mg/100ml, or simply %). Some of these involve sanctions that are administered upon detection at the time of the occurrence, while others are applied upon or after conviction.

In 2013, the Road Safety Research Office within the Safety Policy and Education Branch of the MTO embarked on a two-year systematic and quantitative evaluation of all current provincial drinking and driving countermeasures. The purpose of this evaluation was to support evidence-based policy by aiding decision makers in refining current programs, developing new initiatives, and guiding public education campaigns. In addition, this evaluation helped ensure accountability to the public, to stakeholders, and to service providers. Through an impact evaluation, we intended to determine whether Ontario's drinking and driving countermeasures are demonstrating a tangible effect on the health and safety of the population and, if so, to subsequently calculate the impact attributable to such countermeasures. Process evaluations were also conducted, as necessary, in order to support the validity of the impact evaluation. Economic evaluations are out of scope. For the remainder of this report, the term “countermeasure” will be used to refer to these deterrence initiatives in a theoretical (or policy) sense. The term “intervention” will be used in lieu of “countermeasure” in a technical context, when discussing research and evaluation.

The criminology literature identifies two distinct deterrent effects that can be produced by a countermeasure. A general deterrent effect is present when a countermeasure is successful at preventing an undesirable behaviour by all members of the target population (i.e., licenced drivers). A specific deterrent effect is present when a countermeasure is successful at preventing an undesirable behaviour amongst members of the population who have already received the corresponding sanction (Bosworth 2005). General deterrence of drinking and driving is particularly important in Ontario because approximately 89% of alcohol-impaired drivers involved in a fatal or injury collision in 2012 had received no criminal convictions for drinking and driving over the previous ten years⁷. Similarly, 87% of such drivers had received no roadside

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⁷ For these calculations, “alcohol-impaired” refers to a driver whose condition was coded in the Accident Data System as “Ability impaired Alcohol (Over .08)” or “Ability Impaired Alcohol”. “Previous convictions” for these calculations were taken to be convictions under CCC s. 253 or 254. The 89% figure for previous convictions and the 87% figure for administrative sanctions were calculated based on drivers of all ages.
administrative sanction for BAC > 0.08% (or refusal to submit to screening) in that time period. In other words, 87% of alcohol impaired drivers had no previous drinking and driving related contact with enforcement before collision involvement, so their drinking and driving could not have been prevented via specific deterrence\(^8\). Given the importance of the general/specific deterrence distinction, we evaluated each countermeasure separately for both of these properties whenever possible.

Immediacy, certainty, and severity are often cited requirements for countermeasures to produce maximum deterrent effects, whether general or specific. Ontario's countermeasures differ from each other not only in their varying capacities to produce general and specific deterrence, but also in their relative degrees of immediacy, certainty and severity. Deterrence theory provides a useful conceptual framework and a shared vocabulary when discussing drinking and driving countermeasures and their effectiveness, so these terms will be referred to frequently in the remainder of this report.

In what follows, we provide a summary of the evaluated countermeasures, explaining their use in Ontario as well as their effectiveness in other jurisdictions (Background); we outline the methodology used to evaluate these interventions (Methods); we present the findings of our evaluation (Results); and, finally, highlight implications and conclusions resulting from this work (Discussion). The Background, Results, and Discussion sections are organized into sub-sections defined by countermeasure, whereas the Methods section is organized into sub-sections by analytical approach. This division was found necessary because analysis of the potential general deterrent effects of closely spaced countermeasures had to be performed as a “batch” whereas analysis of specific deterrence was performed for each countermeasure in relative isolation, with unique methodology appropriate for each.

The Methods and Results sections of this report require familiarity with statistical methodology. However, the Background and Discussion sections, along with results contained in the Executive Summary can be read independently of the Methods and Results sections.

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\(^8\) After May 2009, administrative Warn Range sanctions could have provided an additional point of contact between drinking drivers and enforcement, but not enough time has passed to assess whether alcohol-impaired drivers involved in injury/fatal collisions are likely to have received such a sanction prior to collision.
BACKGROUND

90-day Administrative Driver’s Licence Suspension

OVERVIEW

Administrative Driver’s Licence Suspensions (ADLS) were first introduced in December 1981 under the legal authority of HTA s.48. This policy authorized police to immediately suspend a driver’s licence for 12 hours if the driver is determined to have a BAC at or above 0.05%. On November 29, 1996, this ADLS policy was amended under the legal authority of HTA s.48.3 in three ways: 1) to be extended to 90 days; 2) to apply to drivers with a BAC at or above 0.08% or who refused to provide a breath sample; and 3) to be noted on the driver’s record. This amended ADLS policy is hereafter referred to as the 90-day ADLS and is the subject of our evaluation. A driver may launch an appeal of a 90-day ADLS to the Licence Appeal Tribunal on limited grounds of mistaken identity or for medical reasons.

RELEVANT LITERATURE

There is extensive literature demonstrating the effectiveness of roadside ADLS as a deterrent to drinking and driving. Numerous studies have shown that immediate roadside suspensions for drinking and driving are highly effective deterrents resulting in lower driver re-arrests and collisions. Research suggests that suspensions are more effective when their administration is not dependent upon the discretion of enforcement officers, and that longer suspensions are more effective than shorter ones (Ross 1987; Ross and Gonzales 1988; Chaloupka, Saffer, and Grossman 1993; Williams, Weinberg, and Fields 1991; Watson 1998; Nichols and Ross 1990).

In Ontario, the effectiveness of both the current 90-day ADLS and the earlier 12-hour suspension have both been studied. The 12-hour suspension was shown to produce small and short-term general deterrent effects (Vingilis et al. 1988). Using fatality data to record changes in the proportion of alcohol-related fatalities amongst drivers older than 16 years old between January 1979 and December 1982, Vingilis et al. noted a short-term positive effect immediately in January 1982 following the introduction of the 12-hour suspension followed by a return to previous fatality rates in September 1982. Researchers cited the lack of a well-organized media campaign as well as ineffective
coordination of enforcement efforts for the return to baseline levels. One weakness of the Vingilis et al. study was that only one year of post-intervention data was analyzed, which provided too short a time series upon which to draw strong conclusions.

The 90-day ADLS policy has had a significant deterrent effect both on total driver fatalities and driver fatalities with a BAC level over 0.08% (Mann et al. 2002; Asbridge et al. 2009). As an early positive indicator, Mann et al. (2000) cited evidence suggesting good public awareness of the new countermeasure amongst randomly surveyed drivers and a pre- to post-implementation decrease in the number of drivers who self-reported driving after consuming two or more drinks in the previous hour. Subsequently, Mann et al. (2002) employed an ARIMA-based interrupted time series model to analyze the observed monthly proportion of driver fatalities with a BAC level at or above 0.08% between January 1, 1988 and December 31, 1997, finding that the implementation of 90-day ADLS was associated with a 17% reduction in fatalities. Similar to Vingilis et al. (1988), this study used only short-term (i.e., 13 months) post-intervention data. A longer period would need to be studied in order to produce the strongest conclusions and to determine whether the observed effects were sustained over time.

Providing more recent evidence of the effectiveness of Ontario’s 90-day ADLS, Asbridge et al. (2009) studied its effect on the monthly numbers of driver fatalities in Ontario between January 1, 1988 and December 31, 1998 using fatality rates in Manitoba and New Brunswick as control groups. A significant intervention effect from the introduction of the 90-day ADLS was found in Ontario, with an estimated 14% decrease in driver fatalities in the two years following implementation, while no corresponding effect was observed in the control provinces.

Currently, various forms of the ADLS countermeasures are in effect across Canadian provinces. Studies of their effectiveness have been conducted in Alberta, where a small reduction in the number of alcohol-related collisions and a significant reduction on alcohol-related fatal collisions were observed following the implementation of the ADLS in 1999 (Howard Research and Management Consulting 2005). In Saskatchewan, a time series analysis of monthly driver fatality data between January 1987 and December 2001 showed no significant effect of the 1996 implementation of ADLS (Beirness and Singhal 2007). However, a deterrent effect was identified via reduced recidivism by first-time offenders. This effect did not extend to previous offenders. Given the smaller population of Alberta and Saskatchewan in comparison to Ontario, it seems reasonable to assume that monthly fluctuations in fatality outcomes would be larger in
these provinces, likely reducing statistical power and making detection of ADLS effects difficult.

In the US, the effects of ADLS have been evaluated at the national level with a number of studies capturing changes across contiguous states. For example, Zador et al. (1989) studied drivers 21 or older who were involved in fatal crashes between 1978 and 1985 in 48 contiguous US states. They found that administrative suspension policies had little impact on driver involvement in fatal crashes during hours of low to moderate alcohol involvement (i.e., during time periods when most collisions do not involve alcohol), but were associated with a significant 9% reduction in driver involvement in fatal collisions during hours of high to very high alcohol involvement (i.e., during time periods when most collisions do involve alcohol). Additional studies of ADLS-like interventions in the US have been performed by Ross (1987) and Klein (1989), suggesting effectiveness of these countermeasures in reducing alcohol-related fatalities.

Analyses of countermeasure effectiveness often rely on fatality data, as with the studies described above. This is likely because such data is widely available. However, fatality data tends to demonstrate large variation from month-to-month because of the relatively low number of outcomes that occur within any 30-day period. These low numbers can violate the continuity conditions necessary to perform the types of statistical analyses typically employed for countermeasure evaluation. In our work, we circumvent this difficulty by combining fatalities with major injuries (or all injuries) to generate outcome measures for analyses.

**Warn Range Sanction**

**OVERVIEW**

Warn Range Sanctions were introduced on May 1, 2009 under the legal authority of HTA s.48. This countermeasure targets drivers who are detected driving with a BAC above 0.05%. For a first detection, drivers have their licence suspended for three days. Upon a second detection within a five-year period, drivers will have their licence suspended for seven days and be required to take a one-day remedial alcohol education course (see Remedial Measures section below for details). On a third detection, drivers will be subject to a 30-day licence suspension, remedial alcohol
treatment (a two-day course), and a 6-month ignition interlock condition. In all cases, a $150 licence re-instatement fee must be paid by the driver in order to resume legal driving privileges (see Figure 2). These countermeasures are intended to provide a general deterrent to drinking and driving for the entire population, while the escalating nature is intended specifically to deter a small proportion of the driving population who repeatedly drive under the influence of alcohol.

![Figure 2. Graphical depiction of Warn Range Sanctions.](image)

**RELEVANT LITERATURE**

The introduction of a reduced legal BAC limit of 0.05% is supported by two bodies of literature. First, numerous laboratory studies demonstrate cognitive, sensory, and motor impairments at this blood alcohol level or lower. For example, Ferrara et al. (1994) reviewed 38 studies examining the effects of BACs ranging from 0.029% to 0.15%. These studies all examined driving-relevant cognitive and/or motor functions with the overall conclusion being that psychomotor performance on complex tasks is impaired even at BACs well below 0.05%. Similarly, Chamberlain and Solomon (2002) reviewed evidence suggesting that, at non-zero BAC levels below 0.05%, there is a reduced ability to maintain attention and wakefulness along with deficits to motor, vision, and information processing functions. For example, Liu and Ho (2010) tested eight participants at increasing increments of BAC (0.00%, 0.05%, 0.08%, and 0.10%) on divided attention, distance estimation, and flicker fusion tasks. The experiment involved two scenarios representing different road complexities and four simulated driving sessions at each BAC level. Reaction times and divided attention task response times
increased with increasing BAC; this pattern was found in simulated driving environments that required both low and high cognitive load. Importantly, for both the divided attention and distance estimation tasks, each incremental increase in BAC led to significantly slower reaction times.

Second, the significant positive relationship between collision injury severity and blood alcohol levels also offers a rationale for lowering BAC limits to 0.05% (Phillips and Brewer 2011; Fell and Voas 2006). In a general review on BAC limits, Killoran et al. (2010) suggest that drivers with BACs between 0.02% and 0.05% have at least three times greater risk of dying in a vehicle collision than sober drivers. Although these findings clearly indicate that low but non-zero BACs should reasonably be expected to cause collisions, they do not tell us directly whether lowering the illegal BAC limit for drivers will actually reduce alcohol-related injuries and fatalities. Indeed, it has been pointed out that success of any BAC limit reduction will likely depend on public awareness of new policies (Ferguson 2012).

The bulk of research on the impact of reducing BAC limits on alcohol-related collision rates have assessed the transition from BAC limits of 0.10% or higher to a BAC limit of 0.08% (Fell and Voas 2014; Ferguson 2012; Killoran et al. 2010; Fell and Voas 2006; Chamberlain and Solomon 2002; Mann et al. 2001). Although studies in individual U.S. states have yielded mixed results, researchers have suggested that this may be due to poor advertising of new regulations or differences in policy enforcement. In contrast, studies that combine data across multiple U.S. states show a more reliable pattern of reduced fatalities as a result of lower BAC limits. Hingson et al. (2000; 1994) examined alcohol-related fatal collision rates in 11 states that introduced a BAC limit of 0.08% in the early 1990s by pairing each state with a neighbouring state with a BAC limit at or above 0.10%. Alcohol-related fatality rates were found to be significantly lower after the limit reduction than before in all states that introduced such reductions, with no corresponding change in control states. Likewise, Bernat et al. (2004) found that BAC limit reduction policies were associated with a significant decrease in single vehicle nighttime fatal collisions (an often used proxy for alcohol-related collisions) in states that introduced a BAC limit of 0.08% before 2001. In a more recent study, Tippetts et al (2005) and Wagenaar et al. (2007) evaluated the effectiveness of policies mandating a BAC limit of 0.08% in 28 states using an ARIMA-based interrupted time series approach. The results demonstrate great variability in effectiveness across states, but equally demonstrate that policies mandating a BAC limit of 0.08% reduce alcohol-related fatal collisions and alcohol-related collisions at any blood alcohol level.
Relatively few studies examine transitions to a 0.05% limit, with most such investigations being of lower methodological rigour (Deshapriya & Iwase (1996; 1998); Bartl & Esberger (2010); Homel (1994)).

Turning to BAC limits below 0.08%, Hingson et al.’s (1998) well-controlled study investigated the effects of Maine’s 1988 BAC limit reduction from 0.10% to 0.05%. Using fatal collision data from 1982 to 1994, the authors found a significant reduction in the ratio of alcohol-related fatal collisions to non-alcohol-related fatal collisions in Maine compared to the same data from six other control states in New England. This study’s main strength was the use of numerous control groups, while its weakness was in the pooling of data across large timespans. This pooling might have hidden time trends that existed in Maine, but that might not have existed elsewhere. Similarly, Desapriya et al. (2007) evaluated the effectiveness of a BAC limit transition from 0.05% to 0.03% in Japan in 2002, using non-alcohol related collisions as a control. They found a 30-40% reduction in alcohol-related collisions, with no significant reduction in total collisions. As with Hingson, rates were pooled over long time spans (3 years), and in contrast to Hingson, no control jurisdictions were used.

Interrupted time series approaches to evaluating effectiveness of low BAC limit countermeasures are rare in comparison with how frequently they have been employed to evaluate BAC limits of 0.08%. In one study, Andreucetti et al. (2011) evaluated the effectiveness of Sao Paulo’s 2008 BAC limit reduction from 0.06% to 0.02% on fatal and injury collisions. ARIMA models were fit to the collision rates with covariates representing the intervention and the temporally proximal police strike. Although collision rates significantly decreased at the time of the policy change, no control time series were used and, therefore, interpretation is difficult. Albalate (2008) has performed what is perhaps the most rigorous study of the effectiveness of BAC below 0.05% policies to date. The author investigated fatality rates across several European Union countries that introduced such policies from 1991 to 2003. This analysis involved a linear regression with controls for seasonal effects and confounding effects, including the presence of random roadside breath tests in each country, country-specific unemployment rates, etc. The BAC policies clearly produced reductions in fatalities for the studied countries, but because of a fixed effects design, generalization is difficult.

In addition to studies that investigate the effect of reduced BAC limits on collisions, some examine a more indirect measure: the amount of alcohol consumed before and/or during driving. For example, Berenhoft & Behrensdorff (2003) conducted telephone...
surveys in Denmark, which revealed that people claimed to drink less and/or less often before driving after a 1998 policy reducing the BAC limit from 0.08% to 0.05%. Using a similar methodology, Assum (2010) found similar results when Norway reduced its BAC limit from 0.05% to 0.02% in 2001. However, in these two cases, there appeared to be no corresponding reduction in collisions or fatalities related to drunk driving. Using a more direct approach, Mathijssen (2005) examined random roadside breath test data from the Netherlands, revealing a 27% reduction in the number of drivers with BAC at or above 0.05% after the introduction of a 0.05% limit in 1974. This change, however, occurred in concert with numerous other drinking and driving countermeasures.

In summary, the evidence base supports the expectation that reductions in the legal BAC limit would improve road safety. Quality evidence clearly supports this assertion by demonstrating that reductions in BAC limits from 0.10% to 0.08% are effective. Evidence of lower quality favours further reduction to 0.05% or less.

**Vehicle Impoundment**

**OVERVIEW**

*Long Term Vehicle Impoundment*

The Long Term Vehicle Impoundment was introduced in February 1999 under the legal authority of HTA s.55.1. This program applies to vehicles being operated while the driver is under a driving prohibition as a result of a CCC conviction. Long Term Vehicle Impoundments are administered according to the number of times a vehicle has been impounded, with the first impoundment lasting 45 days followed by 90- and 180-day impoundments for a second and third time occurrence within two years as indicated in Figure 3. In addition to having the vehicle impounded, the vehicle owner or plate holder must pay storage and towing costs, which vary depending on the police service. Upon conviction, the driver must also pay a fine as determined by the court. The vehicle owner or plate holder may appeal the impoundment on the grounds of 1) a stolen vehicle, 2) the driver was not under a criminal prohibition order at the time, 3) due diligence, and 4) exceptional hardship resulting from loss of vehicle.
Seven-day Vehicle Impoundment

Seven-day vehicle Impoundments were introduced in December 2010 under the legal authority of HTA s.55.2 and s.41.4. This countermeasure targets four groups: 1) those with a BAC over 0.08%; 2) those who refuse to provide a breath sample; 3) those who violate their ignition interlock condition; and 4) those who are detected driving during any licence suspension imposed under the HTA (see Figure 4). Drivers within these subgroups receive an impoundment notice from a police officer at roadside, and their vehicle is impounded.

Figure 3. Graphical depiction of the the Long Term Vehicle Impoundment countermeasure.

Figure 4. Graphical depiction of the Seven-Day Impoundment countermeasure.

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*Also receive a 90-day ADLS at roadside (HTA s.48.3.2), along with criminal charges; Various additional charges/penalties apply; These include the 90-day ADLS, the Warn Range suspension, Zero BAC suspension, and failure to complete remedial measures suspension*
RELEVANT LITERATURE

Vehicle impoundment as a countermeasure for alcohol and other driving-related offences has a relatively long history. However, to our knowledge, no peer-reviewed published studies of vehicle impoundment as an isolated countermeasure have been conducted outside the US. Studies in US jurisdictions suggest impoundments in isolation can reduce various forms of recidivism. In conjunction with other driver countermeasures such as licence suspensions, some studies have claimed a positive impact of vehicle impoundment on reducing fatal collision rates.

In Canada, vehicle impoundment countermeasures have been introduced in conjunction with other countermeasures. For example, in British Columbia, the 2010 introduction of immediate roadside prohibitions including vehicle impoundment and licence suspension resulted in a 21% decrease in fatal collisions, 8% fewer collision-related hospital admissions, and 7.2% fewer ambulance calls for road traffic injuries (Brubacher et al. 2014; Beirness and Beasley 2014). In Manitoba, Beirness et al. (1997) found the introduction of vehicle impoundment in conjunction with licence suspension in 1989 resulted in reduced fatal and injury nighttime collisions. However, there is an extensive literature which shows that licence suspensions themselves have deterrent effects (Mann et al. 2002). Thus, the effectiveness of impoundments in isolation could not be ascertained from these studies. However, studies performed on US impoundment programs have provided for a measure of impoundment’s effectiveness in isolation (Voas et al. 2004).

