Traffic-law enforcement and risk of death from motor-vehicle crashes: case-crossover study

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Summary

Background Driving offences and traffic deaths are common in countries with high rates of motor-vehicle use. We tested whether traffic convictions, because of their direct effect on the recipient, might be associated with a reduced risk of fatal motor-vehicle crashes.

Methods We identified licensed drivers in Ontario, Canada, who had been involved in fatal crashes in the past 11 years. We used the case-crossover design to analyse the protective effect of recent convictions on individual drivers.

Findings 8975 licensed drivers had fatal crashes during the study period. 21 501 driving convictions were recorded for all drivers from the date of obtaining a full licence to the date of fatal crash, equivalent to about one conviction per driver every 5 years. The risk of a fatal crash in the month after a conviction was about 35% lower than in a comparable month with no conviction for the same driver (95% CI 20–45, p=0·0002). The benefit lessened substantially by 2 months and was not significant by 3–4 months. The benefit was not altered by age, previous convictions, and other personal characteristics; was greater for speeding violations with penalty points than speeding violations without points; was no different for crashes of differing severity; and was not seen in drivers whose licences were suspended.

Interpretation Traffic-law enforcement effectively reduces the frequency of fatal motor-vehicle crashes in countries with high rates of motor-vehicle use. Inconsistent enforcement, therefore, may contribute to thousands of deaths each year worldwide.

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See Commentary

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Introduction

Motor-vehicle crashes are a common cause of death, disability, and demand for emergency medical care. Globally, about 1 million people die each year from traffic crashes and about 25 million are permanently disabled.[1] Unlike many common diseases, the victims are frequently young and need substantial related care for decades. Most crashes are unintended, unexpected, and could have been prevented by small differences in driver behaviour.[2] Prevention is particularly important for protecting health, given that most drivers will be in at least one crash during their lifetime. Moreover, about half of all crash deaths occur at the scene, with no opportunity for life-saving treatment.[3]

An individual's crash risk depends on how that person drives and how other road users behave,[4] yet the public is somewhat sceptical about traffic-law enforcement.[5,6] News exposés and the entertainment industry have suggested some law-enforcement efforts are merely revenue generating in locations with low crash rates, done by biased officers.[7] Any balance between safety and mobility involves trade-offs, and people generally resist efforts that interfere with their driving.[8] Police, themselves, sometimes view traffic enforcement as a duty beneath their skills.[9] Furthermore, the effectiveness of most laws has not undergone scientific scrutiny, and the few available studies are mostly ecological analyses using disputable before-and-after comparisons of intermediate outcomes (adherence) rather than definitive outcomes (death).[10,11]

Rigorous testing of the effectiveness of traffic enforcement for preventing deaths might contribute to better decisions. First, testing could check the popular claim that enforcement yields no lives saved and a contrary net increase in crashes because drivers watch for police instead of hazards[12] would be useful. Second, testing could help to assess the effect of allocation of scarce police resources to traffic safety compared with other community services, and also affect attitudes about charging.[13] Third, results could raise debate on adoption of new enforcement technologies such as photo radar and red-light cameras.[14,15] A shortage of data may underlie inconsistency in enforcement practices globally, which could indirectly contribute to hundreds of preventable deaths each day.[16]

Methods

Setting

Ontario, Canada, in 1993—the study mid point—had a population of about 9·6 million people and 6·8 million drivers; 0·4 million drivers were involved in crashes, and there were 1135 crash deaths.[17] Police were responsible for 6·0 million licensed vehicles, 20 000 km of roads, and 1·0 million traffic convictions, but used no special enforcement technologies.[18] Licences were graduated for the first 2 years of driving (restrictions on highway

driving and other limitations), and general licences could be suspended after accumulation of nine penalty points (the annual rate of suspension was about 0·6% of drivers). A conviction for speeding at 20 km per h higher than the limit, for example, involved a Can$100 fine (around UK£42) and three penalty points. Ontario had no programmes for dismissing convictions if a person completed a driver improvement course.

