Evaluation of legislation to reduce the drink drive limit in Scotland: a natural experiment

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Conflict of interest

Competing interests: Prof Emma McIntosh and Prof Andy Jones are members of the NIHR PHR funding board.

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Scientific summary

Background

It is well recognised that drink driving is a leading cause of road traffic accidents (RTAs). The first country to introduce a legal BAC limit to combat drink driving was Norway in 1936, where it became illegal to drive with a blood alcohol concentration (BAC) level of 0.05 g/dL or above. Since then many other countries and jurisdictions have followed this 'Scandinavian model' to deter drink driving, and legal BAC limits are in place in countries and regions across Europe, North America, Japan and Australasia. The British Road Safety Act (BRSA) introduced a legal limit of 0.08 g/dL in 1967 which is still in place today, with the exception of Scotland where the BAC limit was reduced to 0.05 g/dL on 5th December 2014.

The study was based on assessing whether the change in drink driving legislation in Scotland (the intervention) led to a reduction in RTAs in the first two years of follow-up. We also wanted to assess whether any intervention effect differed by level of socio-economic deprivation (SED), did it have a wider (unintended) outcome of reducing alcohol consumption per capita, and to perform an economic evaluation to assess cost-effectiveness of the intervention.

Research questions

This research answered four primary questions:

1. Has the change in drink driving legislation in Scotland been effective (reduction in RTAs)?
2. Has the change in drink driving legislation in Scotland led to changes in relative and absolute RTA rates that differ by levels of socio-economic deprivation?
3. Has the change in drink driving legislation in Scotland led to a reduction in population alcohol consumption?
4. Has the change in drink driving legislation in Scotland provided good value for money (been cost-effective)?

Methods

Research design

We employed a natural experimental design to measure the causal effect of the change in BAC legislation in Scotland from 0.08 to 0.05 g/dL. Our control group was England and Wales, the other countries in Great Britain (GB) that still have a 0.08 g/dL BAC law. The data for the intervention and control groups used to measure effectiveness came from the same data sources and covers the same study period (four years in duration, two years pre- and post-legislation change – January 2013 to December 2016).

A representative from The Royal Society for the Prevention of Accidents was part of our steering group and contributed to the research design. Further, we engaged a public involvement group and discussed the research with them. This led to a list of potential confounding variables being created and these variables were then considered for adjustment in the statistical analyses.

Research question 1

Outcome measures: weekly counts and rates of all RTAs.

Data source: police accident data (STATS19). Variables that were used are accident index, date of accident, age of driver(s), sex of driver(s), postcode of driver(s), and names of local police force reporting the RTA.

Denominators for RTA rates: the ideal denominator (that does not exist) is the number of miles driven by each person at risk of having a RTA and we used traffic counts as a proxy for this. The traffic counts were obtained from automatic traffic counters (ATCs) which count vehicles passing over them 24 hours a day across the GB road network. We also used mid-year population estimates as an alternative denominator.

Statistical analysis: descriptive statistics to assess the comparability of the intervention and control groups and plots of RTA counts and rates over time were produced. For RTA counts and rates, and separately for the intervention and control group, negative binomial regression models were fitted to panel data sets to test for a change in outcome level after the new legislation was in place. That change in level was measured by fitting a covariate that took the value zero at all points before the new legislation and took the value one at all points thereafter (note: in the control group models, this covariate represents a pseudo-change in legislation). The models were adjusted for underlying temporal trend by fitting a covariate representing week number, and for seasonality by covariates representing 4-weekly periods of the year. The models were then further adjusted for age, sex and SED. To obtain a 'Difference-in-Differences' (DiD) type measure of effect, an interaction term between intervention group indicator and the binary covariate for indicating pre- and post-change in legislation ('pseudo' change for control) was assessed.

Research question 2

To answer this research question, we followed the same methods as outlined for research question 1.

Measuring SED: we required an area-based measure of SED that was appropriate to use across GB. During the course of the study it became apparent that there was not a gold standard approach to follow. We originally planned to use the Carstairs index. A difficulty to overcome was that postcode sectors and electoral wards, traditionally the levels at which Carstairs is measured, markedly differ in size when comparing Scotland to the rest of GB. Unfortunately, after starting to explore a standardisation approach to address this it became apparent it would be logistically impossible to complete due to a lack of bespoke look-up tables. Therefore, we changed our approach and rather than standardise for the whole of GB, we used an area-based measure of SED derived separately for Scotland and England & Wales.

Statistical analysis: we tested for effect modification by including an interaction term between the intervention group indicator and SED. If this interaction was statistically significant, we planned to measure the slope index of inequality (SII) and relative index of inequality (RII) based on rates of RTAs in the before and after intervention periods, and in both the intervention and control groups, and test for change in SII and RII.

Research question 3

Outcome measures: alcohol consumption per capita split by whether estimates were from off- or on-trade alcohol retail sales data.

Data source: alcohol retail sales data were provided from market research specialists. These data provide population level estimates based on electronic sales records from large retailers and a weighted stratified sample of smaller retailers. In the absence of a true gold standard, alcohol retail sales data is a high-quality measure of per capita alcohol consumption.