The introduction of a multi-stage vehicle impoundment policy in Ohio in 2013 has been studied by Voas, Tippetts and Taylor (1998). In this program, drivers charged for driving while suspended (DWS) would be subject to vehicle impoundment for 30 and 60 days for a first- and second- time offence respectively. Third offences were subject to vehicle forfeiture. Driving under the influence (DUI) offences were also subject to 90-day impoundment for a second violation and 180-days for a third. However, a large proportion of drivers eligible for impoundment did not actually have their vehicles taken. This provided the study authors with a reasonable control group. The reasons that a driver might not have received impoundment in Ohio vary, and the authors suggest the possibility that they may not have been able to fully correct for these influences in their analysis. Notwithstanding this reasonable caveat, the authors found significantly less recidivism for those receiving impoundment versus those who did not. This was true for both DUI and DWS recidivism, with impoundment-related reductions in recidivism of up to 58% in some cases.
In California, a 30-day vehicle impoundment policy was implemented in 1994 for vehicles being operated while the driver is under suspension. This countermeasure resulted in a reduction of DWS and DUI recidivism, but had no observable effects on overall collision rates of suspended drivers (DeYoung 1999; 2000). Deyoung's (1999) study involved comparing recidivism for those who offended (DWS/DUI) within the year before and the year after the implementation of the impoundment countermeasure. To ensure equivalency of the pre- and post-implementation groups, the author performed statistical matching and adjusting between groups based on demographic characteristics. Impounded first-time offenders showed a 24% reduction in DWS/DUI recidivism, while previous offenders demonstrated 34% fewer re-offences. Similarly, DeYoung (2000) performed ARIMA-based interrupted time series analysis of collision rates for drivers who had been suspended five years before or after implementation. Using non-suspended drivers as a control group, he found no effect of impoundment on the collision rates of suspended relative to control drivers. However, in a much smaller study in Uplands, California, Cooper, Chira-Chavala & Gillen (2000) found that impoundment countermeasures not only reduced driving by disqualified drivers and DUls, but additionally some types of collisions.

In addition to vehicle impoundment, various jurisdictions have also implemented other vehicle-based countermeasures, including vehicle forfeiture and plate impoundment, both with some evidence of reduced recidivism (e.g., vehicle forfeiture in Portland, Oregon (Crosby 1996) and plate impoundment in Minnesota (Preusser 2011). As these programs are different from impoundment in many important respects, we will not review them in detail.

It should be noted that existing research on vehicle impoundment highlights several specific unintended effects of this program. For example, Peck & Voas (2002) describe how vehicles of DWS and DUI offenders are often of very low value, even going so far as to say that experienced offenders might purchase very low value cars so that they can be abandoned upon impoundment. The authors also point out another problem with vehicle countermeasures, being that there is often shared ownership for vehicles driven by impaired drivers. Thus impoundment might create undeserved hardship for non-offending community members. Ontario’s Long Term Vehicle Impoundment program does make allowance for these factors, as described above. A more intractable difficulty with impoundment/forfeiture programs was pointed out by Lee et al. (2009), who found that such sanctions might induce drivers to flee the police to avoid losing personal property of high value, thereby endangering members of the community,
themselves, and police officers. Presumably, this would be of greater concern with forfeiture programs.

**Zero BAC Requirements**

**OVERVIEW**

Zero BAC Requirements for drivers were introduced on August 1, 2010 under the legal authority of HTA s.44.1 and 48.2.1. These target novice-classed drivers in the graduated licencing system (GLS) and young drivers under 22 years old who are found driving with any detectable BAC. These drivers receive an immediate 24-hour licence suspension and, if convicted, receive an additional 30-day suspension with a fine ranging between $60 and $500 (see Figure 5). Novice-classed drivers are subject to an escalating component, whereby a 90-day suspension results upon conviction for a second offence within five years. For a third conviction, the novice-classed driver has their licence cancelled and must begin the licencing process anew.

![Diagram of Zero BAC countermeasure](image)

*Figure 5. Graphical depiction of Zero BAC countermeasure.*
RELEVANT LITERATURE

There seems to be general consensus in the literature that policies prohibiting alcohol consumption in young and/or novice-classed drivers in the US are effective at decreasing collision rates. A recent study of all 50 states provides a seeming counterexample in that it revealed no attributable effects of zero tolerance (ZT) legislation on alcohol-positive drivers in fatal collisions (Fell et al. 2007). However, this study aggregated outcomes across drivers irrespective of age, therefore potentially masking an effect in the under 21 population toward whom ZT policies are targeted. The same data source was used previously in another nation-wide study, which examined outcomes specific to the under 21 group. This study found a 24% reduction, as a result of ZT policies, in the number of drinking drivers under 21 who were involved in fatal collisions (Voas et al. 2003).

When examining collision outcomes in young drivers only, numerous studies show the benefit of such laws. For example, in one well-controlled study, 12 pairs of matched states, where one state in that pair had enacted a lower legal BAC policy for young drivers and one had not, were examined (Hingson, Heeren, and Winter 1994). The number of single vehicle nighttime fatal collisions as a proportion of total number of fatal collisions in the under 21 age group was compared in the pre-policy versus the post-policy period for each pair of matched states. Results show a 16% decrease in the proportion of single vehicle nighttime fatal collisions in the post-policy years compared to the pre-policy years among the under 21 group in the intervention states, while the proportion among this age group in the 12 matched comparison states increased by 1%.

This pattern was also found in studies that examined single states or a select handful of states. Lacey and colleagues (2000) examined the impact on ZT policies on single vehicle nighttime injury collisions in Florida, Maine, Oregon, and Texas. This statistic was compared to drivers under age 21 who were involved in multi-vehicle daytime injury collisions. In two of these states (Maine and Oregon), ZT policies were accountable for a reduction in the alcohol-related collisions by approximately 30-40%. Bloomberg (1992) evaluated the impact of Maryland’s ZT policy over a period of 48 months pre-policy and 24 months post-policy. Results indicated a significant decrease in “had been drinking” drivers under 21 in collisions after controlling for secular trends. Similarly, a Michigan-based study showed a statistically significant decrease in “had been drinking” drivers under 21 in the post-policy period compared to the pre-policy period with no
corresponding decrease in equivalent drivers over 21, suggesting that the ZT policy had a unique effect on the under 21 target population (Streff and Hopp 1997).

Converging results emerged in a California study; in this study, the number of drivers aged 16-20 and 21-35 with a positive BAC in fatal collisions was compared to equivalent populations in Colorado, Wyoming, Texas and Nevada over an eight-year time frame (Martin and Andreasson 1996). In California, during the first 18 months of the ZT policy, the proportion of drivers age 16-20 with a positive BAC in fatal collisions declined by 21% compared to the previous six years (relative to a 1% post policy decline amongst California drivers aged 21-35). This large difference in fatal collision rate decline between the youngest (targeted) driver group and older (untargeted) driver group was not seen in the four control states. For these control regions, a 12% decline among 16-20 year olds during the first 18 months after California’s ZT policy and a similar 9% decline among adults aged 21-35 was observed (Voas, Lange, and Tippetts 1998). However, there is some argument about whether the decrease in alcohol-related collisions amongst the under 21 population in California are attributable to ZT legislation, as equivalent reductions were observed in other groups in various circumstances (Grant 2010).

**Alcohol Education and Treatment Remedial Measures Program**

**OVERVIEW**

The Alcohol Education and Treatment Remedial Measures Program was introduced in September 1998. It is a mandatory component of the licence reinstatement process, under HTA s.41.1, for all drivers convicted of alcohol-related criminal driving offences under CCC s.253 or 254 and, starting in May 2009, for drivers in receipt of repeat Warn Range Sanctions. This program, called Back on Track (BOT) is currently administered by the Centre for Addictions and Mental Health (CAMH).

When first implemented, the program included an education component only. The education component is a one-day (8-hour) course where participants learn about myths and factors about alcohol and other drugs; how alcohol and other drugs affect driving performance and safety; the legal and personal consequences of an impaired driving conviction; and ways to avoid drinking and driving.
In October 2000, the program was expanded to include assessment and treatment components, and a follow up interview. Within 30 days of applying to the program, drivers undergo an assessment which streams them into either the education component or the treatment component, which they undergo within 60 days of the assessment. The treatment component is a two-day (16-hour) course where participants learn about why people drink or use other drugs and how it affects a person’s life; learn about their alcohol and drug use and its consequences; commit to reducing problem use; learn strategies for managing stress, communication, anger, etc.; plan to avoid relapses; and avoid drinking and driving. Both components include presentations, exercises, discussions and group work. The follow up interview (30-minutes) occurs via telephone six months after the completion of the education/treatment component. This program must be completed before the licence suspension expires in order to have the licence reinstated. Otherwise, drivers receive an indefinite “Failure to Complete Remedial Measures” suspension under the HTA.

![Graphical depiction of the remedial measures program.](image)

**RELEVANT LITERATURE**

In Ontario, the BOT program has been assessed to determine the reliability of individual assessment measures (e.g. the ACSAS and the RIASI) (Mann et al. 2006) and to study specific clinical or non-driving related outcomes for participants, such as mortality rates (Mann, Vingilis, and Stewart 1988; Mann et al. 1994). In 2012, Flam-Zalcman et al. compared the outcomes of participants enrolled in the shorter education program versus the longer treatment program. Using a regression-discontinuity design, Flam-Zalcman et al. found that the longer treatment program resulted in a significant reduction in self-reported alcohol consumption in the 90-days following completion of the program compared to the shorter treatment. Participants were assigned to either the shorter or longer treatment based on their performance on two addiction severity scales.
Stoduto et al. (2014) analyzed the self-report questionnaires of a large sample of 22,277 BOT participants between 2000 and 2005 before and after they completed the program. They also observed a significant reduction in participants’ reports on subsequent number of days of alcohol use and number of drinks per occasion. Participants reported reductions in alcohol and drug use as well as reductions in the negative social impacts associated with drinking (e.g. physical health, cognitive abilities, mood, relationships, and aggressive behaviour).

Rootman et al. (2005) have assessed the factors associated with completion of the BOT. In a sample of program participants between 2000 and 2002, Rootman et al. observed a program completion rate of 97%. They identified the demographic characteristics of the non-completers as being predominantly younger, less likely to own a home, more likely to be living in an urban centre, more likely to have two or more lifetime impaired drinking convictions, more likely to report frequent drinking, and more likely to have experienced more than one adverse consequence related to substance abuse. To date, there have been no studies of BOT which assess the relationship between participation and recidivism. The wider literature on remedial driver programs across North America expands on this clinical outcomes focus to trace the evolution of treatment approaches which has taken place since the late 1960s and address key questions relating to the impact of remedial programs on driver recidivism rates.

Early remedial programs focused on teaching drivers how alcohol impairs driving. Evaluations of these programs showed mixed results on measures of alcohol-related highway collision reduction and drinking and driving recidivism (Malfetti and Winter 1980; Swenson and Clay 1977; Whitehead 1975; Holden 1983; Elisabeth Wells-Parker et al. 1988; Watson 1998; Swenson et al. 1981). A comprehensive meta-analysis of 215 studies on the effectiveness of remedial measures programs by Wells-Parker et al. (1995) found an 8-9% average effect of remediation on drinking and driving recidivism compared to no remediation. Using regression analysis, the authors further suggested that the most effective programs combined education, psychotherapy, and follow-up contact with participants. Researchers have also observed that the application of concurrent licence-based sanctions alongside remedial treatment produces the most positive results for participants. These new formats have been shown to increase driver awareness about the risks of drinking and driving and some studies show an impact on alcohol consumption, recidivism, collisions and fatalities (Foon 1988; E. Wells-Parker et al. 1995; Mann, Vingilis, and Stewart 1988; Nochajski and Stasiewicz 2002; Brown et al. 2010; DeYoung 1997).
The introduction of cognitive behavioural therapy in a Florida-based program called TRIAD has been shown to be effective in reducing recidivism. However, non-completion of the entire program was a key risk factor. Researchers found that the very small (3%) proportion of participants who did not complete the program following an initial assessment were responsible for 75% of the re-offences (Moore et al. 2008). Another factor strongly associated with offender recidivism is offender depressive mood. Wells-Parker et al. (2009) identified depression as a key risk factor for recidivism regardless of whether the participant received a more individualized program with follow-up counselling versus a shorter, generic regimen. This finding is particularly relevant due to the higher rates of depression observed within the DWI population as a whole (Gamble et al. 2013).

The Mississippi Alcohol Safety Education Program is exemplar of a remedial measures program whereby results from evaluation studies are used to inform program design iterations. The curriculum and structure of the program has evolved over four versions to adapt to changing demographics as well as to maximize effectiveness of the program on reducing recidivism amongst DUI convicts. In a recent evaluation, DUI recidivism rates were examined between two groups of participants- timely completers (i.e., completed within 3 months) and noncompleters- and equivalent offenders who did not enroll in the course (Robertson et al. 2013). Participants received one of two versions of the program. Cox regression models were built to model recidivism rates by program completion status and program version. Compared to those who did not complete or did not enroll in the program, offenders who completed the program had significantly lower DUI recidivism at 12 months and at 36 months. Recidivism rates were not significantly different between participants subject to the 2008 versus the 2000 version of the program. Compared to the non-enrollment group, both timely completers and noncompleters had longer survival time until recidivism. Although suggestive of an intervention effect, the study is hampered by a few limitations, one of which is that the outcome measure is defined differently between groups which would make the survival time calculation not directly comparable.
Ignition Interlock

OVERVIEW

Ignition Interlock Program

The Ignition Interlock program was introduced in December 2001 under the authority of HTA s.41.2. An ignition interlock is an in-car alcohol breath screening device that prevents a vehicle from starting if the driver registers a BAC in excess of a pre-set limit (i.e., BAC = 0.02%). The device is installed inside the vehicle, near the driver's seat, and is connected to the engine's starter mechanism.

To be eligible for the original Ignition Interlock Program (before introduction of the Reduced Suspension with Ignition Interlock Conduct Review Program on August 3, 2010), drivers were required to serve the full duration of their criminal driving prohibition and complete the Alcohol Education and Treatment Remedial Measures Program. Once these conditions were met, drivers would have an interlock condition placed on their licence that required installation of an interlock device in any vehicle they operated. Drivers who chose not to install an interlock were not permitted to drive for the duration of their interlock licence condition. The length of the interlock condition depended on the number of prior impaired driving convictions, with first-time offenders receiving a one-year condition, second-time offenders receiving a three-year condition, and third-time offenders receiving a lifetime condition.

Drivers in the original program were required to obtain the interlock from and register with the interlock service provider and have the interlock installed by the service provider in their vehicle. The device is inspected regularly by the service provider every 30 to 60 days and, upon inspection, data from the interlock device on the number and type of fails (distinguished based on the BAC found and whether the vehicle has already been started) is downloaded to the service provider's database. Program violations, such as tampering with the interlock device, driving a vehicle not equipped with an interlock, or missing an appointment with the interlock service provider, increased the amount of time the condition remained on the licence and could subject the driver to either criminal or provincial charges. Regardless of whether an interlock device was installed, a driver was required, upon expiration of the interlock condition, to apply to the MTO to have the condition removed from their licence. Otherwise, the condition would remain indefinitely.
In May 2009, the Ignition Interlock program was expanded to include drivers whose licences were suspended for registering a BAC above 0.05% three or more times in a five-year period. These drivers receive a six-month interlock condition on their licence, after which point, the condition will be automatically removed from their licence.

Reduced Suspension with Ignition Interlock Conduct Review Program

The Reduced Suspension with Ignition Interlock Conduct Review Program (“Conduct Review Program”) was introduced on August 3, 2010 under the legal authority of HTA s.41, ss.41.4.1; and HTA Part IV- O. Reg. 287/08. It targets drivers with a first-time alcohol-related criminal driving conviction. This countermeasure modifies the pre-existing program— the Ignition Interlock Program— by reducing the length of the licence suspension period—in conjunction with a reduced period of federal driving prohibition—upon the mandatory installation of an ignition interlock device (see Figure 8). While an Ontario driver convicted under CCC s. 253 or 254 for the first time would typically receive a 12-month driving prohibition, followed by a provincially mandated 12-month ignition interlock condition, the Conduct Review Program (in conjunction with the CCC) allows for a reduced driving prohibition period for eligible first-time offenders. In particular, one variant (“Stream A”) allows for a three-month driving prohibition followed by a nine-month ignition interlock condition, while a second variant (“Stream B”) allows for a six-month prohibition followed by a 12-month ignition interlock condition.

Participation in the Conduct Review program requires that the convicted driver be eligible, and that the driver meets certain milestones by certain deadlines. To be eligible for Stream A the driver must plead guilty to their alcohol-related criminal driving charge, and the driver must be convicted and sentenced within 90-days of the offence. If sentencing is not complete within 90 days but all other eligibility criteria are met, the driver is eligible for Stream B. An eligible convicted driver is invited to participate in the Conduct Review program via mailed letter and must subsequently act according to
program conditions. In particular, the driver must complete the assessment component of the Alcohol Education and Treatment Remedial Measures Program, provide proof of interlock lease agreement before the end of their driving prohibition, and install the device within 30-days of the placement of the interlock condition.

Should a driver be ineligible for the Conduct Review program, that driver will be subject to the original pre-existing interlock program, referred to here as “Stream C”. Similarly, if an offender fails to meet the obligations of the Conduct Review program or commits a program violation, he or she is re-suspended under HTA s.57, are sent to Stream C, and might be subjected to further criminal charges. Drivers in Stream C (the pre-existing program) are expected to install an interlock if they wish to drive. Otherwise, they may simply “sit out” in the condition by not installing the device and not driving during the 12-month condition.

Figure 8. Graphical depiction of the Reduced Suspension with Ignition Interlock Conduct Review program.

*aBased on “minimum absolute prohibition period” (CCC s.259.1.2). Relating to suspension, driver must complete assessment component of the remedial measures program, sign interlock lease agreement, pay outstanding fees before suspension end, and install device within 30 days of suspension end. bBased on time remaining between “mandatory order of prohibition” and “minimum absolute prohibition period” as long as there is a 9- and 12- month minimum for Stream A and B respectively. Relating to interlock condition, driver must return to interlock service provider every 30-60 days to download data and complete remedial measures program during interlock condition.
RELEVANT LITERATURE

Although interlock-based countermeasures yet to be systematically evaluated for impact in Ontario, evidence from other jurisdictions has found evidence of effectiveness during device installation. Below, we first describe the findings of three comprehensive review studies (two systematic) and two more recent studies from Florida on interlock effectiveness in terms of preventing drinking and driving recidivism. Next we describe results from two Canadian studies. Turing to process considerations, we then discuss studies relating to the effectiveness of mandatory installation programs. We finish by considering Sweden’s well-studied program and the relationship between its process implementation and effectiveness.

Coben and Larkin (1999) reviewed six studies of interlock programs to determine their effectiveness in preventing recidivism, one of which was a randomized control trial while the others were quasi-experimental studies. Five studies showed the interlock to be effective in reducing recidivism while the sixth study showed no significant difference in recidivism between the intervention and control groups. Furthermore, in all studies, the interlock was one part of a larger program that included other judicial countermeasures. The effect of these other elements was not explicitly accounted for in any study. The authors concluded that interlocks were an effective deterrent to recidivism during interlock installation. However, only one study reported on recidivism after the interlock was removed. All other work studied re-arrest rates during the period the interlock was installed, so no conclusions regarding long-term effectiveness were drawn.

Similarly, Beirness and Marques (2004) reviewed 11 studies of interlock programs in the US, Canada, and Sweden. All studies had similar findings, with a decrease in recidivism during the interlock period but little residual effect after the device was removed. The authors concluded that increased re-arrest rates following removal of the interlock did not diminish the effects of the interlock during the period it was installed, arguing that the interlock should not be employed as a treatment for alcohol abuse but rather as a barrier to prevent drinking and driving. Therefore, long term changes in behaviour or alcohol consumption should not be expected from use of the interlock alone. All studies reviewed had low participation rates (less than 20%) and judicial or self-selection bias; only one was a randomized controlled trial. The paper also discussed potential ways to extend the benefits of the interlock after its removal, such as by introducing a counselling element or by using predictive profiling of drivers that could be used as a criterion for licence reinstatement (e.g., Marques et al. 2003). In other words, drivers
that continued to exhibit high-risk behaviour while using the interlock would be required to keep it installed for a longer period of time.