Drivers and driving records

We identified all drivers involved in fatal crashes between Jan 1, 1988, and Jan 1, 1999, in Ontario. A fatal crash was defined as causing death of any person at the scene, on arrival at hospital, or within 1 month of the event. We included drivers irrespective of whether they survived, were at fault, or held special diplomatic immunity from prosecution. We excluded drivers who were unidentified by police, whose licences were not registered in Ontario, or who had held licences for less than 2 years, because of graduated licence restrictions. Duplicate records were deleted if they showed identical time, place, and driver. The primary analyses focused on drivers whose driving permit was maintained during the study period; we assessed drivers whose permits were suspended in secondary analyses.

Ontario drivers' records were traceable to individual-driver level and accessible for research purposes.[17,18] Such research did not require voluntary consent and covered a person's full driving record. These databases were identical to the official files on drivers, serious crashes, and traffic convictions. Individual convictions could be removed from the public record after 2 years, but were not erased from computer files; hence, drivers' lifetime histories were available for analysis. The available data did not include parking violations or driving violations on roads outside Ontario. Similarly, the information on the date of obtaining a full licence reflected Ontario residency and did not include earlier licences elsewhere.

Records were linked by use of the encrypted licence number to data on the person, vehicle, and roadway conditions, with the following stipulations. Age, years of licensed driving, and previous convictions were current on the day of the crash. Licence class was simplified to the highest certification for people holding multiple licences. Data on alcohol were based on police reports, and missing values were coded as negative. Vehicles were classified as car, truck, or other because of small numbers of specific types. Road surface conditions were classified as dry, wet, or snowy (including ice, sleet, slush, and similar winter conditions). Crash locations were described as related or unrelated to an intersection, as recorded in the police report.

Analysis

We analysed convictions by use of a case-crossover design, a technique for assessing a temporary change in risk associated with a transient exposure.[19] Each person was his or her own control and thereby eliminated confounding due to all fixed characteristics, including genetics, personality, education, lifestyle, and chronic diseases.[20] The primary analysis used a pair-matched analytical approach to contrast a period immediately before the crash with a comparable period substantially before the crash.[21] This analysis would identify a safety benefit if periods with convictions were followed by fewer crashes than would be expected due to chance. Therefore, a benefit is implied if the absence of a conviction is associated with the onset of a crash.

In the primary analysis we assessed licensed drivers and compared the month immediately before the crash with the same month 1 year before. For example, for a crash on July 1, 1995, we compared the month of June, 1995, with June, 1994. Supplementary analyses compared the same immediate previous period to five alternative control periods to check the robustness of our findings: with the month 11 months previously, 13 months previously, 24 months previously, 36 months previously, or an extended full-year span centred 12 months previously. For example, we compared the control month of June, 1994, with July, 1994, May, 1994, June, 1993, June, 1992, and the 1-year period with July 1, 1994, as the central date. We repeated the analysis for suspended drivers to test whether smaller safety benefits were observed where smaller safety benefits would be anticipated.[22,23]

We assessed further issues by stratification. The first approach relied on grouping drivers by personal characteristics or crash features and testing for discrepancies across major subgroups. We analysed crash severity by two separate methods. First, fatal crashes were investigated by police who estimated the damage to drivers' vehicles. Second, a fatal crash did not always kill all persons involved and we assessed benefits among drivers who survived admission to hospital, were discharged into the community, and returned to active driving by analysis of their driving records after the crash. In addition, we explored how long a potential association might persist, denoted as a persistence analysis, by examining hazard intervals shifted progressively backward in time from the crash day (with corresponding displacements of control intervals). For

	Number (% [n=8975])
Characteristics	
Age (years)*†	
<30	2229 (25)
30–50	3921 (44)
>50	2800 (31)
Sex	
Male	6512 (73)
Female	2463 (27)
Years of licensed driving*†	
⩽9	4032 (45)
⩾10	4918 (55)
Corrective eyewear	
Yes	3224 (36)
No	5751 (64)
Licence class	
General	7110 (79)
Advanced‡	1865 (21)
Previous driving convictions*	
⩽3	6853 (76)
⩾4	2122 (24)
Alcohol detected	
Yes	634 (7)
No	8341 (93)
Road surface condition	
Dry	5822 (65)
Wet	1636 (18)
Snowy	1517 (17)
Road configuration	
Intersection	2836 (32)
Non-intersection	6139 (68)
Vehicle type	
Car	5689 (63)
Truck§	2649 (30)
Other¶	637 (7)