Statistical analysis: plots of alcohol consumption per capita over time were produced. For off- and on-trade sales, and separately for the intervention and control group, seasonal autoregressive integrated moving average (SARIMA) error models were fitted to the relevant time series of data points. A change in level associated with change of legislation was measured by fitting a covariate that took the value zero at all points before the new legislation and took the value one at all points thereafter. The form of the autocorrelation for the SARIMA errors was identified from autocorrelation plots and partial autocorrelation plots. Each SARIMA model controlled for two exogenous variables, off-trade sales from the same intervention group (or on-trade sales, depending on model) and on-trade sales from the other intervention group (or off-trade sales, depending on model). In a sensitivity analysis, we adjusted for country-specific Aldi and Lidl market share percentages (as data from those supermarkets are not part of the market research data.

Research question 4

Three frameworks for economic evaluation (cost-effectiveness, cost-utility and cost benefit analysis (CBA)) were considered to reflect different outcomes from the effectiveness analysis. If differences in effectiveness were not detected, the economic evaluation reverted to a cost analysis.

Costs: those associated with prevention of accidents were categorized into ‘Casualty related costs’ and ‘Accident related costs’. Using this distinction, casualty related costs comprise human costs, medical and ambulance costs and lost output. Accident related costs comprise police costs, insurance and admin costs and costs of damaged property. Additional associated costs related to deaths were estimated (Ambulance, A&E admissions, coroner and legal costs) based on Scottish data sources and estimates from the literature. Costs also included the implementation costs of the law change and associated campaign to advertise the reduced limit as well as the associated surveillance and monitoring costs.

Outcomes: these included: weekly rate of RTAs (for the cost-effectiveness analysis); years of life lost (YLL), Quality Adjusted Life Years (QALYs) (for the cost-utility analysis) and contingent valuation values related to prevented fatalities as well as human injuries (for the CBA). The estimation of QALYs gained though the prevention of RTAs was informed by a literature review. By utilising health states and QALYs associated with RTAs from previous literature, estimates of QALYs lost per injured survivor were included in the evaluation to generate any resulting QALY impacts associated with the change in drink driving legislation. If we find evidence of effectiveness, the cost and outcome results from the 2-year follow-up results would be extrapolated within a lifetime cohort model to identify the likely longer-term impacts of the change in Scottish drink driving legislation.

Perspective / discounting / sensitivity analysis: the perspective of the economic evaluation is the societal perspective to allow health sector and broader judiciary costs to be included. The time horizon is lifetime costs and outcomes with a population health economics discount rate of 1.5% applied. If evidence of effectiveness, detailed sensitivity analysis will be undertaken to identify thresholds of cost-utility (when using QALYs as the outcome measure) and cost-benefit (when using contingent valuation for prevented injuries).

Results

Research question 1

The distribution of age, sex and SED demographics was very similar for the intervention and control groups and therefore risk adjustment for these variables had little impact on the statistical models. The change in drink drive legislation was associated with a 2% relative decrease in RTA counts in Scotland (relative risk (RR) 0.98, 95%CI (0.91, 1.04), p=0.53). However, the pseudo-change in legislation was associated with a 5% decrease in RTA counts in England and Wales (RR 0.95, 95%CI (0.90, 1.00), p=0.05). For RTA rates with traffic flow denominator, the DiD type estimate indicated a 7% increase in rates for Scotland relative to England & Wales (unadjusted RR 1.07, 95%CI (0.98, 1.17), p=0.1).

Research question 2

The effect sizes observed overall were similar in value across SED levels and this was reinforced by the p-values for the tests of interaction – p=0.72 (RTA counts), p=0.71 (RTA rates with traffic flow denominator) and p=0.72 (RTA rates with population denominator).

Research question 3

The change in drink drive legislation was associated with a 0.3% relative decrease in per capita off-trade sales (-0.3%, 95%CI (-1.7%, 1.1%), p=0.71) and a 0.7% decrease in per capita on-trade sales (-0.7%, 95%CI (-0.8%, -0.5%), p<0.001). The corresponding results for the effect of the pseudo-change in legislation in England and Wales indicated increases in per capita off- and on-trade sales. The results were not sensitive to adjustment for country-specific Aldi and Lidl market share percentages.

Research question 4

With the results revealing no significant change in effectiveness, the economic evaluation became a cost analysis focusing on the resource impacts of the legislation. Our cost results showed the financial costs of changing the drink driving legislation in Scotland were not insubstantial.

Principal findings

RQ 1) we found that lowering BAC limit from 0.08 to 0.05 g/dL in Scotland was not associated with a change in the level of RTAs in the first two years post-legislation change; RQ 2) as well as no overall effect for RTA outcome, we found no effect modification by socio-economic deprivation level; RQ 3) we found that lowering BAC limit from 0.08 to 0.05 g/dL in Scotland was not associated with a change in the level of off-trade alcohol consumption in the first two years post-legislation change, however we found that lowering BAC limit from 0.08 to 0.05 g/dL in Scotland was associated with a small relative reduction (less than 1%) in the level of on-trade alcohol consumption; RQ 4) significant costs were incurred in changing the legislation.

Conclusion

The change in drink drive legislation in Scotland in December 2014 did not have the expected effect of reducing RTAs in the country, nor did it change Scotland's alcohol drinking levels. Our main finding for RTAs was unexpected. In our *a priori* theory of change, we cited legislation failure as a plausible explanation if we went on to find no change in the RTAs outcome. Our research has shown lack of enforcement as the most likely reason for legislation failure. Another possible reason is that large effect sizes observed in previous high-quality studies of the same intervention are now difficult to achieve given the large improvements over time in road safety and drink driving becoming increasingly socially unacceptable. Although our study did have limitations (e.g. denominator for rates, unmeasured confounding) we do not feel any resulting bias would be large enough to change our conclusions.

Registration

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