Willis and colleagues (2004) conducted a systematic review of 14 studies (1 randomized controlled trial based on Maryland’s interlock program, and 13 other controlled trials) on a population of drivers with drinking and driving convictions in the US, Canada, Australia, and Sweden. In Maryland’s randomized control trial, recidivism rate was the outcome measure of interest, and was calculated for three risk periods: while the interlock device is installed, after it has been removed, and the combined time period. In an assessment of external validity, the review authors cautioned that the randomized controlled trial was limited to offenders who had demonstrated compliance with prescribed treatments and were approved for relicencing by Maryland’s Medical Advisory Board. In terms of internal validity, 3 controlled trials were administered through the courts, thus implicating potential judicial bias for group selection; only one study had mandatory participation, resulting in self-selection bias for the remaining studies; and only some studies had control groups with the same “on the road” exposure as the intervention group. The remaining studies had control groups that were still suspended, with drivers who may be limiting their mileage and thus limiting their likelihood for committing a drinking and driving event. Results demonstrated that recidivism was lower in the interlocked group while the device was installed in the vehicle, but the effect disappeared in the time period after interlock removal. Due to these methodological limitations and the fact that, to date, only one randomized controlled trial has been conducted, the authors questioned whether the results of any study could be extrapolated to the general population. Similar to Beirness and Marques (2004), the authors also mentioned the potentially beneficial effects of coupling treatment with an ignition interlock program.

Before turning to specific Canadian studies, it is notable that two studies published more recently than the above review papers looked at recidivism along individual offender trajectories. In Florida, participation in an interlock program is a prerequisite to licence reinstatement following suspension. An evaluation was conducted via survival analysis to compare 2-year recidivism rates amongst first time and repeat DWI offenders in the different steps of the program: suspended, completed suspension, on interlock, and post interlock (Voas et al. 2010). Results indicate that the highest recidivism rate (6.8%) occurred among the DWI offenders who were on suspension, followed by those who had completed the suspension period (3.1%) and those on interlocks (1.2%). The recidivism rate increased (to 5.2%) in the post interlock group, adding converging
evidence that interlocks are effective predominantly when installed. A follow up evaluation was conducted a few years later by the same author (Voas, Tippetts, and Grosz, 2013). In this updated study, data covered a 10-year period and analyses were restricted to 3 rather than 4 periods: during the period of suspension, during the period on II, and during the period after removal of the interlock. Like the previous study, 2-year recidivism rates for first and multiple offenders in various steps of the program were compared to each other via a survival analysis. Recidivism rates were higher in both the one-year (3.6%) and two-year (6.8%) period after device removal than in the period during installation (0.55% at six-months and 1.2% at 12-months).

Turning to specifically to Canadian programs, one of the earlier studies was conducted on an Alberta population of first-time and repeat DUI offenders between 1996 and 1998 (Voas et al. 1999). Two control groups were used to compare recidivism rates at multiple time points. The first control group comprised a group of equivalent drivers who chose not to enter the interlock program and the second comprised a group of offenders not eligible for the interlock program. Efforts were made to minimize selection bias by restricting conviction dates of comparison groups to approximate the distribution of that of the intervention group and by ensuring at-risk periods for recidivism were equivalent., although it seems reasonable to assume that this procedure still left many pre-existing between group differences. Survival analysis were conducted for the period of suspension between index conviction and time when offenders could become eligible to enter the interlock program, the period during the interlock program when participants are driving with the device and non-participants are suspended, and the two-year period after device removal where participant’s licences have been fully reinstated and non-participants may or may not have been relicenced. Overall, results indicated that the participation in the interlock program was associated with reduced recidivism for the intervention group compared to both control groups, but only while the device was installed.

A more recent Canadian study of the 2008 interlock program in Nova Scotia relied on conviction and collision records, self-administered questionnaires, monthly charge-, conviction-, and collision- counts, and interlock logged events for analyses (Vanlaar, Mainegra Hing, and Robertson 2015). Two intervention groups were compared to two control groups. One intervention group comprised of voluntary interlock participants while the other comprised of mandatory interlock participants. The control groups either consisted of eligible offenders or of offenders without an opportunity to participate in the program given their offence occurred in a prior time period. Results suggest that the
intervention group were less likely to recidivate, even after device removal, although they exhibited no significant differences from the control group in terms of collision outcomes. This effect on post-interlock recidivism is inconsistent with the findings of most studies of other interlock programs; the authors speculate that this effect may be due to the elaborate treatment and rehabilitation program to which the interlock group is also subject (Vanlaar et al. 2015; Vanlaar, personal communication). Time series analyses were also conducted to determine the effect of this program on overall population trends; the number of alcohol-related charges, convictions, and crashes did not permanently change after program implementation at the 5% level of statistical significance, indicating little improvement in terms of the overall population impact and a lack of general deterrent effect.

Many jurisdictions have begun to make interlock installation a mandatory component of licence reinstatement. This raises two pertinent questions: 1) whether mandatory installers respond differently to interlock program than those who do so voluntarily, and 2) whether mandatory programs are necessary to increase interlock uptake. The results of our evaluation of Ontario’s Reduced Suspension with Ignition Interlock Conduct Review program suggests that the answer to question 2) is that mandatory programs are not necessary (see Results). To answer question 1) we turn to the literature.

Beirness et al. (2003) built upon the Alberta study (Voas et al. 1999) by examining whether the effectiveness of Alberta’s interlock program differs as a result of participation being mandatory or voluntary. To this end, DWI offenders in Alberta’s interlock program between 1990 and 1996 were followed up during interlock installation and post-removal. Drivers with voluntary participation in the interlock program were compared to drivers with mandatory participation. Survival analysis via Cox regression was used to determine time to subsequent DWI offence. While covariates such as the number of prior DWI and the number of interlock warns at five months were significant in predicting subsequent recidivism, once these covariates were accounted for, the mandatory versus voluntary nature of participation did not demonstrate any statistically significant differences on recidivism.

In the case of Nova Scotia’s interlock program described above, mandatory and voluntary install groups were not significantly different from each other in terms of risk for alcohol-related recidivism (Vanlaar, Mainegra Hing, and Robertson 2015). This echoes the finding by Beirness et al. (2003) that the nature of interlock program participation was not associated with risk of recidivism.
Internationally, Sweden’s interlock program has been evaluated numerous times. In one study, recidivism rates in the two-years of interlock use was compared for the intervention group to two control groups (Bjerre 2003). In the intervention group, there was a reduction of DWI prevalence from 4.7% a year to zero. This was significantly different from the control group of matched individuals from external counties without interlock programs, where DWI prevalence increased from 2.8% a year to 2.9 over the same time period. In a follow-up study (Bjerre 2005), the author found a reduced proportion of DWI offences per year during the program among all 3 intervention subgroups (i.e., those still in the program, those who completed the program, and those that were dismissed from the program) and an increased proportion of DWI offences during the program in both control groups. Data on the period after the program demonstrated a long lasting (>2.5 years) reduction in DWI recidivism for program completers after program completion (and interlock removal).

Sweden’s program appears highly effective, but there are important caveats. The program does not only involve interlock installation, but also has strict medical and other requirements. Only a small proportion (11%) of eligible DWI offenders began the program, while a higher number of participants were dismissed from the program (222) than the number who completed (171). Those who were dismissed did not show the long term benefit that the completers did. Because of the way Sweden’s program was implemented, and because of the results described, its effectiveness could be interpreted as suggesting it is a challenging but effective program. Alternatively, the program might simply be serving as a filtration mechanism, only allowing through the offenders who would not have recidivated anyway. Either interpretation would be consistent with the idea that the interlock device is an effective barrier to drinking and driving while installed, but that it does not induce long-term behavioural change in isolation.

In summary, although we are only aware of one randomized control trial (Maryland), the results of numerous studies converge to the conclusion that the interlock device is effective at reducing recidivism, but likely only while installed. Additionally, in studies where interlock participation was voluntary, a very low percentage of drivers who were eligible to install an interlock chose to do so. Consequently, the interlock programs had very little impact on the overall driving population. In most ignition interlock programs, participation was voluntary or at the discretion of a judge. This resulted in selection bias, where drivers’ personal characteristics affected whether they installed the device. Furthermore, in many studies, the interlock was one part of a larger program that
included other judicial countermeasures and treatment. The effect of these other elements was not explicitly accounted for in any study. Although there may have been overall lower recidivism rates in society as a result of the interlock policy (resulting from a rotating roster of individuals with the device temporarily installed), the lower recidivism rates for individual drivers may not have necessarily sustained a decrease over time, especially after device removal. Lastly, there has been some evidence that interlock participants had a higher risk of being involved in a crash possibly due to the fact that drivers with an interlock drove more frequently and greater distances than drivers with a suspended licence (Elder et al., 2011).
METHODS

Overview of outcome measures
For each of the eight countermeasures, specific research questions were defined based on the intended and/or expected impact of that countermeasure. The resulting questions can be divided into two broad categories: those investigating general deterrence (i.e., effects of a countermeasure’s existence on the behavior of all drivers) and those investigating specific deterrence (i.e., effects of a countermeasure on the behavior of those drivers who have been subject to it). Questions of general deterrence were typically addressed using collision outcomes, such as the number of drivers involved in alcohol-related collisions or the number of injuries and fatalities resulting from such collisions. These collision outcomes are fundamentally what drinking and driving countermeasures are intended to prevent, and there is enough data to address them when looking at the full driving population. Questions of specific deterrence were typically addressed using behavioural outcomes, such as drinking and driving re-offences and driving while suspended offences.

Research questions were addressed using a mix of study designs and analytic techniques, each tailored for a specific research question and to the design features of the countermeasure. In examining general deterrence with large collision-based datasets, ARIMA-based interrupted time series models were typically employed. Interrupted time series approaches based on segmented ordinary least squares and/or Poisson regression were used when looking at specific deterrence in a population of previous offenders in which individuals were not specifically tracked. Assessment of specific deterrence capacity involving tracked drivers relied primarily on Cox and/or logistic regression. Threats to internal validity were treated through restriction if necessary, matching when possible, and adjustment (either with or without matching).

Overview of data
Data were derived primarily from MTO’s Licencing Control System (LCS) and Accident Data System (ADS). The LCS is a database containing lifetime driver records for all drivers with an Ontario drivers’ licence. From the LCS, data were extracted for all drivers with an alcohol-related suspension or conviction date from January 1994 to
November 2014, including demographic information, conviction and suspension history from 1988, ignition interlock condition information, and remedial measures program completion information. The ADS is a standardized collision database compiled from police reports and subsequently verified via coroner’s reports where necessary. From the ADS data were extracted for all collisions from 1988 to the end of 2012, including collision date, driver age, driver licence status, driver condition (including alcohol-involvement) and injury level of all involved individuals (includes drivers, passengers, pedestrians, cyclists, etc.).

Injuries recorded in the ADS can range in severity from those treatable at the scene of a collision to those requiring hospital admissions, with the latter categorized as a “major injuries”. Fatalities recorded in the ADS refer to deaths resulting from collision-related injuries that occur within 30-days of the collision. The likelihood of detecting alcohol in drivers involved in a collision, and thereby having it recorded in the ADS, is affected by numerous factors, including the fact that injury treatment for all involved parties must take priority over alcohol testing. In principle, the recorded number of drinking and driving collisions might therefore contain some level of systematic bias. However, for our purposes, it is only important that the probability of alcohol detection at collision does not change abruptly or substantially across implementation of any countermeasure.

External data were obtained from the Ministry of Attorney General of Ontario (MAG), Statistics Canada, Liquor Control Board of Ontario (LCBO), Alcolock, and LifeSafer of Canada. MAG provided data on traffic-related charges and their related court outcomes. Statistics Canada provided demographic information from their voluntary, population-based National Household Survey (“Statistics Canada’s National Household Survey” 2011). The LCBO provided data on alcohol sales volume. Alcolock and LifeSafer of Canada provided data on ignition interlock registrations, installs, de-installs, and performance. All data analysis was performed using IBM SPSS v21.

**Part I. General Deterrence Analyses**

General deterrence analyses were performed in two separate batches, based on the temporal groupings of countermeasures. The first batch consisted of the 90-day ADLS, Alcohol Education and Treatment Remedial Measures Program, and the Ignition Interlock Program, introduced between 1996 and 2001. The second batch consisted of the Warn Range Sanctions, Zero BAC Requirements, and Seven-day Impoundment, introduced between 2009 and 2010.
Collision outcomes (e.g. number of drinking drivers involved in collisions) were calculated for short-duration time bins that collectively covered a time period starting well before and ending well after countermeasure implementation dates. An unexpected change in the outcome series occurring at the time of a countermeasure’s implementation date, as assessed using ARIMA-based interrupted time series analysis, was taken as an indication of an effect. The interrupted time series approach involves first finding a mathematical model that can reproduce (“fit”) the observed data. Typically two versions of a model are considered, one that is built on the assumption that nothing happens at countermeasure implementation (null hypothesis), and another that contains an intervention covariate, which represents an unexpected change at the time of implementation (alternative hypothesis). If the model containing this intervention covariate fits the data substantially better than the one without (i.e., if the covariate achieves statistical significance), then the intervention (i.e. countermeasure) is said to be associated with an effect. If other potential causes of the effect occurring at the time of countermeasure implementation can be ruled out, the the countermeasure is likely effective.

Outcome measures and confounding

Two outcome series were investigated: outcome 1 was the number of drinking drivers involved in collisions while outcome 2 was the number of injuries and fatalities resulting from collisions in which at least one driver had been drinking. Outcome 1 serves as a proxy for driver behaviour. Outcome 2 is a measure of the immediate harm associated with drinking and driving. The two measures are related but not redundant. This is because fatalities and injuries associated with drinking and driving depend not only on the number of drinking drivers, but also on other factors, including how willing passengers are to get in a vehicle with a drunk driver. A third outcome measure was also used for batch one analysis. Outcome 3 is a modification of outcome 2, where injuries were restricted to those requiring hospital admission (i.e., major injuries).

The pattern of alcohol-related collisions, along with the corresponding fatalities and injuries, can be affected by factors other than countermeasures. Economic conditions, weather, availability of public transit, and other variables can affect the overall amount of driving that occurs, and thereby the overall collision rate. Other factors, including changes in vehicle safety standards and roadway infrastructure, can affect the overall rate of fatalities and injuries resulting from each collision. In order to control for these confounding factors, we further refined our outcome measures so that outcome 1 becomes the ratio of the number of drinking drivers in collisions to the number of non-drinking drivers in collisions, while outcome 2 becomes the ratio of the number of
people injured or killed in collisions involving at least one drinking driver ("D&D collisions") to the number of people injured or killed in collisions not involving any drinking drivers, and likewise for outcome 3. Through use of a ratio measure, changes in the alcohol-related outcomes of interest will only become apparent if they are different than changes in the equivalent non-alcohol measures.

A limited number of extraneous factors could also affect the alcohol-related numerator of our outcome measures independent of the denominator. The most obvious is changes in overall alcohol consumption. To account for this, we obtained data on alcohol sales volume, which, coupled with population estimates from Statistics Canada, allowed us to estimate a covariate time series of alcohol sales per capita in Ontario. This time series served as a proxy to detect changes in alcohol consumption, and was explicitly included in our models to account for such changes.

**Modeling**

The general model used for all of our collision-based analysis was an ARIMA-based transfer function model of the form,

\[
(1 - B)^d (1 - B^s)^sd r_t = \mu + \sum_i T_i(B) x_{it} + \frac{\theta_q(B)\theta_{sd}(B)}{\Phi_p(B)\Phi_{sp}(B)} \epsilon_t,
\]

where \( r_t \) is the outcome series, \( t \) is time bin, \( B \) is the temporal back-step operator, \( \epsilon_t \) is a normal random variable, \( d \) and \( sd \) are the degrees of differencing and seasonal differencing, and \( s \) is the seasonal period. The functions \( \theta_q(B), \phi_p(B), \theta_{sd}(B), \) and \( \Phi_{sp}(B) \) are polynomials of the back-step operator of orders \( q, p, sq, \) and \( sp, \) respectively. These polynomials are the moving average (MA), auto-regressive (AR), and seasonal equivalent components of the model responsible for producing the proper autocorrelation structure. Finally, \( x_{it} \) are covariate time series that can be used to account for external influences on \( r_t \), such as the intervention or confounding variables. The covariate series are brought into the model via the transfer function, \( T_i(B) \), which can also contain AR, MA, seasonal AR, seasonal MA, and lag components. Without covariates the model in Equation 1 is referred to as ARIMA(p,d,q)(sp,sd,sq).

Batch one countermeasures were implemented within five years of one another, but were separated from each other by two to three years. Moreover, they were surrounded a relatively long “clean” period during which no other countermeasures were introduced.
(1988 to 2009). This makes fitting Equation 1 to the outcome series fairly straightforward. However, batch two countermeasures were implemented closer together in time (all within two years) and were followed by less “clean” time (the years 2011-2014) than batch one countermeasures. Therefore, fitting Equation 1 to collision data requires more care for batch two countermeasures. These differences between batch one and batch two countermeasures warranted the use of slightly different methodological approaches, especially when quantifying the magnitude of intervention effects associated with each countermeasure.

**Batch One Analyses (for countermeasures introduced between 1996-2001)**

*Model fitting and testing*

All three outcome series covered the period between January 1, 1988 and December 31, 2010 and was comprised of data binned monthly. Aside from the countermeasures of interest, relatively few changes to drinking and driving policy occurred during this ~22 year interval. For outcome 1, property damage only (PDO) collisions were excluded from the outcome because reporting requirements have changed over time and reporting procedures have changed with the gradual introduction of collision reporting centers.

Interrupted time series models included three intervention covariates, representing each countermeasure implementation. Three intervention effect types were modelled for each countermeasure: a sudden permanent effect, a sudden temporary effect, and a gradual effect. The sudden permanent effect was modeled as a Heaviside step function, transitioning from zero to one at the time of intervention; the sudden temporary effect was modelled as a rectangular step function transitioning from zero to one at the time of intervention and from one to zero either two or four years later; and the gradual effect was modeled as a linear ramp originating at intervention. Outcome series were log-transformed before ARIMA modelling to stabilize variance, which is often necessary to meet the ARIMA stationarity requirements.

To begin analysis, ARIMA(p,d,q)(sp,sd,sq) models were first fit to the period before implementation of the 90-day ADLS for each log-transformed outcome series, producing three “pre-intervention” models. These pre-intervention fits were used to estimate model complexity (i.e. model order) by providing the optimal number of (seasonal)
autoregressive terms, (seasonal) moving average terms, and the degree of (seasonal) differencing (i.e., values of p,d,q, etc.) required to explain background trends in the outcome measures. Next, for each log-transformed outcome series, three ARIMA models, one with sudden permanent intervention covariates, one with sudden temporary covariates, and one with gradual covariates were fit to the entire data range using the pre-intervention model order (p, d, q, etc) determined for that series.

A total of nine models were fit: (three intervention covariate types) x (three outcome series). For each model, the Ljung-Box Q statistic was checked to determine if the model successfully removed autocorrelation from the residuals. If not, the model orders were adjusted manually in order to achieve proper fit. Next, the model with the lowest Bayesian Information Criterion (BIC) was chosen to represent each outcome time series of interest.

Since the intervention covariates are not orthogonal, a backward elimination procedure was employed for each outcome. In particular, the intervention covariate with the highest non-significant p-value (based on a Wald test) was removed from the model before re-fitting. This procedure was repeated until further removal would either: 1) eliminate a statistically significant intervention covariate, 2) worsen (increase) the BIC, or 3) produce a model with no remaining intervention covariates.

**Estimation of effect size**

For batch one countermeasures, a forecasting approach was used to measure the size of effect associated with any countermeasure that produced a statistically significant effect in the interrupted time series analysis. This involved applying the “pre-intervention” models toward forecasting future values of the outcome series. The difference between the forecasted values and the observed values were taken as an indication of the magnitude of effect.

**Batch Two Analyses (for countermeasures introduced between 2009-2010)**

**Sub-groups**

Given that batch two countermeasures were implemented in temporal proximity, sub-group analyses were conducted with the intention of isolating intervention effects. However, increased data variability due to the reduced sample size for each sub-group
was a drawback. The first sub-group consisted of high-risk drivers, i.e., drivers (outcome 1) or collisions involving at least one driver (outcome 2) who committed a previous drinking and driving offence within the ten years prior to the collision, but not in the three years immediately before the collision. The excluded three-year period was imposed so that none of the selected drivers could have offended after the introduction of the Warn Range Sanction. This sub-group was intended to identify drivers who, due to a previous drinking and driving conviction, may be more strongly influenced by new countermeasures. The second sub-group was composed of drivers/collisions involving at least one driver who was under 22 years old or who held a novice-classed licence at the time of collision. This young/novice sub-group was analyzed in order to focus on the drivers who would be most affected by the Zero BAC Requirements. The complement of this group (i.e., non-young and non-novice) was also analysed in isolation to detect effects of the Seven-day Impoundment. The last sub-group consisted of drivers/collisions with a BAC level of at or above 0.08%. Drivers with BACs in this range are targeted specifically by the Seven-day Impoundment.