*Updated to time of fatal crash. †Excludes 25 drivers with missing birth dates. ‡Includes permits for motorcycles, trucks, and special vehicles. §Includes passenger vans or sports utility vehicles (n=605) and delivery vans (n=165). ¶Includes motorcyles (n=227), buses (n=137), bicycles (n=58), and 17 other types (n=215).

Table 1: **Selected characteristics of drivers and crashes**

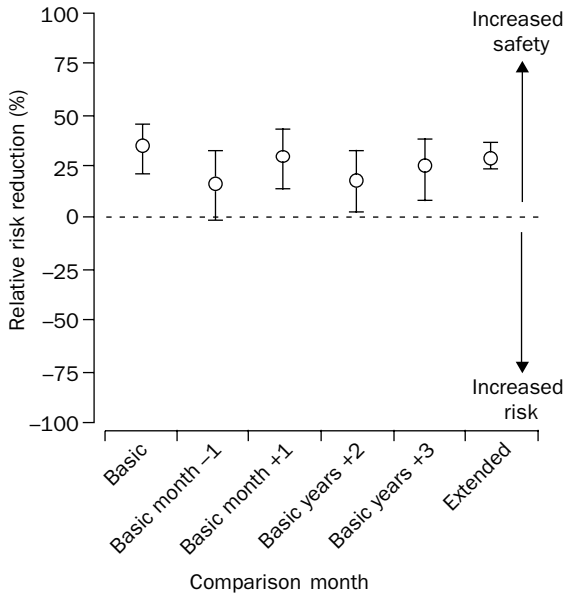

Figure 1: **Estimated relative risks (95% CI) for six different control intervals**

Basic=1-month control periods before collision separated by 12 months. Basic −1 month=separation of 11 months. Basic +1 month=separation of 13 months. Basic +2 years=separation of 24 months. Basic +3 years=separation of 36 months. Extended=1-year control period centred on date 12 months before collision.

example, a 1-month persistence interval would include May 1994 and May 1995 when assessing a crash on July 1, 1995.

Statistical analysis

We calculated the sample size to provide an 80% chance of detecting a 15% increase or decrease in crash rates. Relative risks were estimated with methods for matched-pairs studies on the basis of exact binomial tests and conditional logistic regression. Analogous methods were applied when the control interval was 12 months rather than 1 month in length. In all analyses, the time immediately before the crash was 1 month in length (estimates based on intervals of 2, 6, and 8 weeks yielded similar results and are not shown). Each month before the fatal crash was assessed as an independent hazard time period. All p values were two-tailed, all relative risks calculated with 95% CI, all analyses drawn from all data available. Relative risk reductions greater than zero show a safety benefit, and CI that exclude zero are significant. We did all analyses on S-PLUS (version 3.4) and Statview (version 5.0) software.

Role of the funding source

The study sponsors had no role in the study design, data collection, data analysis, data interpretation, the writing of the report, or in the decision to submit the paper for publication.

Results

8975 licensed drivers were involved in fatal crashes during the 11-year study period. In addition, 4861 suspended drivers were involved in fatal crashes. Data on convictions showed no anomalous entries or gaps related to licence numbers or to date, description, and demerit points for each offence. Data on crashes also showed no irregularities over the critical data on drivers' licence numbers and dates. Data on sex, licence class, road surface, road configuration, and vehicle type had no irregularities. Data on corrective eyewear and alcohol consumption were assumed complete with missing values interpreted as negative. Data on previous convictions were derived directly from the file of each individual. Data on birth date and first licensing date were missing for 25 individuals; these individuals appear in the primary analysis but are excluded from the subanalyses of driver age and experience.