Model fitting and testing

All three outcome series covered the period between January 1, 1998 and December 31, 2012 and comprised of data binned into intervals of either 15, 30 or 60 days. PDO collisions were excluded for outcome 1. The time series start date was chosen to produce the largest possible “clean” pre-countermeasure time interval for base model selection. The end date was chosen based on the availability of well-validated data. Different time binnings (15, 30, or 60 days) were employed in order to ensure the smallest possible bin size was used for each analysis. A small bin size could be advantageous in looking for effects of closely spaced countermeasure implementations.

For batch two countermeasures, we constrained the transfer function in Equation 1 by giving it the same degree of differencing as applied to the outcome series, along with zero order MA and AR components. Therefore Equation 1 can be re-written as

\[(1 - B)^d (1 - B^z)^{sd} (r_t - \sum_i a_i x_{it}) = \mu + T_{alc}(B)x_{alc,t} + \frac{\theta_q(B)\theta_{sq}(B)}{\phi_p(B)\phi_{sp}(B)} \epsilon_t,\]

where \(x_{alc,t}\) is an alcohol consumption proxy, and the \(a_i\)’s are a measure of intervention effect. If the intervention covariates are taken as sudden permanent (step functions), then \(a_i\) has a straightforward interpretation as the exact amount by which a countermeasure shifted an outcome. Consideration of only sudden permanent
covariates was reasonable for batch two because of the short post-implementation period for which data was available.

To stabilize variance, ratio measures were log-transformed before fitting. The seasonal period, $s$, in Equation 2 was set to 24, 12, or 6, for outcome measure bin widths of 15, 30, or 60 days, respectively, producing an approximately year-long period in all cases.

Fitting Equation 2 directly to the outcome series would be risky because high MA, AR, etc. orders could sufficiently account for abrupt shifts in the data, even in the presence of true intervention effects. Therefore, the values of $p$, $d$, $q$, $sp$, $sd$, and $sq$ (i.e., the model order) were constrained before fitting the full model including covariates. In this way, the model would be “forced” to use the intervention covariates if such effects exist. This restriction of model order was performed using only pre-intervention data, and is analogous to choosing linear, versus quadratic, etc. polynomials when performing ordinary least squares regression.

For each outcome series, the final model was constructed using a three-step procedure. First, Equation 2 without intervention covariates was fitted to the outcome series between January 1, 1998 and January 1, 2007. During this time, no alcohol-related countermeasure was introduced, so the resulting model should have been of sufficient complexity (order) to capture background trends in the outcome series of interest. In step two, this assumption was checked by using the fitted model from step one to forecast the outcome series over the short time period from January 1, 2007 to July 1, 2008, which also lacked any alcohol-related countermeasure changes. The model order was then manually adjusted to minimize prediction error over this second time interval, ensuring the final model could adequately capture background trends. Steps one and two were repeated for 15-, 30-, and 60-day data binning. The bin size that minimized prediction error the most at step two was chosen for final analysis. The Ljung-Box Q statistic was also checked to ensure the fitted model de-correlated residuals.

The third and final step of the model construction procedure consisted of fitting Equation 2 with intervention covariates over the full time period up to December 31, 2012. For this final fitting, model order was constrained to that determined at the end of step two, but all parameters (coefficients of MA, AR, etc. terms, as well as intervention coefficients) were free to vary. In this way, the fitting algorithm could not simply add more model terms to account for a true interruption, but would be forced to use the intervention covariates if such interruptions were present. The Ljung-Box Q statistic was
checked to ensure the final model de-correlated the time series residuals.

Statistical significance for each intervention coefficient in Equation 2 was checked using a Wald test, and approximate 95% confidence intervals were taken to be 1.96 times the standard error of the coefficient estimate. Given the form of Equation 2 and the simple step function nature of the intervention covariates, it was straightforward to calculate the values that a given outcome series would have taken if a countermeasure had not existed (i.e., if the statistically significant intervention coefficient, \( a_i \), were equal to zero). Specifically, the observed outcome series would simply have been shifted vertically by an amount equal to the negative of that coefficient, but only for the period after countermeasure implementation. Since our outcome measures are ratios, the shift magnitude along with the observed non-alcohol related denominator used in constructing the outcome ratio could be used to estimate the absolute reduction of alcohol-related outcomes associated with the countermeasure of interest.

For batch two, two collision outcomes were considered (outcome measures 1 and 2) along with five groups (the full study population along with four sub-groups), and four intervention covariates (the three countermeasures of interest and the CCC changes). This yields 40 possible statistical tests. However, testing only the Zero BAC coefficient for the young/novice group, only the Seven-day Impoundment coefficient for the non-young/non-novice group, only the Seven-day Impoundment coefficient for the BAC at or above 0.08% group, and leaving out Zero BAC for the high-risk group reduced this number to 20. This number of comparisons could easily lead to Type I errors, which would make a countermeasure look effective even if it were not. The 20 comparisons were not independent from one another, but we still adopted the most conservative multiple comparison correction to control Type I error probability (Bonferroni correction) and set statistical significance at \( \alpha = \frac{0.05}{20} = 0.0025 \).

Additional outcomes for young drivers
An intervention effect of the Zero BAC requirements on collision-related outcomes was difficult to detect due to small sample size. As such, supplementary offence-related outcomes were included. First, we examined a time series consisting of the ratio of the number of 90-day ADLS given to drivers under 22 to the number of these sanctions given to the population 22 and over. Second, we examined the proportion of all Warn Range Sanctions given to young drivers in the year before and after Zero BAC.
implementation. A decrease in either of these measures across Zero BAC implementation might not be particularly meaningful in isolation. However, since 90-day ADLS and Warn Range Sanctions differ substantially in terms of who they target, how they are applied, and the consequences for the driver, a decrease in both measures would be consistent with a decrease in drinking and driving amongst young drivers.

For the 90-day ADLS outcome, we performed an ARIMA-based interrupted time series analysis on the ratio of the number of suspensions given to younger drivers to the number given to non-young drivers using the three-step procedure described above. Since the Warn Range Sanction was introduced just over a year before Zero BAC, we compared the proportions of Warn Range Sanctions given to young drivers in the year before Zero BAC introduction to the proportion given to them in the subsequent year using Pearson's chi-squared test.
Part II. Specific Deterrence Analyses

90-day Administrative Driver’s Licence Suspension

The 90-day ADLS, issued immediately at roadside, was designed to reduce further drinking and driving incidents by recipient drivers for the duration of the suspension. In cases where conviction for a criminal drinking and driving offence has not occurred by the end of the 90-day ADLS, it is also hoped that experience of the 90-day suspension would reduce drinking and driving by recipient drivers at least until the time of conviction (should it occur), when additional sanctions would apply. To measure the effect of the 90-day ADLS countermeasure, we examined only drivers who were charged and convicted under CCC s. 253 or 254. Drivers who are never convicted can also receive a 90-day ADLS (because it is applied at roadside even before charging), however we have no information about drivers who were not convicted criminally before the 90-day ADLS program began. As such, restriction to convicted drivers was necessary to perform time series analysis.

The two outcomes for which the specific deterrence capacity of the 90-day ADLS was evaluated were derived from a population of drivers convicted under CCC s. 253 or 254 for drinking and driving. For the first outcome measure, a decrease in the number of such drivers who recidivate (i.e. commit a second CCC s. 253 or 254 offence leading to conviction) between initial offence and conviction for that initial offence would be indicative of an intervention effect. The second outcome measure examined recidivism in the post 90-day ADLS/pre-conviction period. All data were obtained from the LCS for drivers convicted of a CCC s. 253 or 254 offence committed between November 28, 1991 and November 28, 2001.

To prepare the data for the first outcome measure, the time between November 28, 1991 and November 28, 2001 was divided into quarterly (three month) time bins. The number of drivers, $N_{90}$, who committed an offence under CCC s. 253 or 254 within a given time bin and who subsequently committed a second such offence during the first 90 days after their initial offence was taken as the outcome for that bin. Changes in recidivism rates (i.e. $N_{90}$) at the time of countermeasure implementation could occur either because the 90-day ADLS is having a direct effect or because the rate of drivers committing an original CCC s. 253 or 254 offence changed over time. As such, an exposure measure, $E_{off}$, was constructed from the total number of drivers committing a CCC s. 253 or 254 offence in a given time bin, regardless of whether they subsequently
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recidivated. A sudden change in the 90-day recidivism proportion time series, $P_{90} = N_{90}/E_{off}$, at the time of 90-day ADLS implementation would be consistent with program effectiveness.

To prepare the data for the second outcome measure, the time gap between offence and conviction dates were calculated and its distribution was examined. Between November 28, 1991 and November 28, 2001, approximately 36% of drivers convicted under CCC s. 253 or 254 had an offence-to-conviction time gap greater than six months. We created a three-month follow-up window for each driver, which covered the time between 90 days and six months after their initial offence, and examined outcome events occurring in that follow-up window. In order to perform interrupted time series analysis, the time between November 28, 1991 and November 28, 2001 was again divided into quarterly time bins. The number of drivers, $N_{follow}$, who committed an offence under CCC s. 253 or 254 within a given time bin and who subsequently committed a second such offence during their follow-up window was taken as the outcome for that bin. The exposure, $E_{follow}$, for that time bin was the total number of drivers who committed a CCC s. 253 or 254 offence within that bin, regardless of whether they recidivated in their follow-up window. Importantly, in order to contribute to either $N_{follow}$ and/or $E_{follow}$, a driver must not have recidivated within the first 90 days after their original offence. A sudden change in the follow-up recidivism proportion time series, $P_{follow} = N_{follow}/E_{follow}$, at the time of 90-day ADLS implementation would be consistent with program effectiveness.

Analyses were conducted using an interrupted time series approach based on segmented Poisson/negative binomial regression. The two outcome measures contain a count variable, denoted by $N_x$, representing the number of outcomes in a given time bin, as well as an exposure count, denoted by $E_x$. We modeled the $N_x$ variables as negative binomial variables. The expected value of counts, $\bar{N}_x$, was modeled as

$$\log(\bar{N}_x) = \log(E_x) + \beta_0 + \beta_1 t + S_i + \beta_0^I I,$$  \hspace{1cm} (3)

where $t = 1, 2, 3, ..., 41$ is time-bin number and $I$ is a binary variable taking a value of zero before 90-day ADLS implementation and one after. A non-zero value of $\beta_0^I$ indicates the presence of a level shift in the log of the outcome ratio at the time of countermeasure implementation. Here, $S_i$ ($i = 1, 2, 3, 4$) is a seasonal factor that varies.
Equation 3 was fit separately, using the IBM SPSS v21 GENLIN routine, to data for both outcome measures. After fitting, first without the seasonal factor, the Ljung-Box Q statistic was used to assess autocorrelation in the model residuals. Residuals were created by subtracting the observed proportion, $P_x$, from the model prediction, $\hat{P}_x$. If autocorrelation was found, then the seasonal factor was added to the model before re-fitting and re-testing.

**Seven-day Impoundment**

The Seven-day Impoundment program was designed to work in tandem with the 90-day ADLS by removing the immediate threat posed by a drinking driver. Additionally, the increase in roadside sanction severity created by the seven-day impoundment was hoped to increase compliance with the HTA-mandated 90-day ADLS by deterring incidences of DWS while subject to the 90-day ADLS. Accordingly, the first outcome measure is the incidence of DWS during the 90-day ADLS.

Many 90-day ADLS recipients will have their licence re-instated at suspension end because they will not (yet) have been convicted criminally of the offence associated with the initial drinking and driving incident. A second possible benefit of the seven-day impoundment countermeasure would be to decrease incidents of drinking and driving after licence re-instatement from the 90-day ADLS, but before any criminal drinking and driving conviction (should it occur). As such, the second outcome measure involves drinking and driving detection in the period after the 90-day ADLS but before a CCC conviction. Data were obtained from the LCS for drivers who received a first 90-day ADLS between December 1, 2005 and December 1, 2013.

To prepare the data for analysis of the first outcome measure, the period between December 1, 2005 and December 1, 2013 was divided into two-month time bins. The number of drivers, $N_{DWS}$, with a first 90-day ADLS starting in a given time bin who subsequently committed a DWS offence during the suspension period (and was later convicted) was taken as the outcome for that bin. However, this number is only meaningful relative to the amount of opportunity for such offences to occur. With this in mind, an exposure measure, $E_{\text{susp}}$, was constructed from the total number of drivers with a 90-day ADLS starting in that time bin, regardless of whether they subsequently drove while suspended. A sudden change in the $DWS$ ratio time series, $R_{DWS} = \ldots$
\( N_{DWS}/E_{susp} \), at the time of Seven-day Impoundment implementation would be consistent with program effectiveness.

To prepare the data for analysis of the second outcome measure, the study population was restricted to drivers who were either never convicted for their offence or who did not have a disposition of their charges in the three-month following termination of the 90-day ADLS. The period between December 1, 2005 and December 1, 2013 was divided into three-month time bins. The number of drivers, \( N_{\text{recid}} \), with a first 90-day ADLS starting in a given time bin who subsequently received a second 90-day ADLS during their three-month follow-up window, was taken as the outcome for that bin. Exposure, \( E_{\text{follow}} \), consisted of the number of drivers with a 90-day ADLS starting in that time bin who had a full three-month follow-up window, regardless of whether they recidivated in that window. A sudden change in the recidivism ratio time series, \( R_{\text{recid}} = N_{\text{recid}}/E_{\text{follow}} \), at the time of Seven-day Impoundment implementation would be consistent with program effectiveness.

Analyses were conducted using an interrupted time series approach based on segmented Poisson/negative binomial regression. The outcome measures each contain a count variable, denoted by \( N_x \), representing the number of outcomes in a given time bin, as well as an exposure count, denoted by \( E_x \). We modeled the \( N_x \) variables as either Poisson or negative binomial variables, depending on the absence or presence of over-dispersion. The expected value of counts, \( \hat{N}_x \), was modeled as

\[
\log(\hat{N}_x) = \log(E_x) + \beta_0 + \beta_1(t - t_i) + S_i + \bar{S}_i t \\
+ \beta_0 I + \beta_1 I(t - t_i),
\]

where \( t = 1, 2, 3, ..., m \) is time-bin number (\( t = t_i \) corresponds to the bin number immediately after the countermeasure of interest was implemented), \( I \) is a binary variable taking a value of zero before implementation and one after. A non-zero value of \( \beta_0 I \) or \( \beta_1 I \) indicates the presence of a level shift or slope change in the log of the outcome ratio of interest at the time of countermeasure implementation. Here, \( S_i \) (\( i = 1, 2, ..., k \)) is a seasonal factor that varies according to bin within year, with \( k \) being six for two-month bins, or four for three-month bins. Similarly, \( \bar{S}_i \) represents a seasonal factor that interacts with time, allowing for growing or shrinking seasonal fluctuation in outcome over time.
Equation 4 was fit, using the IBM SPSSv21 GENLIN routine, to all outcome measure data by first treating $\mathcal{N}_x$ as negative binomial, and without the seasonal factors. If the negative binomial parameter was not significantly different from zero, a Poisson distribution was adopted. After this fitting stage, the Ljung-Box Q statistic was used to assess autocorrelation in the model residuals. Residuals were created by subtracting the observed ratio, $R_x$, from the model prediction, $\hat{R}_x$. If autocorrelation was found, then one or both seasonal factors were added to the model to remove it before re-fitting and re-testing.

**Long Term Vehicle Impoundment Program**

The Long Term Vehicle Impoundment program was introduced as a means to enhance compliance with criminal driving prohibitions and, thereby, to improve their effectiveness. The first outcome measure examined changes in the number of driving while prohibited (DWP) offences committed during the period of criminal drinking and driving prohibitions received for CCC s. 253 or 254 convictions. This countermeasure also carries an escalating component, whereby the duration of the impoundment will be extended if the vehicle has been impounded previously. Increasing impoundment durations were designed to address instances where one or more drivers use a particular vehicle repeatedly to commit the same offence. Thus, our second outcome measure examines changes in DWP amongst previous offenders. In this instance, previous offenders were defined as those drivers who had already committed a DWP offence within the previous five years. Driver records were obtained from the LCS for all drivers who committed an offence (i.e. were subsequently convicted) between January 1, 1994 and January 1, 2009 under CCC s. 253 or 254. Any CCC s. 253 or 254 offence record with missing suspension dates or a corresponding prohibition length of less than 350 days was removed from the data.

To prepare the data for analysis of the first outcome measure, the period between January 1, 1995 and January 1, 2009 was divided into two-month time bins. Within any given bin, the total number of DWP offences committed by drivers during criminal driving prohibitions (for CCC s. 253 or 254 convictions), $N_{DWP}$, was counted as the outcome. The exposure measure, $E_{prohib}$, consisted of the number of drivers prohibited under CCC s. 253 or 254 for the majority of that time bin. Any sudden change in the DWP ratio time series, $R_{DWP} = N_{DWP}/E_{prohib}$, at the time of implementation would be consistent with program effectiveness.
For the second outcome measure, $N_{DWP}$ was restricted to counting DWP offences committed during criminal driving prohibitions (for CCC s. 253 or 254 convictions) by drivers who had already committed a DWP offence within the previous five years. Similarly, $E_{prohib}$ was restricted to counting drivers criminally prohibited for conviction under CCC s. 253 or 254 who had committed a DWP offence within the previous five years. Using these restricted variables, we created a “previous offender” DWP ratio. Modeling was performed using identical methodology to that used with Seven-day Impoundment.

**Warn Range Sanctions**

As the unique nature of the Warn Range Sanctions is in its escalation, our evaluation examined two outcome measures, the first being elapsed time between Warn Range Sanctions, where drivers subject to these sanctions multiple times are presumed to demonstrate a decreasing re-occurrence rate, i.e., they “slow down”; and the second being cumulative re-occurrence rate, where drivers who have received two sanctions are presumed to drink and drive less than equivalent drivers who have only received one sanction.

To create the first outcome measure, data were obtained from the LCS for drivers who received three or more alcohol-related suspensions between May 1, 2009 and November 25, 2014 where the first two suspensions were Warn Range Sanctions and where the third suspension was for driving with any presence of alcohol. Drivers with alcohol-related offences prior to May 1, 2009 were excluded. Next, suspensions for every driver were ordered sequentially and aggregate measures were calculated across all drivers for a) the time period in days between the first and second suspension “$T_1$”; and b) the time period in days between the second and third suspension “$T_2$”. If $T_2$ is found to be significantly larger than $T_1$, this could indicate the presence of a decreasing rate of offence for multiply offending drivers, i.e., drivers subject to escalating Warn Range Sanctions slow down. However, any observed effects could also be a result of changing background trends; as such, the rate of first Warn Range Sanctions in the general population was examined. If a slowing effect was found, but less sanctioning was occurring in the general population, the cause of the latter might be responsible for the former.

To create the second outcome measure, data were obtained from the LCS for drivers who received at least one Warn Range Sanction between May 1, 2009 and November 25, 2014. This time period was then divided into three sub-periods, with the first ranging
from May 1, 2009 to December 31, 2010, the second from January 1, 2011 to July 31, 2012, and the third from August 1, 2012 to November 25, 2014. Two groups of drivers were then created based on the occurrence of sanctions with the three sub-periods. In the one sanction group, drivers were retained if they had one Warn Range sanction, their “index suspension”, in the second sub-period (but no other alcohol-releated offences), and no alcohol-related sanctions in the first sub-period. In the two sanction group, drivers were retained if they had one Warn Range sanction, their “index suspension”, in the second sub-period (but no other alcohol-releated offences), and one in the first sub-period (but no other alcohol-releated offences). Both groups were followed up for two years after their index Warn Range suspension. A subsequent alcohol-related suspension occurring in the follow-up window was the outcome of interest.

Naturally, the two-sanction group is likely to contain more higher-rate offenders than the one sanction group. Thus, the former group would be expected to re-offend twice as often as the one sanction group, even if Warn Range Sanctions had no effect. In order to make the two groups comparable, we attempted propensity score matching on age, gender, and offence history. Propensity score matching acts on two groups that are dissimilar from each other on “nuisance” characteristics by selecting sub-samples from the two groups that are equivalent on those characteristics. In this way, it is hoped that the only difference between the two matched groups is the variable of interest (number of previous Warn Range suspensions). In order to calculate offence history, all relevant offences were grouped into eight offence categories (see Appendix 1). A single count variable containing the number of times a driver committed an offence in a category over the previous five years was constructed for each such category. Propensity score matching was conducted with a 1:1 ratio, with cases being selected from the larger group to match the smaller group. Any statistical significance in between-group differences on the matching variables was removed. The calculation of propensity scores and subsequent matching were conducted using a Python extension for SPSS. Unfortunately, matching does not appear to have been effective in removing between-group differences (see Results).

Zero BAC Requirements

GLS restrictions for novice-classed drivers are intended to socialize a new driver into a safe driving culture by providing them opportunity to gain skill, experience, and maturity under more controlled driving conditions. A similar logic can be applied to young drivers. The Zero BAC restrictions act both as a licence condition as well as a regulatory
deterrent against undesirable behaviour, in this case, drinking and driving. Evaluation of the Zero BAC requirement was done to determine whether its corresponding sanctions serve as an effective learning mechanism.

Data were obtained from Ontario’s Integrated Court Offences Network (ICON) database for drivers with a first or only Zero BAC charge at the time of disposition, i.e., at trial, with the date of disposition being between August 1, 2010 and November 16, 2012. Driver records from the LCS were also extracted and linked to the original data set via driver identifiers. Some drivers charged with a Zero BAC offence were convicted and thus sanctioned for breach of Zero BAC requirements. For these drivers, we verified that their suspension was recorded in the database and that the suspension duration was 30 days. This serves as an additional verification that the incident was a first Zero BAC for all drivers, but more importantly, this confirms that novice-classed drivers included in the sample did not receive an escalating sanction for a previous non-Zero BAC related licence condition contravention. The remaining, unconvicted drivers formed a control group.

Restrictions were placed on the study population, so that the remaining sample consisted of drivers who were not already suspended on their Zero BAC offence date or on the corresponding disposition date; who did not receive a non-Zero BAC suspension on or immediately after disposition date; and who did not receive a non-alcohol related suspension at any point in the two years after disposition. These restrictions are severe, but were found necessary to make the exposed and control groups similar enough to allow meaningful comparison of recidivism.

The two-year cumulative recidivism rates (BAC at or above 0.05%) were calculated and compared between exposed (convicted) and control (un-convicted) groups after adjusting for driver offence history in the five years prior to their Zero BAC offence. Analyses were conducted with a binary logistic regression equation. We verified that outcomes did not occur in the 30-days immediately after disposition, as that would have been the period during which the convicted group was subject to their licence suspension. The analyses were repeated after restricting the study population to those under 22 years of age.
Alcohol Education and Treatment Remedial Measures Program

Ontario’s remedial measures program was intended to rehabilitate drivers through alcohol education and treatment, with the goal of reducing negative road safety outcomes through imparting knowledge and provoking changes in drinking and driving attitude.

High-quality interrupted time series techniques could not be used to evaluate the specific deterrence produced by the BOT program because it was implemented in a staggered fashion. Exposure of an offending driver to the single-component, education-only program versus the multi-component program was determined by conviction date, while exposure to the program itself was determined by offence date, so there is no clear “time” variable upon which to build a time series. Instead we were forced to rely on a simple pre-post comparison of recidivism between those subject to the program (post) and those who were not (pre).

In order to account for background time trends in recidivism that have the potential to confound a pre-post design, we first examined recidivism in the four years leading up to BOT implementation. This allowed us to establish a baseline rate of change against which the magnitude of any pre-post differences in recidivism could be compared. We first calculated a monthly recidivism proportion covering the four-year period immediately before BOT implementation. The recidivism proportion for each month consisted of a denominator equal to the number of drivers charged with a CCC s. 253 or 254 offence during that month who were subsequently convicted of that offence. The numerator consisted of the number of those drivers who subsequently re-offended (and were eventually convicted) within three years from the end of their corresponding CCC driving prohibition. Drivers re-offending between initial offence and CCC prohibition end were excluded from both the numerator and denominator. Poisson regression was used to model the numerator of the recidivism proportion, with the denominator providing an exposure offset, and time being the only covariate predictor. Residual autocorrelation was assessed with the Ljung-Box Q statistic.

For our main analyses two outcome measures were examined: first, drinking and driving recidivism by offenders after the end of their prescribed CCC driving prohibition, regardless of whether their licence had yet been reinstated; and, second, recidivism by drivers who completed the program, and whose licences were reinstated, even if this
occurred after their prescribed CCC prohibition end date. Sub-analyses were also conducted to determine the effects associated with program completion time and age.

Data were extracted from the LCS for drivers who were charged with and subsequently convicted of an alcohol-impaired driving offence (under CCC s. 253 or 254) between November 29, 1996 and December 23, 2001. All drivers with an index offence during this time period would have received the 90-day ADLS, but would not have been subject to the Ignition Interlock Program that took effect in the beginning of 2002. The study population was restricted to those who had no previous criminal alcohol involved driving offence that subsequently led to a conviction in the five year period before their index offence, who were not already suspended at the time of their index offence, and who, upon conviction, received the standard one-year driving prohibition under the CCC.

Three study groups were constructed from the study population. First, an unexposed “No BOT” group was created from drivers with an index offence between November 29, 1996 and September 29, 1998, during which time no remedial measures program was in effect. This group only had to wait out their one-year CCC driving prohibition to have their licence re-instated. Second, an education only group, “Edu BOT”, was created from drivers with an index offence after September 29, 1998 and a corresponding conviction on or before October 1, 2000. These drivers were exposed to a partial version of the BOT program that only required them to complete a one-day alcohol education course before the end of their CCC mandated one-year driving prohibition. Third, a “Full BOT” group was created from drivers with an index offence between September 29, 1998 and December 23, 2001 and a corresponding conviction after October 1, 2000. This group of drivers received the Full BOT program that involved an initial assessment, a one-day education program or a two-day treatment program depending on assessment results, and a brief follow-up interview conducted six months after completion of education/treatment. This full version of the BOT program is currently in effect.

For the first outcome, comparisons were made on the three-year cumulative recidivism rate between the No BOT and Edu BOT groups, and between the Edu BOT and Full BOT groups, where the follow-up window for a given driver began on their prescribed CCC driving prohibition end date. The Full BOT and No BOT groups were not compared directly to each other because the index offence intervals for these groups were not temporally adjacent, potentially increasing the effect of time-varying confounds implicit in pre-post designs. In order to have their licence re-instated, drivers in the BOT groups
had to complete all program components, including the follow-up interview for those in the Full BOT group. Failure to do so by the end of their one-year CCC driving prohibition would render a driver indefinitely suspended until completion of all program requirements. The indefinite suspension, to which the No BOT group was never exposed, is as an active component of the overall BOT countermeasure. Thus, the first outcome evaluates the effectiveness of the BOT program as a “package”.

The second outcome measure is concerned with the effects of BOT “components”, solely referring to the assessment, education, treatment, and follow-up components, on recidivism after licence reinstatement for drivers who completed the program. Specifically, the three-year cumulative recidivism rate, which was calculated based on a three year window starting on each driver’s re-instatement date, was compared between the three BOT groups in a stepwise fashion, as above. Given that only some drivers completed the BOT requirements within their one-year CCC prohibition (i.e., on-time completers), we performed comparisons described above separately for this group, and for a group of late completers, who completed BOT requirements after their prescribed one-year CCC prohibition end date but within the following year. For comparisons restricted to the group of late completers, a driver’s three-year follow-up window began on their remedial measures program completion date.

We also compared on-time and late completers to each other on the outcome measure and on certain demographic characteristics. The latter includes age, gender, licence class, and offence-to-conviction time period. We then calculated propensity scores on these variables and matched on-time and late completers according to the propensity scores to investigate whether these demographic characteristics accounted for between-group differences in recidivism. In other words, we examined and matched on demographic characteristics so as to investigate and subsequently treat potential selection bias.

Finally, we examined whether the putative effect of the BOT program is carried primarily by one or another age group, by performing stratified analyses on the No BOT and Edu BOT groups, using three age groups.

**The Ignition Interlock Program**

Drivers with an alcohol-related criminal driving conviction receive an ignition interlock condition on their licence after the expiration of the criminal driving prohibition and the fulfilment of provincial requirements. The condition allows them to drive legally only if
there is an ignition interlock device installed in the vehicle. As drivers are given the choice to install the device, two groups naturally emerge, one group of “installers” and one group of “non-installers”. These groups were compared on two main outcome measures: 1) drinking and driving recidivism during the interlock condition and 2) drinking and driving recidivism after removal of the condition. Drivers were also compared on collision outcomes during the interlock condition period.

Data were extracted from the LCS for drivers who were convicted for the first time of an alcohol-related criminal driving offence under CCC s. 253 or 254 for an offence that occurred after December 23, 2001, with a corresponding conviction date before August 3, 2010. The study population was further restricted to those whose interlock condition started on or after January 1, 2005, since very few offenders installed prior to this date, to those whose driving prohibition period lasted one year, and to those who did not have a criminal drinking and driving conviction for an offence committed in the 10 years prior to their index offence.

For the first outcome, the sub-group of these drivers who installed an interlock (“On Interlock”) for at least 31 days was compared to the non-installing (“Never Installed”) sub-group on the binomial proportion who re-offended under CCC s. 253 or 254 during their interlock condition. For the “On Interlock” group, re-offences were only captured if they occurred while a device was installed, as some drivers had chosen to install the device for only a portion of their entire interlock condition. Since the proportion of re-offenders was small in each group, recidivism events were treated as Poisson counts and were entered into a Poisson regression, with group membership being the sole (binary) predictor. Exposure offsets were taken to be the total “at-risk” time for each group, i.e., the total interlock install time for the On Interlock group and the total interlock condition time for the Never Installed group. Groups were matched on age at offence, gender, and offence history in the five years prior to conviction (most previous studies have not taken the step of matching on detailed offence history). An additional calculation was made for the “On Interlock” group; re-offences that occurred during the interlock condition but while a device was not installed were captured as rates in person-time (“Off Interlock”). This latter quantity did not form the basis of any formal analysis, but was intended to refute a concern that those who chose to install for some period of time were systematically lower rate offenders, even after matching, than those who chose to never install.
For the second outcome, collision rates during installation (On Interlock group) were compared to collision rates during the control interlock condition (Never Installed group). All reportable collisions were included in the analyses regardless of whether the collision resulted in property damage, injury, or fatality. Analyses were conducted using Poisson regression, as above.

For the third outcome, drivers who installed an interlock ("On Interlock") for at least 300 days of their interlock condition was compared to the non-installing ("Never Installed") group in terms of their time to first re-offence after the interlock condition had been removed from their licence. Outcomes were defined as drinking and driving incidents leading to a CCC s. 253 or 254 conviction. Groups were matched on age at offence, gender, and offence history in the five years prior to conviction. Cox regression was performed after propensity score matching with additional covariates to adjust for residual differences in the matching variables. Censoring occurred at the end of the study period (November 25, 2014). Because the assumption of proportionality was violated, as tested using Cox’s (1972) method, time-to-event analyses were replaced with a comparison of the three-year cumulative recidivism rate using logistic regression. Potential confounders were treated via model adjustment, first using un-matched data, then using matched data.

**The Reduced Suspension with Ignition Interlock Conduct Review Program**

The Conduct Review program is a modification of the pre-existing Ignition Interlock Program. It was designed to meet three goals. The first goal is to increase installation uptake through an incentive (i.e., reducing the driving prohibition period from 12-months in the original interlock program to three- or six- months), as interlock devices cannot be effective until installed, and have been proven to be effective while installed. The second goal is to motivate long term effects via a behavioural reward (i.e., interlock condition removal without extension). Stream A and B participants are subject to a conduct review component where data is routinely downloaded to the service provider are used to monitor performance. If drivers commit a performance failure in the last three months of their interlock condition, they are required to keep the device installed for another three months. These three-month extensions may continue indefinitely until a driver learns to separate drinking from driving, thereby promoting long term behavioural change and a sustained decrease in recidivism after device removal. The third goal is to engage offenders with rehabilitation sooner by mandating a 90-day
maximum elapsed time between offence and conviction as an eligibility criterion. These three goals correspond to the four outcome measures.

Data were obtained from the LCS for drivers with a first-time alcohol-related driving conviction under CCC s. 253 or 254 between August 4, 2005 and August 4, 2012. For the outcome measures corresponding to the elapsed time between offence and conviction, the study period was reduced to August 4, 2007 and August 4, 2012.

To examine changes in installation uptake, the study population was further restricted to those with an interlock condition applied within two-years of their suspension start date. In this population of drivers, we examined changes over a seven-year period in the number of people who installed an interlock device within the three-months before and six-months after an interlock condition was placed on their licence relative to the number of people with an interlock condition on their licence. Data was binned bi-monthly, with a driver’s outcome data contributing to a bin if they were convicted (and started their prohibition) within the time covered by that bin. This bi-monthly proportion of installers was modeled as a saturating exponential function with a multiplicative “interruption” at program implementation, as follows

\[ \hat{p}(t) = \left(1 + h_{t>t_{RS}} \alpha \right) \left( b + a \left( \frac{1-e^{-\beta t}}{1+e^{-\beta t}} \right) \right)^y, \]  

where \( \hat{p}(t) \) is model-predicted install proportion, \( h_{t>t_{RS}} \) is a step function that is coded as zero before program implementation and one after; \( t \) is time; and \( \alpha \) is the multiplicative discontinuity magnitude. Two outliers, corresponding to data points near program implementation, were excluded. Equation 5 was fit to the observed data using a weighted least squares approach, where weight was the inverse variance expected for a binomial variable with observed proportion \( p(t) \) and total count \( n \). Thus, the weights were chosen as \( \frac{n}{p(t)(1-p(t))} \), where \( n \) is the number of interlock conditions resulting from conviction at time \( t \). In this way, the fitting procedure corresponded to maximum-likelihood. Confidence intervals for the estimated value of alpha were calculated via bootstrapping (10,000 re-samplings). Residual auto correlation was checked with the Ljung-Box Q statistic.

To examine changes in the offence to conviction time period, the time between each driver’s offence and conviction dates was averaged for all drivers with a conviction in a given time bin. The resulting time series was modeled with a segmented linear regression, which included an intervention covariate that was set to zero before
program implementation and to one after program implementation. Autocorrelation was treated by including a seasonal factor into the model, as described previously for the impoundment evaluation methodology.

To examine the last outcome, of long-term recidivism, data were extracted from the LCS for drivers convicted of a first-time alcohol-related driving under CCC s.253 or 254, with conviction occurring on or after August 3, 2010. The study population was further restricted to those invited to participate in the Conduct Review program. Three groups were naturally formed within this population: Stream A, Stream B, and Stream C offenders. Drivers were identified into one of these three groups based on the “interlock condition code” on their licence, assigned after drivers respond to the Conduct Review Program invitation by completing several follow up steps, and based on the “interlock registration code” on their licence, assigned after drivers have installed the device with a service provider. Data was reduced by removing drivers with inconsistent interlock and registration codes. The Stream A and Stream B groups were further restricted to those drivers who installed an interlock device for 100% of their ignition interlock condition with a 33-day grace period. Under the HTA, if drivers do not install within 30 days, they are deemed to have violated the conditions of the program and defaulted to Stream C. The additional three days grace was added to our selection criteria as a buffer for early de-installation (e.g., removal on device on Friday if the condition is lifted on Monday).

In order to test the effectiveness of performance failure based extension, we allowed drivers in Stream A and B groups to have received up to two three-month extensions. Drivers with a larger number of extensions represented far less than 1% within each group and produced no post-interlock recidivism, most likely due to their low numbers. The Stream C group consisted of drivers who chose not to install an interlock device, thus defaulting to Stream C.

For all drivers, we verified that they were prohibited from driving for the prescribed time period corresponding to their Stream, and allowed 45-day grace period on either end. Drivers were eliminated from the study population if they removed their interlock devices for any reason other than having completed the program. Propensity score matching between groups was conducted to minimize pre-existing group differences; and were calculated from age, sex, neighbourhood level measures taken from Statistics Canada’s National Household Survey (2011) (see Appendix 2), and aggregate measures of past CCC and HTA infractions in the five years prior to their index offence (see Appendix 1). Residual differences after matching were treated through adjustment, by including these
variables as model covariates.

Survival analyses were conducted separately on Stream A and Stream B, with the reference group in both cases being Stream C, i.e., the original interlock program. A Cox regression was performed, with 90-day ADLS or Warn Range Sanctions taken a failure events, and November 25, 2014 as the censoring date.
RESULTS

90-day Administrative Driver’s Licence Suspension

The general deterrence capacity of the 90-day ADLS was evaluated along with that of the Alcohol Education and Treatment Remedial Measures Program and the Ignition Interlock Program as part of our “batch one” analysis. ARIMA models were first fit to three outcome series, i.e., the drinking driver ratio, the D&D injury/fatality ratio, and the D&D major injury/fatality ratio, for the period between January 1988 and November 1996 (the date of 90-day ADLS implementation). Model fit statistics are shown in Table 1. The Ljung-Box Q statistic shows that the selected models accounted for autocorrelation, while R-squared values were reasonable.

Table 1. Pre-ADLS Model Statistics

<table>
<thead>
<tr>
<th>Outcome Time Series</th>
<th>ARIMA model order</th>
<th>R-squared</th>
<th>Ljung-Box Q p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Drinking driver ratio</td>
<td>(0,1,1)(0,1,1)</td>
<td>.609</td>
<td>.233</td>
</tr>
<tr>
<td>Injury/fatality ratio</td>
<td>(0,1,1)(0,1,1)</td>
<td>.565</td>
<td>.090</td>
</tr>
<tr>
<td>Major Injury/fatality ratio</td>
<td>(0,0,1)(0,1,1)</td>
<td>.345</td>
<td>.495</td>
</tr>
</tbody>
</table>

These “pre-ADLS” models formed an appropriate initial guess for the complexity (order) of the ARIMA models to be used for interrupted time series analysis of all three batch one countermeasures. The pre-ADLS models were also used to perform the forecasting analysis for the 90-day ADLS countermeasure. Reliable evidence was found only for an effect of the 90-day ADLS on the major injury/fatality outcome (see Figure 9 for raw monthly major injury/fatality data). In particular, the 90-day ADLS appears to have caused the number of D&D major injuries/fatalities relative to equivalent non-D&D collision outcomes to decrease continuously over a five-year period before re-stabilizing at a lower value. The alcohol sales proxy for alcohol consumption did not make statistically meaningful contribution to the models.
Figure 9. The monthly number of major injuries and fatalities resulting from vehicle collisions in Ontario.

The gray curve represents all such outcomes, while the black curve represents only those injured or killed in collisions in which at least one driver had been drinking. The vertical dashed line represents the date of 90-day ADLS implementation.

Upon close inspection, the data in Figure 10 seems to indicate that D&D major injuries/fatalities continued from a pre-existing decline across 90-day ADLS implementation, whereas equivalent non-D&D outcomes might have stabilized for a number of years. In any case, the size of the effect of the 90-day ADLS, as estimated from forecasting using the pre-ADLS ARIMA model (see Figure 10), was found to be a reduction in D&D-related major injuries/fatalities by approximately 20.3 per month when the differences between forecasted and observed values were averaged over 14 years. A somewhat more conservative estimate, derived from five-year post-implementation averaging, yielded a reduction by 18.8 per month (or 226 per year). Given that the forecasting approach to estimating the 90-day ADLS effect on D&D-related major injuries/fatalities does not take into account the implementation of the Alcohol Education and Treatment Remedial Measures Program or the Ignition Interlock Program, the size of effect may contain contributions from these programs. However, given that the
interrupted time series approach could not find effects of these programs, any such contributions are likely to be small.

Figure 10. Monthly D&D-related major injuries/fatalities ratio. The solid curve represents the monthly D&D-related major injuries/fatalities ratio. The dashed curve represents the ARIMA-predicted values that the black curve would have taken without implementation of the 90-day ADLS countermeasure. The vertical dashed line is the 90-day ADLS implementation date.

Two measures of specific deterrence for the 90-day ADLS countermeasure were considered: 1) the 90-day recidivism proportion, \( P_{90} \), and 2) the follow-up recidivism proportion, \( P_{follow} \). Raw count data for outcome 1 is shown in the top panel of Figure 11. Of particular interest, the number of drivers convicted of an offence under CCC s. 253 or 254 (gray dots) and the number of these drivers who recidivated (black dots) per quarter both decreased across 90-day ADLS implementation. Raw count data for outcome 2 is shown in the bottom panel of Figure 11. Both the number of drivers who were followed up over three months (gray dots) and the number who recidivated within their follow-up window (black dots) decreased across 90-day ADLS implementation.
Figure 11. 90-day CCC s. 253/254 recidivism rates

Top panel: The gray dots represent initial CCC offences and black dots represent the number of such offenders who recidivated with 90 days of the original offence. The black curve represents the model fit.
values, while the vertical dashed line represent 90-day ADLS implementation. Bottom panel: Identical
collections, but with offenders (gray dots) limited to those who remain un-convicted for at least 180
days, leaving a three-month follow-up window starting 90 days after offence. Re-offences (black dots) are
counted only in the follow-up window.