The typical licensed driver was a man aged 43 years holding a general permit, and who drove a car in dry road conditions (table 1). Most of the crashes did not involve alcohol and were not at intersections. Before the crashes, the lifetime driving-conviction history of the entire group of licensed drivers accounted for 21 501 convictions, most commonly for speeding without penalty points (6682 convictions) or speeding with penalty points (6493 convictions). There was a notable seasonal pattern; crashes and convictions were more common in the summer than the winter.

135 licensed drivers had had driving convictions in the month before the fatal crash, 204 had had convictions in the same month 1 year before, and six had had convictions in both months. The primary analysis indicated that convictions were associated with a 35% reduction in the relative risk of a crash (95% CI 20–45, p=0·0002). Analyses based on alternative control time periods yielded similar findings (figure 1). As expected, the analysis of the extended control time of 1 year resulted in a minor drift of the point estimate and narrowing of the CI. For suspended drivers, however, there was no significant decrease in risk associated with

	Number with conviction in previous month	Relative risk reduction (95% CI)*
Complete cohort	135	35 (20 to 45)
Age (years)		
<30	58	34 (10 to 52)
30–50	62	28 (2 to 48)
>50	15	55 (13 to 75)
Sex		
Male	111	37 (20 to 50)
Female	24	19 (−47 to 50)
Years of licensed driving†		
⩽9	66	39 (17 to 54)
⩾10	69	30 (6 to 48)
Corrective eyewear		
Yes	47	26 (−6 to 48)
No	88	39 (20 to 52)
Licence class		
General	104	32 (13 to 45)
Advanced	31	42 (10 to 61)
Previous driving convictions		
⩽3	64	33 (10 to 50)
⩾4	71	37 (17 to 52)
Alcohol detected		
Yes	15	42 (−15 to 68)
No	120	34 (17 to 45)
Road surface condition		
Dry	90	35 (17 to 50)
Wet	25	31 (−15 to 57)
Snowy	20	38 (−15 to 62)
Road configuration		
Intersection	31	48 (20 to 64)
Non-intersection	104	29 (10 to 43)
Vehicle type		
Car	83	26 (2 to 43)
Truck	42	47 (23 to 62)
Other	10	36 (−54 to 70)

*Indicates decrease in chance of a fatal crash during month after conviction compared with month after no conviction. †Positive values indicate increased safety, negative values indicate increased risk.

Table 2: **Relative reduction in crash risk associated with a conviction**

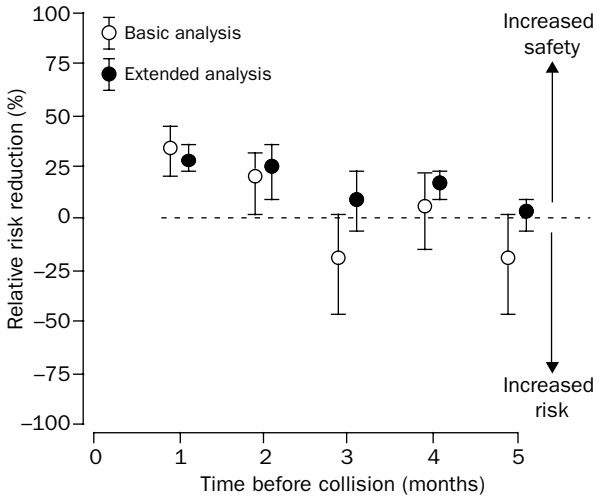

Figure 2: **Relative risks (95% CI) for different persistence intervals**

Basic analysis=1-month control periods before collision separated by 12 months. Extended analysis=1-year control period centered on date 12 months before collision.

convictions (relative risk reduction –16% [–36 to 2], p=0·12).

The relative risk reduction associated with traffic convictions was consistent among subgroups of licensed drivers. In no group were traffic convictions associated with a harmful effect (table 2). The smallest relative risk reduction was for women, although the inconsistency between women and men was not significant (p=0·39) and women were generally under-represented in fatal crashes. The relative risk reduction was almost the same for drivers with four or more and for those with three or fewer previous convictions and almost the same for drivers with alcohol and with no alcohol detected by police. Analyses of each of the 11 separate years showed a relative risk reduction in all but 1 year and no significant increasing or decreasing trends.