The decrease in the exposure denominator of both outcome measures might possibly
be due to the general deterrent effect of the 90-day ADLS, but this cannot be verified
without additional research. Of most importance here are the outcome measures
themselves, which are plotted in Figure 12.
For outcome 1, the 90-day recidivism proportion, Poisson/negative binomial regression, suggested that the value of \( P_{90} \) at the time bin immediately before 90-day ADLS implementation was a strong outlier relative to the fitted model. After re-fitting with this point removed, the model \( P_{90} \) was found to decrease immediately by 65% (95% Wald CI: [59%, 0.70%]), indicating a significant reduction in the number of CCC s. 253 or 254 offenders recidivating in the 90-days immediately after their original offence. Averaging model fit values over the four quarters before and the four quarters after implementation (in order to remove statistically meaningful seasonal fluctuations), yielded a reduction in recidivism rate from 2.45 to 0.82 recidivating drivers per 100 offenders (a 66% decrease).

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The fitted model indicates an “instantaneous” decrease of 65% at implementation; however, the yearly pre-post averaging, which is more susceptible to the presence of background time trends, indicates a decrease of 66%.

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For outcome 2, the follow-up recidivism proportion was found not to decrease significantly across 90-day ADLS implementation. Instead, a simple temporal trend that continued across countermeasure implementation was observed. However, from inspection of the data in the bottom panel of Figure 12, it seems plausible that a level shift of $P_{follow}$ occurred at the time of 90-day ADLS implementation, but if so, more pre-countermeasure data would clearly be required to find it.

**Warn Range Sanctions**

The general deterrence capacity of the Warn Range Sanctions was evaluated along with that of the Zero BAC Requirements and Seven-day Impoundments as part of the “batch two” analysis. The same three collision-related outcomes were considered for batch two countermeasures as for batch one, but with monthly data covering the time period between January 1, 1998 and January 1, 2013 (see Figure 13). Only the ARIMA-based interrupted time series approach was used in batch two. As such, we could employ simple intervention covariate forms from which effect size estimates could easily be determined.

![Figure 13. Drinking and driving related events over time.](image)

Solid gray curve represents the monthly number of injuries and fatalities resulting from non-PDO vehicle collisions in Ontario. The dashed gray curve represents the monthly number of drivers involved in non-
PDO collisions in Ontario. The black curves represent the D&D equivalents. The vertical dashed line represents that date of Warn Range implementation.

ARIMA-based interrupted times series models with sudden permanent intervention covariates were found for each of the three collision-related outcome series. Model details are presented for the full population in Table 2.

Table 2. Model fit statistics

<table>
<thead>
<tr>
<th>Outcome Time Series</th>
<th>ARIMA model order</th>
<th>R-squared</th>
<th>Ljung-Box Q p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Drinking driver ratio</td>
<td>(0,0,0)(0,1,2)</td>
<td>.649</td>
<td>.700</td>
</tr>
<tr>
<td>Injury/fatality ratio</td>
<td>(0,0,1)(0,1,1)</td>
<td>.625</td>
<td>.937</td>
</tr>
<tr>
<td>Major Injury/fatality ratio</td>
<td>(0,0,[3,2])(0,1,1)</td>
<td>.379</td>
<td>.621</td>
</tr>
</tbody>
</table>

The final fit models all had reasonable R-squared values, and all had insignificant Ljung-Box Q statistics, indicating each model sufficiently de-correlated the time series of interest. The alcohol sales proxy for consumption was not found to contribute significantly to our models.

For both the drinking driver ratio, and the D&D injury/fatality ratio, the Warn Range Sanctions were associated with significant reductions in D&D-related outcomes. After backward elimination, the Warn Range Sanctions were also associated with significant reductions in D&D-related major injuries/fatalities. The observed values of the first two outcome measures are plotted in the top and bottom panels of Figure 14, respectively, along with the ARIMA-based model predictions of what values they would have taken in the absence of the countermeasure.
Figure 14. Drinking and driving-related injuries and fatalities pre- and post-implementation of the Warn Range Sanctions

Top panel. The solid curve represents the drinking driver ratio. The dashed curve represents the ARIMA-predicted values that the black curve would have taken without implementation of the Warn Range countermeasure. The vertical dashed line is the Warn Range implementation date. Bottom panel. Identical conventions, but for the D&D-related injuries/fatalities ratio.
The reduction in the absolute number of drinking drivers involved in collisions was calculated from the model to be $27.3 \pm 14.6$ (95% Wald confidence interval) per month, from 179 (predicted) to 152 (observed), representing a 15% decrease. For D&D injuries/fatalities, a monthly reduction of $44.9 \pm 26.2$, from 261 to 217, representing a 17% decrease, was found. For D&D-related major injuries/fatalities, smaller but significant 12% reduction was found after backward elimination. Without backward elimination, the reduction in this latter measure was not significant at 7.7%. Sub-group analyses revealed no significant effect of any countermeasure on any outcome, with the exception that the Warn Range Sanction showed similar effects on the high risk group as it did in the full group.

To evaluate specific deterrence capacity, drivers with three alcohol-related violations, two of Warn Range type followed by a third of Warn Range or 90-day ADLS type, were divided into two groups. The subgroup of “high rate” offenders was defined as those drivers with time intervals between first and third offence ($T_1 + T_2$) that were less than 2.25 years ($N = 427$). The “low rate” group had $T_1 + T_2$ greater than that time but less than five years. For the high rate group, the mean duration between the first and second suspension, $T_1$, was 271 days. However, $T_2$ did not significantly differ from this at a mean of 254 days (see top panel of Figure 15 for a distribution of $T_2 - T_1$). Low rate offenders ($N = 222$) showed a different pattern, with the mean duration between the first and second suspension, $T_1$, being 574 days as compared to the mean duration between the second and third suspension, $T_2$, at 764 days ($p = 0.001$) (see bottom panel of Figure 15).
Top panel. Distribution of $T_2 - T_1$ values for all "high rate" offenders who received three alcohol-related suspensions, with the first two being Warn Range, within a 2.25 year period. Here, $T_1$ is the time between the first two Warn Range suspensions, and $T_2$ is the time between the second Warn Range suspension and the third alcohol-related suspension. Bottom panel. Identical conventions, but for "low rate" drivers whose three suspensions cover a period greater than 2.25 years, but less than 5.
Since implementation in 2009, the rate at which first Warn Range suspensions are incurred by the driving population decreased from approximately 1200 per month to 600 per month, representing a 50% decrease over five and a half years (see Figure 16). It is not possible to know whether factors responsible for this decreasing trend could also account for the “slowing” of low rate offenders.

![Figure 16. Number of first Warn Range offences generated by drivers with no previous Warn Range suspensions as a function of time since May 1, 2009. A decreasing trend is evident.](image)

A second approach to evaluating the specific deterrent effects was also attempted. We created two groups, a one sanction group who had received only on Warn Range sanction in a specific time period, and a two sanction who had received one Warn Range suspension in the same time period as the one sanction group, but also one previous Warn Range suspension. Detected drinking and driving recurrences captured in a two-year follow-up window were counted for each driver. Based on our selection criteria, it would be expected that drivers in the two sanction group would be twice as likely to recidivate, as they were selected to incur drinking and driving suspensions at a higher rate than the one sanction group. Matching on offence history, gender, and age was attempted to remove group differences. Even after matching, the two sanction group were detected drinking and driving twice as often as the one sanction group, indicating that we could not create comparable groups.
Long Term Vehicle Impoundment

Two measures of effectiveness for the Long Term Vehicle Impoundment program were considered: 1) the bi-monthly number of DWP offences committed during criminal driving prohibitions (for CCC s.253 or 254 convictions) relative to the number of such prohibitions, and 2) the same measure, but only for drivers with a previous DWP offence (and conviction) within five years. Raw count data for outcome 1 is shown in the top panel of Figure 17. Of particular interest, the number of drivers prohibited (gray dots) and the number of DWP offences committed during such prohibition (black dots) both showed a decreasing trend over time, aside from an initial increase, which is an artifact due to how we selected drivers (see Methods). Raw count data for outcome 2 is shown in the bottom panel of Figure 17. Similar trends are evident.
Figure 17. Driving while prohibited events pre- and post- implementation of the long term impoundment

Top panel: Gray dots represent exposure: the bi-monthly number of offenders prohibited from driving under CCC s. 253/254 for an offence occurring on or after January 1, 1994. The black dots represent the bi-monthly number of driving while prohibited offences committed during the CCC s. 253/254 prohibitions depicted in gray. The vertical dashed line represents the implementation of the long-term vehicle impoundment program, while the black curve represents the model fit. Bottom panel: Identical to top panel, but representing data in which drivers contributing to either the outcome or exposure measure had
committed a DWP offence within the previous five years.

For outcome 1, the DWP ratio, $R_{DWP}$, Poisson/negative binomial regression revealed that, before this countermeasure was introduced, the DWP ratio was actually increasing over time by 0.9% (95% Wald CI: [0.6%, 1.3%]) bi-monthly. The implementation of the Long Term Vehicle Impoundment was associated with a statistically significant reduction in this increase, essentially bringing it to an end (see top panel of Figure 18).

For outcome 2, the previous offender DWP ratio, a statistically significant decrease of 19% (95% Wald CI: [6.5%, 30%]) was seen across countermeasure implementation. Using model fit values immediately before and after implementation yields a corresponding reduction DWP rate from 5.76 to 4.64 offences per 100 prohibition-quarters (23.0 to 18.6 offences per 100 years of criminal driving prohibition) immediately after at the time of implementation.
Ministry of Transportation

Figure 18. Relative occurrence of driving while prohibited pre- and post- implementation of the long term impoundment.
Top panel. Black dots represent the driving while prohibited ratio, $R_{DWP}$, which is simply the values of the black dots in the top panel of figure 18 divided by the values of the gray dots. The vertical dashed line represents the long-term impoundment implementation date and the black curve is the fitted model.
Bottom panel. Identical, but for the previous offender equivalent outcome.

**Seven-day Impoundment**

The seven day impoundment countermeasure was not found to be associated with any general deterrent effect on collision-related outcomes.

Two measures of specific deterrence for the Seven-day Impoundment program were considered: 1) the bi-monthly proportion of drivers with a 90-day ADLS who are detected driving during that suspension, and 2) the proportion of those receiving a 90-day ADLS who do not have a disposition of their criminal charge in the three months after their 90-day ADLS and who subsequently receive a second 90-day ADLS in their follow-up window. Raw count data for outcome 1, is shown in the top panel of Figure 19.
Of particular interest, the number of drivers receiving a 90-day ADLS (gray dots) remains fairly constant over time, while the number of those drivers who drive while suspended (black dots) appears to drop at Seven-day Impoundment implementation. Raw count data for outcome 2 is shown in the bottom panel of Figure 19. Similar trends are evident, but somewhat less clear.
Figure 19. Driving while suspended and follow-up re-suspension events pre- and post- 90-day ADLS implementation

Top panel. Gray dots represent exposure: the bi-monthly number of offenders receiving a 90-day ADLS. The black dots represent the bi-monthly number of driving while suspended offences committed during the 90-day ADLS suspension depicted in gray. The vertical dashed line represents the implementation of the seven day vehicle impoundment program, while the black curve represents the model fit. Bottom
panel: Similar to top panel, but gray dots represent the bi-monthly number of driver receiving a 90-day ADLS who also remain un-convicted for at least 180 days, leaving a three-month follow-up window after suspension. The black dots represent subsequent 90-day ADLS suspensions received by the drivers just described during their follow-up window.

For outcome 1, the DWS ratio, $R_{DWS}$, Poisson/negative binomial regression revealed a significant decrease of 33% (95% Wald CI: [29%, 45%]) at the implementation of the Seven-day Impoundment (see top panel of Figure 20). This corresponds to a reduction in recidivism rate from 2.12 to 1.42 drivers committing a DWS per 100 drivers with a first 90-day ADLS.

For outcome 2, the recidivism ratio, $R_{recid}$, showed a statistically significant decrease of 29% (95% Wald CI: [4.6%, 47%]) (see bottom panel of Figure 20). This corresponds to a reduction from 0.98 drivers incurring a second 90-day suspension per 100 drivers who had not yet been (or never were) convicted of the original charge to 0.71 such drivers per 100.
Figure 20. Relative occurrence of driving while suspended and follow-up re-suspension pre- and post- 90-day ADLS implementation.

Top panel. Black dots represent the driving while suspended ratio, $R_{DWS}$, which is simply the values of the black dots in the top panel of figure 20 divided by the values of the gray dots. The vertical dashed line represents the seven-day impoundment implementation date and the black curve is the fitted model. Bottom panel. Identical, but for the recidivism ratio.

Zero BAC Requirements

As described above, collision-based outcomes did not generate any evidence for the effectiveness of Zero BAC requirements. However, offence-related outcomes appeared to tell a different story. First, we performed an interrupted time series analysis of the number of 90-day ADLS given to drivers under 22 relative to the number given to the older driving population. An ARIMA(0,1,1)(1,0,0) model with all batch two countermeasure intervention covariates was found to fit this time series with a $R^2$-squared value of 0.414 and an insignificant Ljung-Box Q statistic. Only the Zero BAC intervention covariate was found to produce a statistically significant decrease (~21%, $p = 0.012$) in the relative number of suspensions given to younger drivers. Second, we compared the proportion of Warn Range suspensions given to drivers under 22 in the year before and the year after Zero BAC implementation. The number of such suspensions given to the entire driving population was 15,778 in the year before and
14,175 in the year after implementation. The corresponding proportion of these suspensions given to young drivers was found to decrease significantly across implementation from 10% in the year before to 8.3% after ($\chi^2(1) = 27.5, p < 10^{-6}$). This corresponds to a 18% reduction in the proportion of Warn Range suspensions given to young drivers compared to what would have been expected given the pre-Zero BAC rate. Converting this shift in proportion to a shift in ratio (to make it more directly comparable to the 90-day suspension) yields a reduction of ~19%. Thus, similarly fewer 90-day ADLS and Warn Range Sanctions were given to young drivers (relative to other drivers) after the Zero BAC implementation.

With regard to specific deterrence, the effect of the Zero BAC requirement on two-year recidivism amongst previous offenders was also examined. Of all those with a first or only Zero BAC charge at disposition ($N = 2553$), the majority of them were convicted of their charges and therefore subject to the Zero BAC sanctions (80%). This group, exposed to the actual sanctions, recidivated at a rate of 2.8% within two-years of disposition of the charges. This is 48% lower ($p < 0.01$) than those not subject to post-conviction sanctions, of whom 5.1% recidivated within two years. Of those under 22 ($N = 1535$), the majority of them were convicted of their charges and therefore subject to the Zero BAC sanctions (89%). This group, exposed to sanctions, recidivated at a rate of 2.5% within two-years of disposition of the charges, which is 74% lower ($p < 0.0001$) than those not subject to the post-conviction sanctions, of whom 9.6% recidivated within two years.

**Alcohol Education and Treatment Remedial Measures Program**

We first assessed pre-existing trends in drinking and driving recidivism before BOT implementation. Figure 21 shows a time series of recidivism proportions calculated based on CCC s. 253 or 254 re-offences (charges that led to conviction) within a three-year follow-up window starting at the end of a driver’s CCC driving prohibition. For completeness, we show data to the end of 2001, but points beyond BOT implementation are produced from a mixture of offenders receiving the single-component, education-only program and offenders receiving the multi-component program. There appears to be a sudden drop in recidivism proportion after BOT implementation, but a proper interrupted time series analysis could not be performed due to the mixed nature of the data after implementation. However, Poisson regression performed on the pre-BOT period demonstrates a lack of trend in the four years leading
up to implementation ($e^\beta = 1.000; 95\% CI: [0.997, 1.003])$. For this regression, data from the two months immediately before implementation were excluded due to the possibility of an outlier in July 1998. These results suggest that any difference in recidivism calculated via pre-post comparisons is not due to pre-existing background trends.

![Figure 21](image)

**Figure 21.** Three-year recidivism proportion for drivers convicted of a CCC s. 253 or 254 offence over a seven-year period. The first dashed line represents the 90-day ADLS implementation date. The second dashed line represents the implementation of the single-component, education-only BOT program.

Turning to our study groups, of the total BOT study sample (No BOT $N = 19,163$, Edu BOT $N = 15,362$, Full BOT $N = 15,769$), approximately half of participants—63% for Edu BOT and 50% for Full BOT—completed the program within 24 months of their CCC driving prohibition start date. Completion time distributions for BOT participants are shown in **Figure 22**. Full BOT completers appeared to take longer to complete than Edu BOT completers, likely because of the additional requirements of Full BOT.
Our first set of analyses evaluated the effects of “BOT package” on recidivism. For this analysis, a driver’s three-year re-offence follow-up window began on that driver’s prescribed CCC prohibition end date, regardless of whether their licence had been re-instated at that time. Recall, a re-offence here means a charge that leads to convictions. The three year cumulative recidivism rate was significantly lower for Edu BOT versus No BOT drivers (6.7% versus 8.5%, $\chi^2(1) = 39.02, p < 10^{-6}$), and for Full BOT versus Edu BOT drivers (5.5% versus 6.7%, $\chi^2(1) = 19.00, p < 10^{-4}$), suggesting overall program effectiveness.

The second set of analyses evaluated the effects of BOT “components”, solely referring to the assessment, education, treatment, and follow-up components, on recidivism after licence reinstatement for drivers who completed the program. Specifically, the three-year cumulative recidivism rate was compared between the three BOT groups in a stepwise fashion, as above. These comparisons were made separately for on-time completers (completing BOT requirements within 12-months of driving prohibition start so that licence re-instatement was coincidental with CCC driving prohibition end) and
late completers (completing between 12 and 24 months after prohibition start). Both on-time completers and late completers in the Edu BOT group recidivated significantly less than those in the No BOT group, while on-time and late completers in the Full BOT group recidivated significantly less than those from the corresponding Edu BOT subgroups (see Table 3). Moreover, within the Edu BOT and Full BOT groups, the on-time completers re-offended significantly less than the late completers (all comparisons: Pearson chi-squared, $p < 0.05$ after Bonferroni correction for six comparisons).

<table>
<thead>
<tr>
<th>Comparison Groups</th>
<th>Total group N</th>
<th>Re-offenders N (% of total)</th>
<th>Difference in proportions</th>
</tr>
</thead>
<tbody>
<tr>
<td>No BOT</td>
<td>19163</td>
<td>1625 (8.5)</td>
<td></td>
</tr>
<tr>
<td>Edu BOT (on time)</td>
<td>4410</td>
<td>238 (5.4)</td>
<td>***</td>
</tr>
<tr>
<td>No BOT</td>
<td>19163</td>
<td>1625 (8.5)</td>
<td></td>
</tr>
<tr>
<td>Edu BOT (late)</td>
<td>5200</td>
<td>372 (7.2)</td>
<td>**</td>
</tr>
<tr>
<td>Edu BOT (on time)</td>
<td>4410</td>
<td>238 (5.4)</td>
<td></td>
</tr>
<tr>
<td>Full BOT (on time)</td>
<td>2738</td>
<td>106 (3.9)</td>
<td>**</td>
</tr>
<tr>
<td>Edu BOT (late)</td>
<td>5200</td>
<td>372 (7.2)</td>
<td></td>
</tr>
<tr>
<td>Full BOT (late)</td>
<td>5132</td>
<td>300 (5.8)</td>
<td>**</td>
</tr>
</tbody>
</table>

$X^2$ test: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$, ns – not significant

Demographic differences between on-time and late completers may partially or fully account for differences in recidivism between those groups. In other words, on-time completers may have produced less recidivism due to inherent characteristics rather than due to the BOT program being more effective at reducing recidivism when received in a timelier manner. On average, on-time completers were older than late completers and were convicted sooner. In addition, there were a higher proportion of on-time completers who held a commercial licence in both the Edu BOT and the Full BOT groups. After propensity score matching, the differences in recidivism proportion for the on-time versus late completers disappeared within the Edu BOT comparison and the Full BOT comparison (see Table 4).
Table 4. Comparisons of matched BOT recidivism data for completers

<table>
<thead>
<tr>
<th>Comparison Groups</th>
<th>Total group N</th>
<th>Re-offenders N (% of total)</th>
<th>Difference in proportions</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Before matching</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Edu BOT (on time)</td>
<td>4410</td>
<td>238 (5.4)</td>
<td></td>
</tr>
<tr>
<td>Edu BOT (late)</td>
<td>5200</td>
<td>372 (7.2)</td>
<td>***</td>
</tr>
<tr>
<td>Full BOT (on time)</td>
<td>2738</td>
<td>106 (3.9)</td>
<td></td>
</tr>
<tr>
<td>Full BOT (late)</td>
<td>5132</td>
<td>300 (5.8)</td>
<td>**</td>
</tr>
<tr>
<td><strong>After matching</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Edu BOT (on time)</td>
<td>4410</td>
<td>225 (5.7)</td>
<td></td>
</tr>
<tr>
<td>Edu BOT (late)</td>
<td>5200</td>
<td>198 (5.0)</td>
<td>ns</td>
</tr>
<tr>
<td>Full BOT (on time)</td>
<td>2738</td>
<td>104 (4.0)</td>
<td></td>
</tr>
<tr>
<td>Full BOT (late)</td>
<td>5132</td>
<td>125 (4.8)</td>
<td>ns</td>
</tr>
</tbody>
</table>

Χ² test: * p < 0.05, ** p < 0.01, ***p < 0.001, ns – not significant

In order to investigate any differential relationship between the BOT program and recidivism as a function of age, we performed two age-stratified comparisons of recidivism between the Edu BOT and No BOT groups. First, we compared Edu BOT on-time completers to No BOT drivers, with the follow-up window beginning at each driver’s licence re-instatement/CCC prohibition end date (top panel of Figure 23). Second, we compared all drivers in the Edu BOT group, regardless of program completion, to the No BOT drivers, with the three year follow-up window beginning at each driver’s CCC prohibition end date (bottom panel of Figure 23). Our results show that of on-time completers, younger drivers from the Edu BOT group were less likely to recidivate compared to younger drivers from the No BOT group. However, for older drivers this difference disappears. A similar pattern holds for Edu BOT versus No BOT drivers when education/treatment completion is not considered.
Figure 23. Age stratified analysis comparing three year recidivism proportions for Edu BOT versus No BOT drivers.
Top panel: Comparison restricted to Edu BOT drivers who completed remedial education before the end of their CCC driving prohibition. The three year re-offence window begins at the end date of each driver’s CCC driving prohibition end date. Bottom panel: Unrestricted comparison including all Edu BOT drivers regardless of completion status. The three year re-offence window also begins at the end date of each driver’s CCC driving prohibition. (**p < 0.001).

**Ignition Interlock Program**

Of all drivers (analyses during interlock condition: $N = 26,851$, analyses after interlock condition: $N = 25,226$), the vast majority (analyses during interlock condition: 73%, analyses after interlock condition: 78%) never installed an interlock device despite having the condition on their licence. Before matching, the Never Installed group was younger, had a lower proportion of males, had a higher proportion of drivers who have committed a “novice offence”, and lower proportion of drivers with “moving violations” (see Appendix 1). These differences are presented in Table 5. It is conceivable that installers typically need to drive more than never installers, possibly accounting for the higher number of previous moving violations in this group. Matching eliminated all significant differences between groups constructed for comparison of recidivism during interlock install; however, when creating groups for comparing post-interlock condition recidivism, we were unable to remove residual differences between installers and non-installers in terms of age and in the proportion of drivers who had committed a “breach of condition” previous offence. As such, for post-interlock comparisons, these variables were retained in Cox/Logistic regression as adjustment factors.

Table 5. Comparisons of demographic information for installers versus non-installers

<table>
<thead>
<tr>
<th>Characteristic</th>
<th>Installers</th>
<th>Never installed</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>37.2 yrs</td>
<td>36.8 yrs*</td>
</tr>
<tr>
<td>Male gender</td>
<td>88%</td>
<td>83%***</td>
</tr>
<tr>
<td>Has previous offence</td>
<td>(%)</td>
<td>(%)</td>
</tr>
<tr>
<td>Breach of condition</td>
<td>5.0</td>
<td>5.4</td>
</tr>
<tr>
<td>Novice-related</td>
<td>0.7</td>
<td>1.0**</td>
</tr>
<tr>
<td>Conduct-related</td>
<td>2.8</td>
<td>3.4</td>
</tr>
<tr>
<td>Moving violation</td>
<td>55</td>
<td>49***</td>
</tr>
<tr>
<td>Disobey signals, etc</td>
<td>13</td>
<td>13</td>
</tr>
<tr>
<td>Criminal activity</td>
<td>1.4</td>
<td>1.8</td>
</tr>
</tbody>
</table>

t-test/$\chi^2$ test: * $p < 0.05$, ** $p < 0.01$, ***$p < 0.001$
Comparing recidivism rates for installers during interlock install time to recidivism rates for non-installers during interlock condition time revealed that non-installers recidivated at 2.42 (95% Wald CI: [1.29, 4.50]) times the rate of those with a device installed (see Figure 24). After matching, this larger rate remained little changed, at 2.49 (95% CI: [1.11, 5.45]) times as many re-offences. Re-offences during interlock device install numbered 1.74 per 1000 install-years before matching and 1.35 per 1000 install-years after. For those who never installed, re-offences numbered 4.21 per 1000 condition-years before matching and 3.31 per 1000 condition-years after matching.

Examine collision involvement during the interlock install in comparison to collision involvement during an interlock condition for those who never installed reveals that the former group collides at 3.60 (95% CI: [3.10, 4.23]) times the rate of the former. In particular, those with an installed device had HTA-reportable collisions at a rate of 47 per 1000 install-years, whereas those who never installed were involved in collisions at a rate of 13 per 1000 condition-years. This suggests that the lower level of D&D recidivism shown by those with an installed device occurs in a background of more driving. For comparison, the general population collision rate is 44 per 1000 person-years ("Ontario Road Safety Annual Report" 2006;2010).
Our initial attempt to analyze post-interlock condition recidivism involved performing a Cox regression-based survival analysis to compare survival distributions between those who never installed a device during their condition and those who installed for at least 300 days (~80% of their condition). After matching, Cox’s method (1972) for detecting violations of proportionality reveal a lack of proportionality between hazard functions for the two groups of interest. As such, we switched to a logistic regression approach where recidivism for each driver was captured in a three year follow-up window starting at the end of their interlock condition. Performing this regression on the matched comparison groups (installers vs. never installed), with age, gender and offence history variables included as covariates to account for any between group differences not eliminated by matching, revealed no significant difference in recidivism outcomes. Approximately 6.8% of both installers and non-installers re-offended with three year of condition removal.

**Reduced Suspension with Ignition Interlock Conduct Review Program**

The number of persons receiving an interlock condition increased dramatically but briefly in the period immediately after the Conduct Review Program was implemented (see Figure 25). This suggests some offenders may have been delaying disposition of criminal charges in order to be able to enter the program. Indeed, between program implementation and November 25, 2014, 6,794 drivers have completed Stream A and have had their driving licences re-instated. For Stream B, this number is 3,528, while for Stream C only 609 drivers installed an interlock for between nine months and one year of their one-year interlock condition and have had their licence re-instated. In contrast, there were 2,651 Conduct Review-eligible drivers who sat-out during this time (Stream C by default), but who subsequently had their licence re-instated.
The proportion of those receiving an interlock condition for a first CCC s. 253 or 254 offence who subsequently install a device is shown in Figure 26. Of note, before January 1, 2005 few drivers installed devices according to the data obtained from service providers. For those who are convicted of a CCC s. 253 or 254 offence after that date, and who subsequently receive an interlock condition, the install proportion hovers at around 40%. However, the install proportion takes a large dip for those convicted in 2007, likely because these people would receive their condition just over a year later at the height of the economic recession of 2008. The install proportion then recovers to a plateau above 40%, before taking a jump at the time of Conduct Review implementation. More specifically, modeling of the recovery phase, performed with the nonlinear model described in the Methods section, reveals a 54% (95% CI: [47%, 61%]) increase in installation proportion, from approximately 45% to approximately 70%.
Figure 26. Ignition Interlock uptake pre- and post- reduced suspension with conduct review implementation

Black dots represent the bi-monthly proportion of driver’s who are convicted of a CCC s. 253 or 254 offence, and who subsequently receive an interlock condition and install a device within six months after condition start. The vertical dashed line represents Conduct Review Program implementation. The two hollow data points near implementation were not used for model fitting because, as can be seen from figure 25, these points are based on an unusually large number of interlock conditions and appear to be outliers. The black curve represents the fitting model.

The average time between committing a CCC s. 253 or 254 offence and having the charges disposed of by way of conviction (for those drivers who were convicted) is shown in Figure 27. Linear regression, including seasonal factors, reveals a significant decrease in offence-to-disposition time period of 146 (95% CI: [129, 164]) days across program implementation. This decrease is from a pre-implementation year-long average of 287 days to post-implementation average of 158 days\(^\text{10}\).

\(^{10}\) The fitted model indicates an “instantaneous” decrease of 146 days at implementation; however, the yearly pre-post averaging, which is more susceptible to the presence of background time trends, indicates a decrease of 287 - 158 = 129 days.
Black dots represent the bi-monthly average offence-to-disposition time period for CCC s. 253 or 254 convictions. The vertical dashed line represents Conduct Review implementation, while the black curve is the fitted model.

Of currently re-instated drivers from Stream A (or B), and selected so as to have had time to receive and complete up to two performance failure-based interlock extensions, 11% (8.2% for Stream B) received one extension, while 2.3% (2.4% for Stream B) received two extensions. In order to examine the effectiveness of the performance failure-based interlock extensions that occur in Stream A and B, we compared survival time to subsequent recidivism with those Stream C drivers who sat-out. After matching such Stream C drivers with Stream A (or B) drivers on offence history, age, gender, and neighborhood demographic characteristics (see Appendix 2), there were two few Stream C outcome re-offences to produce a meaningful comparison. In fact Stream C drivers who sat-out (but who were eligible for Conduct Review) produce only 56 Warn Range/90-day ADLS outcomes before (or after) matching. More time must go by before the long term effects of the Conduct Review program can be evaluated.
DISCUSSION

OVERVIEW

In this section, we will summarize the key findings for each drinking and driving countermeasure and discuss how this research can be related to the existing literature base. We will also reflect on the contribution of the countermeasures as a package to Ontario’s efforts to deter drinking and driving and the accompanying policy implications.

The key strength of this research is the way that it has simultaneously assessed all the countermeasures currently in place to combat drinking and driving on Ontario roads. To properly assess each measure a variety of methodologies were used, each one designed to isolate the effects of a particular countermeasure from other concurrent and/or overlapping programs. Together, these analyses provide an exhaustive assessment of the strengths and weaknesses of each measure. Although much research has been done on individual drinking and driving countermeasures across the globe, this assessment of numerous programs in effect in a single jurisdiction is the first of its kind and can provide a comprehensive contextual framework for future research.

90-day Administrative Driver’s Licence Suspension

Ontario’s 90-day ADLS, implemented in November 1996 for BAC 0.08% or above or refusal to provide a breath sample, provides immediate and certain countermeasures for a group of drivers who would previously not have been penalized until the time of disposition of the related charges if a conviction was entered. We first evaluated this countermeasure for its effect on alcohol-related collisions in the general population (i.e., it was evaluated for its general deterrence capacity). Two techniques were used: an interrupted time series approach and a predictive/forecasting approach. Both methods revealed a reduction in the number of fatalities and major injuries resulting from alcohol-related collisions relative to the corresponding number of fatalities and major injuries from other collision types at the time of implementation. This relative measure is important because it accounts for numerous other factors that could have caused a change in collision-related outcomes in general at the same time as 90-day ADLS implementation. Given the agreement between the two time series modelling approaches, we are highly confident that the 90-day ADLS reduces major injuries and fatalities.
The forecasting model predicted that without ADLS, there should have been 90.4 major injuries and fatalities resulting from alcohol-related collisions per month. Following the introduction of 90-day ADLS the actual rate was found to be 21% lower, at 71.6 per month. A cumulative estimate for the period from November 1996 to November 2015 therefore suggests that 4297 major injuries including fatalities might have been prevented due to the introduction of the 90-day ADLS. Of course, numerous other factors have changed since 1996, so this long-term estimate is only accurate to the extent that the 90-day ADLS has maintained exactly the same effectiveness over the forecasted time that it initially demonstrated in the first five years post-implementation.

It is possible that some unaccounted-for external factor produced the effects we are attributing to the 90-day ADLS countermeasure. Since a relative measure (D&D outcomes relative to equivalent non-D&D outcomes) was employed in our time series analysis, any such external factor would have to have influenced alcohol-related collisions differently than it influenced collisions in general, and at the same time that the 90-day ADLS countermeasure was implemented. Only three such possible factors were identified: drinking and driving public education and awareness campaigns, changes in the level of enforcement of impaired driving policies generally, and changes in alcohol consumption.

Given the time that has passed between 90-day ADLS implementation and the present, reliable information on media and enforcement campaigns was difficult to locate. However, alcohol consumption data, insofar as per capita alcohol sales volume from Ontario’s LCBO can be used to represent per capita alcohol consumption, was used as an input to our time series models. The inclusion of this data was found to make no difference to the observed results. However, it should be noted that alcohol sales in Ontario do occur through outlets other than the LCBO, including The Beer Store, a province-wide retailer that focuses exclusively on beer sales.

The findings presented here revise and extend previous fatality-only estimates of 90-day ADLS effectiveness by Asbridge et al. (2009), who observed a 14% drop compared to two control provinces (Manitoba and New Brunswick) and by Mann et al. (2002) who noted a 17% drop in fatalities when examining a shorter post-intervention timespan. We argue that the inclusion of major injuries in this analysis allows for a more complete picture of the total impact of alcohol-related collisions, and makes statistical analysis more robust.
In a second evaluation, the effect of the 90-day ADLS on drinking and driving recidivism was investigated, also using an interrupted time series approach. The focus was on drivers who committed a subsequent offence within 90 days of the first, and before they were actually convicted. Drivers who committed a CCC s. 253 or 254 offence from 1991 to 2001 were examined to determine whether their propensity to produce subsequent CCC s. 253 or 254 charges (that eventually led to conviction) --before conviction for their original offence-- changed across implementation of the 90-day ADLS. We found that drinking and driving recidivism occurring within the 90-days immediately after a CCC s. 253 or 254 offence decreased by 66% after implementation, from 2.45 to 0.82 re-offending drivers per 100 offending drivers. This is likely a direct result of the suspension itself, given that before 90-day ADLS implementation, such drivers only received a 12 hour suspension. Unfortunately, this also means that some drivers continue to drive while under a 90-day suspension.

Enduring effects of the 90-day ADLS on recidivism after the 90-day ADLS itself but before disposition of criminal charges were not observable in our analysis. The reason for this might be that the 90-day ADLS produces no effect in this regard. Equally likely, however, the lack of such an effect might involve reduced statistical power resulting from the relatively short time that exists for most drivers between the end of a 90-day ADLS and subsequent disposition of the CCC s. 253 or 254 charge.

In summary, the 90-day ADLS is clearly an effective countermeasure that protects the drivers of Ontario.
Warn Range Sanctions

Ontario’s Warn Range Sanctions, which took effect province-wide in May 2009, provide immediate and certain countermeasures (e.g., licence suspensions) for previously un-targeted drivers with BACs above 0.05% but below the criminal threshold. As with the 90-day ADLS, the Warn Range Sanction was first evaluated for its ability to deter drinking and driving in the general population. Performing interrupted time series analysis of collision data provided strong evidence that the number of drinking drivers involved in collisions, as well as the number of injuries and fatalities due to alcohol-related collisions decreased significantly at implementation relative to equivalent non-alcohol-related collisions measures.

Our time series models suggest that the number of drinking drivers involved in collisions decreased abruptly by 15% at the time of Warn Range Sanction introduction. This translates into 27.3 fewer drinking drivers involved in collisions per month in the post-implementation period, amounting to a total of 1966 fewer drinking drivers involved in collisions between May 2009 and May 2015. Similarly, the number of injuries and fatalities from alcohol-related collisions was 17% lower than what would have been expected based on pre-existing background trends and changes in non-alcohol-related collisions rates. This translates into 44.9 fewer drinking drivers involved in collisions per month in the post-implementation period, amounting to a total of 3233 fewer injuries and fatalities due to alcohol-related collisions between May 2009 and May 2015.

As with the 90-day ADLS evaluation, it is possible that some unaccounted-for external factor produced the effects we are attributing to the Warn Range countermeasure. Again, since a relative measure was employed in our time series analysis, any such external factor would have to have influenced alcohol-related collisions differently than it influenced collisions in general, and at the same time that the Warn Range countermeasure was implemented. As with the 90-day ADLS evaluation, alcohol sales data from the LCBO was used in our models to account for any effect of changing consumption on D&D outcomes. The inclusion of this data was found to make no difference to the results observed.

No countermeasure can be effective as a general deterrent if the driving population is unaware of it. As such, a public education and awareness campaign was initiated at around the time of Warn Range countermeasure implementation to advertise it. There are at least two ways a campaign could affect drinking and driving and the associated collisions. First, as intended, it could make the driving population aware of the
countermeasure advertised, or second, it could deter drinking and driving directly (e.g., by simply reminding people about the dangers of drinking and driving in general). The campaign associated with Warn Range implementation is estimated to have lasted approximately six months and no more than one year (J. LeFebvre, personal communication). The Warn Range effects last for at least two and a half years, until the end of 2011. In 2012, the effect appears smaller, but more data would be required to determine if this is natural variation or an enduring attenuation of effectiveness. In any case, even if the Warn Range associated effects lasted only two and a half years, it is unlikely that this is attributable solely to the much shorter media campaign.

Clearly, enforcement, or the perception of enforcement is necessary for countermeasures to have any effect. However, increasing enforcement of already existing drinking and driving policies could make a contemporaneously introduced countermeasure look more effective than it actually is. The pattern of results reported in this study is not consistent with increased enforcement being the sole cause of the Warn Range associated decreases in alcohol-related collisions outcomes observed. Specifically, we found that injuries and fatalities resulting from alcohol-related collisions dropped in association with Warn Range implementation by 17% relative to those injuries and fatalities produced by other collisions. When analysis was restricted to major injuries and fatalities, we found only a 7.7% to 12% decrease in association with the Warn Range countermeasure. To the extent that collision severity is correlated with BAC level of the driver(s) involved, the Warn Range countermeasure should have its greatest effects on less severe collisions, which is what was observed here.

Additional analyses attempted to address (i) whether drivers who have been subject to Warn Range Sanctions multiple times demonstrate a decreasing rate in re-occurrence, i.e., whether they “slow down”; and (ii) whether drivers who have received two such sanctions are detected drinking and driving less than equivalent drivers who have only received the first sanction. Results were inconclusive on both of these questions. For question (i), it appears that “low rate” suspension recipients experienced greater elapsed time between their second and third incursion compared to their first and second. This suggests that they are “slowing down” as a result of being subject to the Warn Range Sanctions; in other words, this countermeasure had some deterrent effect with respect to repeated drinking and driving. However, this “slowing” pattern is confounded by the decreasing rate of first Warn Range Sanction generation in the population, which could be due to numerous changing external factors that might or might not also affect recurrence rates of individual drivers. With sufficiently more data, it
should be possible to correct for this decreasing background trend and discover whether the “slowing” recurrence trend is due to the repeated Warn Range Sanctioning.

Of note is that “high rate” warn range drinking drivers were not affected by this decreasing background trend. One plausible reason is that they have a lower exposure to the trend, given that all three of their incidents occurred within two years and three months rather than five years. Another plausible reason is that this group differed from the “low rate” group in that they may be a more clinical population undergoing an acute crisis and thus, unaffected by external factors. Regardless, no significant effects are seen in the “high rate” group and significant effects observed in the “low rate” group are confounded by external factors that cannot be corrected for at this point due to a lack of data.

Returning to question (ii), whether drivers who have received two Warn Range Sanctions re-committed this behavior less than equivalent drivers who have only received the first sanction, it was determined that we did not have access to sufficiently detailed personal data to create equivalent groups. Those who had received two sanctions appear to have been inherently higher rate drinking drivers.

In sum, the Warn Range countermeasure demonstrated a clear beneficial effect on alcohol-related collision outcomes in the general population. However, we were unable to successfully evaluate the effect of escalating sanctions on a driver’s propensity for recidivism at this time.