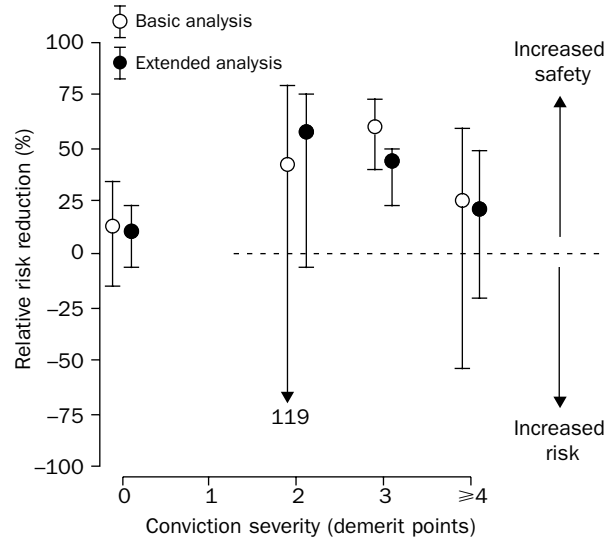

Figure 3: **Relative risks (95% CI) for different types of convictions**

Basic analysis=1-month control periods before collision separated by 12 months. Extended analysis=1-year control period centered on date 12 months before collision. Drivers with no convictions excluded. Relative risks undefined at severity=1 because no driver accumulated exactly 1 point, and do not increase proportionately with conviction severity.

The decrease in risk was greatest for convictions made close to the time of the crash. In the analysis of persistence of effect, for control periods of 1 month's duration the decrease in risk was greatest for convictions made less than 1 month before the crash and was not significant for convictions made 3 or more months before the crash (figure 2). The same analysis with control periods of 12 months' duration indicated that a decrease in risk did not persist for convictions 5 or more months into the past. In no analysis did we find a significant increase in risk. In addition, we found a consistent relative risk reduction after convictions, irrespective of hour of day (range 24–55%), day of week (24–53%), or season of year of the crash (17–52%).

Analysis of crashes according to police estimates of damage, showed marginally inconsistent higher relative risk reduction for drivers whose vehicles were demolished compared with those whose were not (42 *vs* 23%, p=0·22). Relative risk reductions were similar for drivers who did or did not have objective evidence of subsequent driving activity (35 *vs* 34%, p=0·95). Together these findings suggest that safety benefits extended to crashes of greater or lesser severity.

In the subgroups of convictions, speeding convictions in which the driver received penalty points were associated with a larger relative risk reduction than speeding convictions with no penalty points (51 *vs* 0%, p=0·011). Convictions related to administrative errors, careless driving, seatbelt failure, and disobeying of a traffic signal were all associated with similar relative risk reductions (range 31–57%). When based on severity of punishment rather than the type of offence, convictions for which two to three penalty points were awarded showed generally more safety benefit than did convictions with no penalty points (figure 3).

We tested for adverse effects related to enforcement by review of coroners' data on all deaths involving police activity. We found 24 deaths related to traffic enforcement during the study period. These deaths included 17 drivers suspected of criminal activity, five bystanders, and two police officers. The typical driver who died was a man aged 26 years pursued by police after fleeing a spot check for alcohol or a speeding violation. Four of the five bystanders were passengers in a vehicle fleeing a spot check, four had positive toxicology at autopsy (alcohol or illicit drugs), and four were teenagers. The two police officers who died (separate events) were each hit by drivers while writing a speeding ticket for another motorist.

Discussion

Almost no driver wants to be in a serious crash, yet almost all drivers violate traffic laws at some time, such as by intermittent speeding.[24] We studied more than 10 million people for longer than a decade and found that convicting drivers for traffic offences reduces the rate of fatal crashes. Each conviction leads to a 35% decrease in the relative risk of death over the next month for drivers and other road users; conversely, each conviction not issued would lead to a corresponding increase in risk. Our findings also imply that increasing the frequency of traffic enforcement might further reduce total deaths, that emphasis of moderate penalties (around three points) is useful, and that past procedures led to some deaths that might not have otherwise occurred.