**Long Term Vehicle Impoundment**

The Long Term Impoundment program, implemented in February 1999, aims to reduce recidivism by increasing compliance with CCC driving prohibitions. Effectiveness of this countermeasure was investigated by examining drivers who were prohibited due to a CCC s. 253 or 254 conviction at any time between January 1995 and January 2009 for an offence committed on or after January 1994. Interrupted time series analysis was performed on the number of driving while prohibited (DWP) offences committed by such drivers relative to the number of such drivers. An analysis of all prohibited drivers (CCC s. 253 or 254) showed that, prior to the Long Term Vehicle Impoundment countermeasure, the relative number of DWP offences was actually increasing over time by approximately 0.9% bimonthly. Countermeasure implementation was associated with an abrupt end to this increasing trend.
When analysis was further restricted to CCC prohibited drivers with at least one DWP offence in the previous 5 years, Long Term Vehicle Impoundment implementation was associated with a sudden decrease in the relative number of DWP offences committed during CCC s. 253 and 254 prohibitions of 19%. This corresponds to a reduction from 23.0 to 18.6 DWP offences per 100 years of criminal driving prohibition.

**Seven-day Vehicle Impoundment**

The Seven-day Vehicle Impoundment program, implemented in December 2010, is an immediate and certain countermeasure meant to reduce drinking and driving recidivism immediately after an offence. This countermeasure is applied on top of the already implemented 90-day ADLS, and for the same incidents offences. As such, it can be viewed as an increase in the severity of an already existing sanction.

We first evaluated whether the Seven-Day Vehicle Impoundment increased compliance with the 90-day ADLS. An interrupted time series analysis was performed on an outcome measure consisting of the number of drivers detected driving during a 90-day ADLS relative to the number of drivers with a 90-day ADLS. A 33% reduction in this measure was found at the time of countermeasure implementation, corresponding to a reduction from 2.12 to 1.42 drivers committing a DWS per 100 drivers with a first 90-day suspension.

Next, the effect of the Seven-day Impoundment on post 90-day ADLS drinking and driving recidivism was investigated by restricting the sample of drivers to those with a full three-month follow-up window after their 90-day ADLS, i.e., a three month period during which they were not convicted under CCC s. 253 or 254 for the incident that generated the original 90-day ADLS. Time series analysis showed that the proportion of such drivers receiving a second 90-day ADLS in their follow-up window was reduced by 29% across implementation, from 0.98 drivers receiving a second 90-day ADLS in their three month follow-up window per 100 drivers who had such a window to 0.71 such drivers.

While the number of those who drive during a 90-day ADLS received for drinking and driving, or who recidivate immediately after, is relatively small in relation to the total Ontario driving population, it is likely that the deterrent effects observed extend to the many drivers who might drive while suspended, or who might quickly engage in drinking and driving recidivism, but who are never detected. Some estimates place the number of drivers who drive while suspended as high as 75% (e.g. Lenton et al. 2010), although
such drivers report driving less often and more carefully (e.g. Clark and Bobveski 2008). Moreover, the types of drivers who exhibit these repeat drinking and driving behaviours, or who attempt to continue driving on their suspended licence might represent a particularly dangerous group.

Given that the Seven-Day Impoundment could discourage drivers in the general population from drinking and driving, we also evaluated its effectiveness on reducing negative alcohol-related collisions outcomes. This was done in conjunction with the Warn Range evaluation. No general deterrent effects corresponding to Seven-Day Impoundments were found.

**Zero BAC Requirements**

Zero BAC Requirements were intended to both reduce alcohol-related collision outcomes in the young/novice-classed driver population, and to quickly teach a young/novice-classed driver who drinks and drives that they cannot expect to do it without consequences.

The new Zero BAC Requirements, introduced in August 2010, were evaluated for their general deterrent capacity using collision related outcomes. No effects were found, but this might simply be because the effect size for this sub-population was too small to observe in the full population data.

As an alternative to collision outcome, we also looked for general deterrent effects by examining the rate of alcohol-related countermeasures applied to young drivers. In particular, we looked at how the number of non-Zero BAC alcohol-related countermeasures given to young drivers relative to the number given to the non-young population changed across Zero BAC implementation. A significant decrease in the relative number of 90-day ADLS and Warn Range Sanctions given to young drivers (below 22) was observed in association with Zero BAC implementation. Specifically, the proportion of Warn Range Sanctions given to young drivers was reduced by 18% (a ~19% decrease if expressed as a ratio instead of proportion), while a reassuringly similar 21% reduction in the ratio of 90-day ADLS given to young drivers (relative to non-young drivers) was observed. However, the magnitude of these reductions must be interpreted with care, as the estimated reduction likely occurred from an unknowable mix of behavioural change on the part of young drivers (i.e., reduced drinking and driving, indicative of a true intervention effect) and other unknown factors. It is important to note that the Warn Range and 90-day ADLS countermeasures are given for very
different reasons and have very different implications for a young driver. Since we observed similar reductions in both countermeasure types for young drivers at Zero BAC implementation, the most parsimonious explanation appears to be that young drivers engaged in less drinking and driving.

The effect of Zero BAC countermeasures on subsequent drinking and driving in the warn range or above the legal limit were investigated by comparing young/novice-classed drivers charged with a first Zero BAC violation but who were not convicted of that offence (unexposed group) to those who were charged and convicted (exposed group). Ideally, the only difference between these two groups would have been that the latter received Zero BAC countermeasures while the former did not. However, both groups differed substantially in offence history and in propensity to commit other, non-drinking and driving offences. Severe constraints had to be placed on the exposed and unexposed groups in order to make them comparable, reducing sample size by approximately two-thirds. These constraints also prevented the use of a survival analysis.

After restriction, those subject to the Zero BAC countermeasure, as a result of a conviction for driving with detectable alcohol levels, recidivated 1.8 times less often than the unexposed group in the two years after court disposition. This pattern also held after further restricting the study population to those under 22; however, to a much larger extent (3.8 times fewer re-offences by those receiving the Zero BAC countermeasure). Although these results demonstrate the specific-deterrent effectiveness of the Zero BAC countermeasures, they must be interpreted with care for three reasons. First, restrictions placed on the study population eliminated much of the overall sample; cognizant that analyses were conducted on this restricted population, our results may not be generalizable to the overall Ontario young and novice-classed population. Second, when logistic regression is used on such a small sample, an overestimation of effect can occur. Third, attempts to treat selection bias should have made groups more equivalent; however, whether all meaningful differences were successfully removed could not be verified. This limitation is particularly relevant due to the fact that our exposed and unexposed groups were very different on non-alcohol-related offence patterns. There are undoubtedly systematic differences between the convicted and unconvicted groups in a number of demographic and driving behavioural factors that could have affected conviction probability, including access to lawyers, etc. Even with these limitations, the suggestion of effectiveness found here is consistent with the existing evidence base.
Alcohol Education and Treatment Remedial Measures Program

Previous research on the “Back on Track” program has examined changes in non-driving related participant outcomes (Wickens et al., 2013; Stoduto et al., 2014). Our study extends that research with the finding that the program as a whole, including the indefinite suspension for late- or non-completers, was associated with decreased drinking and driving recidivism that could not be explained by pre-existing temporal recidivism trends. Those who were subject to the single-component program (including those whose licence suspensions were extended for any given length of time) recidivated less within three years than those not subject to the program. Moreover, larger effects were observed for the more involved program, even when analyses were performed separately for the late-completer group and the on-time completer group (i.e., comparing late completers in the multiple-component program to late completers in the single-component program and likewise for on-time completers). However, the full (current) program also takes longer to complete relative to the initial education-only version, meaning that it might leave more drivers suspended longer and completers might be inherently more “diligent” than non-completers. We were unable to determine if those who completed the more involved program appeared to do better for these reasons alone.

In terms of performance of late versus on-time completers, propensity score matching was found to remove the difference in recidivism between these groups. Thus, there is clear evidence that individual characteristics of those drivers required to complete remedial measures are associated with the propensity to recidivate (as well as the propensity to complete the program late). Regardless of what these specific characteristics are, “late completion” of the program in its current form appears to be a risk factor for future recidivism.

Given that the whole BOT package appears to be an effective countermeasure, it would be desirable to maximize its effect. Interestingly, better outcomes, as indicated by lower recidivism for BOT completers relative to those who did not have an opportunity to participate, were associated with younger age (< 45 years old). This finding holds whether BOT is viewed as a whole package, including indefinite suspension for non-completers, or just with respect to the education/treatment components. Other demographic factors to which we did not have access might also produce differential outcome patterns. As such, a stronger consideration of demographic targeting is warranted. In addition to age-specific targeting, removing obstacles to timely program
completion would be beneficial for program participants, allowing those who would eventually complete the program to resume legal driving as soon as possible via participation in the Ignition Interlock Program.

With regard to collision outcomes in the general population, the interrupted time series analysis conducted in conjunction with the 90-day ADLS analysis found that the BOT program had no observed effects on the general population. Remedial type countermeasures only appear to exert effects on those who participate in the associated programs. This is unsurprising since the BOT program targets convicted impaired drivers rather than the general public, so the result reflects the original intention of the program.

**Ignition Interlock Program**

In examining drinking and driving behaviour occurring during the interlock condition amongst those convicted of criminal drinking and driving, a lower incidence of recidivism was observed only during the period of interlock installation. Specifically, the recidivism rate for non-installers was 2.49 times higher (per unit of interlock condition time) than for installers (per unit time installed). Viewed another way, interlocks reduced D&D recidivism by 60% during install, which is consistent with Willis, Lybrand, and Bellamy’s (2004) meta-analysis of U.S. programs, suggesting interlocks there produce a 67% reduction during install. This is the case even though our data show that collision rates for installers during install were similar to the general Ontario driving population, while those who did not install collided significantly less often during their condition. As such, installers appeared to drive more, but drink and drive less than non-installers. However, once the device was removed, rates of recidivism returned to the same levels as amongst drivers who were convicted but never installed the device (effectively remaining suspended for the duration of their condition).

From these findings, we can infer that the key mechanism responsible for preventing drinking and driving amongst installers is the physical obstacle provided by the interlock device itself. In instances where drivers with an interlock condition and an installed interlock were detected drinking and driving, it is possible that these occurrences took place while the individual was driving another vehicle without the device. Lack of an enduring effect after interlock removal is unsurprising in light of the majority of published literature (e.g., Beirness and Marques 2004; Willis, Lybrand, and Bellamy 2004).
Reduced Suspension with Conduct Review Program

The Reduced Suspension with Conduct Review Program was designed to address some of the observed weaknesses of the original Ignition Interlock Program. We found clear evidence that this program has resulted in two positive outcomes: the average time period between a incident leading to a CCC s. 253 or 254 charge and a subsequent conviction for that charge (for those convicted) decreased by 146 days after implementation, and installation of the ignition interlock device by those receiving an interlock condition has increased by 54% (from 45% to 70%).

The first outcome meets the goals of the program to improve the efficiency of a charged drivers’ passage through the judicial system. Moreover, it reduces the time drivers with a propensity to drink and drive present a risk to themselves and other road users before conviction. The second outcome provides a major increase in the number of drivers using interlock, which has been demonstrated to reduce recidivism while installed. Other jurisdictions have attempted to increase the number of installs, many of whom do so through making interlock laws mandatory (To repeat, in Ontario drivers are allowed to “sit out” the period when they have an interlock condition on their licence). However, the introduction of the reduced suspension component to incentivize interlock installation uptake, as demonstrated in Ontario, seems to have resulted in higher installation rates than laws (i.e., in New York state, in Illinois) making installation mandatory (Robertson et al. 2010).

Two specific implications flow from this. First, attaining an increased rate of installation uptake is possible through program design, and without the need to make it mandatory. Second, consideration of an optimal duration for the ignition interlock condition duration should err on the side of requiring longer lengths, so as to maximize the period during which the device is installed and effective.

Increased interlock installation results in an increase in the number of drivers on roads who would otherwise have been suspended and unable to drive legally in the absence of the Reduced Suspension with Conduct Review Program. Moreover, these drivers might be considered higher risk. Thus, decreasing the incidence of drinking and driving might be associated with increasing overall collision risk on Ontario roads (Elder et al. 2011). However, relative to the total driving population, the number of drivers with an interlock condition is small. More importantly, as discussed above, we found that people who installed interlocks do not collide with significantly greater frequency than the general population. This indicates that, so long as a driver convicted of drinking and
driving is in a vehicle with an interlock, there is no justification for treating them differently than any other driver.

We were unable to observe a long term benefit to the conduct review aspect of the program (i.e., reductions in recidivism after device removal for those drivers incurring performance failure extensions), likely because not enough time has yet elapsed since program inception to properly execute such an analysis. As such, we make no claims about the effectiveness of the conduct review aspect at this time.

Conclusions and Implications
Over the last two decades, Ontario has introduced eight countermeasures, all with the same goal of reducing the burden of injury and fatality resulting from alcohol-related collisions through deterring drinking and driving behaviour. Although implemented in two “batches” approximately 10 years apart, these countermeasures are individually distinct in that they target a range of subgroups and circumstances, leverage both general and specific deterrent mechanisms, and take varying forms (i.e., actions on the licence, actions on the vehicle, and actions on the driver). By studying these numerous and varying approaches to deterrence, which have all been implemented in the same jurisdiction, two main themes have emerged, along with additional specific findings.

First, of drivers involved in fatal or injury collisions in 2012, and who were recorded in the Accident Data System as “ability impaired alcohol” or “ability impaired alcohol (over 0.08%)” at the time of collision, approximately 89% had no criminal alcohol-related driving convictions in the previous ten years. Moreover, 87% of such drivers had no roadside 90-day ADLS sanctions in that period. In order to produce maximum road safety gains, drinking and driving countermeasures should be designed so as not to depend on previous drinking and driving detection or criminal conviction for their effectiveness. Instead, countermeasures should produce general deterrence through their immediacy and perceived certainty. Our results demonstrate that new initiatives should target novel subgroups and/or circumstances with immediate and certain sanctions, or focus on increasing the immediacy and certainty of existing countermeasures. In order for a countermeasure to produce general deterrence, the driving population must be aware of it and they must believe that they are likely to be detected (e.g. Bertelli & Richardson, 2008). Increasing the level of resources dedicated to public awareness of new and existing administrative sanctions, and to high-visibility enforcement campaigns would, arguably, be the most beneficial approach to reducing alcohol-impaired driving in Ontario.
In this evaluation, we found that licence suspensions were effective at producing general deterrence when delivered administratively at roadside. However, we also note that suspensions provide drivers with an opportunity to drive while suspended and therefore these drivers could become less observable within the legal driving system governed by MTO.

Second, when the remedial education/treatment requirement was added for those convicted of a CCC s. 253 or 254 offence, no effect was found in full population collision data. This lack of general deterrent effect was also noted when the ignition interlock program and seven-day impoundment were introduced. All three countermeasures constitute an extra burden on the subjected driver. In the case of remedial education/treatment, the time and financial commitments necessary to complete the program are incurred in addition to the original CCC driving prohibition. In the case of the interlock program, drivers face either the cost of installation and maintenance of the device, or an additional effective suspension, either of which are an addition to the pre-existing CCC driving prohibition. Seven-day impoundments were added on top of the pre-existing 90-day ADLS. All of these changes can therefore be interpreted as an increase in sanction severity associated with being detected for drinking and driving, even though this was not the actual intention for remedial education/treatment or ignition interlock programs. Our results should not be taken to suggest that increases in penalty severity have no effect on population-based outcomes, but they do suggest that such effects are small, if they exist. Thus, even though increases in penalty severity appear to reduce recidivism (e.g. seven-day impoundments), they will not likely impact road safety to the extent that increasing the public awareness and enforcement of currently existing administrative penalties would.

Turning to remedial-type countermeasures, our results have important program implications for education/treatment and ignition interlock programs. In the case of education/treatment, we found evidence suggesting that program effectiveness could be maximized by considering participant characteristics in program design. In the case of ignition interlocks, our results suggest that installation rates can be improved substantially via incentivization. This is important because interlock devices clearly reduce drinking and driving while installed, and those with an installed device collide at rates no higher than the general population. Moreover, there is still no substantial evidence in the literature of enduring effects on recidivism after interlock device removal, suggesting the need to maintain or increase the duration of an interlock condition, while further reducing barriers to installation.
Remedial-type countermeasures such as the Alcohol Education and Treatment Remedial Measures program, Ignition Interlock Program, and the Reduced Suspension with Ignition Interlock Conduct Review Program should also be simplified and harmonized as much as possible. This would reduce the operational burden for administrators and likely improve participant adherence. As a specific example, many drivers have had difficulty finishing remedial education/treatment requirements quickly enough to receive an interlock condition (and, hence, to be eligible to drive) at the intended time. Some simple program modifications, including re-consideration of which BOT components must be completed before a driver can receive an interlock condition could remedy this issue. Furthermore, a program that interacts in multiple ways with other existing programs is difficult to evaluate. This is especially true for evaluations that require a high degree of rigour for inferring causality, which often depend on consistency of program design over time.

As a final note, a commitment to alcohol-related countermeasures should prioritize evaluation and, thus, ensure that the proper infrastructure (e.g., reliable and valid data measures, data sources, and databases) is in place to enable such work. This is required to continue moving toward a culture of evidence-based policy making, emphasized in the Drummond Report (2012) and elsewhere.
APPENDIX 1.

Driver records extracted from the LCS included the following convictions. These convictions were grouped into meaningful categories by similarity. Groupings were conducted after consultation with the Road Safety Policy Office. All codes and their corresponding definitions are found in the HTA and CCC.

CATEGORY 1: BREACH OF CONDITION
- HTA 32-1
- HTA 32-9
- HTA 36
- HTA 51
- HTA 53
- HTA 190

CATEGORY 2: IGNITION INTERLOCK- RELATED
- HTA 41.2

CATEGORY 3: NOVICE-RELATED
- HTA 44.1
- HTA 48.2.1

CATEGORY 4: CONDUCT- RELATED
- HTA 130
- HTA 172
- HTA 200
- HTA 216

CATEGORY 5: MOVING VIOLATION
- HTA 128
- HTA 141
- HTA 142
- HTA 143
- HTA 147
- HTA 148
- HTA 149
- HTA 150
- HTA 156
- HTA 157
- HTA 158
- HTA 166
- HTA 167
- HTA 160
- HTA 161
- HTA 162
- HTA 165
- HTA 168
- HTA 169
- HTA 178

**CATEGORY 6: DISOBEY SIGNS, SIGNALS OR PEDESTRIAN PRESENCE**
- HTA 134(1)(3)
- HTA 135(2)(3)
- HTA 136
- HTA 138
- HTA 139
- HTA 140
- HTA 144
- HTA 146
- HTA 151
- HTA 153
- HTA 154
- HTA 159
- HTA 163
- HTA 164
- HTA 174
- HTA 175
- HTA 176
- HTA 182

**CATEGORY 7: CRIMINAL ACTIVITY**
- CCC 219
- CCC 220
- CCC 221
- CCC 249(1)(2)(3)(4)
- CCC 249.1
- CCC 252
- CCC 252(1)
- CCC 253(A)(B)
- CCC 254(1)(2)(3)(4)(5)(6)
- CCC 255(1)(2)(3)(4)
- CCC 259(4)
APPENDIX 2.

Driver records extracted from the LCS for the Warn Range, Seven Day Impoundment, Zero BAC, and Conduct Review programs included driver postal code history so that an offending driver’s neighbourhood of residence at the time of offence could be determined. Using this information, along with Statistics Canada’s March 2009 Postal Code Conversion File\textsuperscript{11}, allowed us to determine the census sub-division or census tract (if applicable) in which a driver resided at the time of offence. Demographic information for each census geographical unit was then obtained from Statistics Canada’s 2011 National Household Survey\textsuperscript{12}. Data extracted for each geographical unit and used for matching consisted of:

1) The proportion of the population with post-secondary education,
2) The median after tax income household income,
3) The proportion of owner/tenant households spending more than 30% of their total household income on shelter, and
4) The proportion of the employed population over 15 years old who drive themselves to work in a car, truck, or van.

These variables were considered especially important in matching interlock groups for two reasons. First, the amount of “expendable” money a driver has available to pay expenses associated with an interlock will likely be related to measures 1), 2) and 3). The availability and desirability of alternative methods of transportation, aside from being the driver of a vehicle, will be related to 4).


\textsuperscript{12} https://www12.statcan.gc.ca/nhs-enm/index-eng.cfm
REFERENCES