Our findings extend past research because the individual rather than the region is the unit of analysis

and because each person is their own control rather than using statistical models to adjust for confounding. A meta-analysis of past ecological data implied a 2% risk reduction from manual speed enforcement, a 19% reduction from automated speed enforcement, an 11% reduction from red-light violation enforcement, and a 4% reduction from enforcement of drink-driving laws.[25] The results of individual reports varied even more, presumably because of difficulties in separating the effects of enforcement from publicity campaigns, fallible implementation, statistical artifact, and unmeasured ecological bias.

The major impediment to general traffic-law enforcement is a lack of public support. Unlike when receiving preventive health care, individuals commonly resist convictions with deception or argument.[23,26] Enforcement can reduce civil liberties, disrupt traffic flow, restrict mobility, or have other unintended consequences on quality of life and economic prosperity. Enforcement strategies are also inconsistent, since many drivers have violations, but few are apprehended, and even fewer have malicious intent.[7] Finally, police resources are scarce and apprehending other types of offenders may be a higher societal priority because one murder may draw more attention than the thousands killed daily in motor-vehicle crashes worldwide.

Traffic enforcement has potential indirect effects on health of uncertain importance. A road-safety programme may intercept other unlawful activity because criminals frequently drive to and from their illegal operations, including the traffic of illicit drugs. Visible police presence might deter violent behaviour or stop repeat offenders; for example, the convicted Oklahoma City bomber was apprehended at an incidental traffic stop. In addition, crashes are an economic drain on society—costs are about US$200 billion yearly in the USA[27]—that the public cannot escape because of insurance premiums or other market forces, and that ultimately decreases the funding available for medical care.

Our research has limitations. The intermittent nature of driving and the potential for out-of-region activity leads to spurious positive correlations in case-crossover analysis and causes us to underestimate the risk reduction. Selection bias may cause further underestimation because enforcement targets drivers who are predisposed to crashes and thereby may further obscure potential protective associations.[20,28] Our estimates do not imply that every conviction is effective and do not predict how results might change at extremes of enforcement or with cultural adaptation. Finally, we once more raise the issue of hard-core problem drivers, who drive despite having suspended licences, but we can provide no headway on this issue.[29]

Our research is prone to misinterpretation. We have not assessed other deterrents, such as being charged but not convicted, being stopped but not charged, or being an observer when others are stopped. We have not definitively proved causality, yet a randomised experiment of individual drivers would be very difficult. We have not shown that traffic-law enforcement is the only way to reduce motor-vehicle deaths since gains may also be possible through advances in information, incentives, technology, or culture. We have not tested highly specific questions about road safety because we have limited statistical power and imperfect direct data on alcohol or other disturbances, as is typical in studies of human behaviour.

Our data suggest that about one death is prevented for every 80 000 convictions, one emergency department visit for every 1300 convictions (assuming the benefits apply to crashes of all severity), and $1000 in societal costs for every 13 convictions (including property damage and lost time). The observed 35% relative risk reduction in death is greater in magnitude than the roughly 20% relative risk reduction from all mandatory vehicle improvements of the past 50 years, yet enforcement effects are transient.[3,30] Policies of more frequent enforcement could yield more net savings and could also be revenue neutral if designed efficiently. A small relative risk reduction could immediately prevent a large amount of death, disability, and health-care demands.

Contributors

All researchers contributed to the design, analysis, and reporting of this research. D Redelmeier had full access to all of the data in the study, and bears final responsibility for mistakes.

Conflict of interest statement

D Redelmeier draws income from medical practice at Canada's largest trauma centre, Sunnybrook and Women's. R Tibshirani draws earnings as a member of the advisory board of several companies, none of which is involved in traffic safety. Leonard Evans draws a pension from the General Motors Corporation and has earnings from writing, speaking, and consulting on matters related to traffic safety.

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