# Drink, Death and Driving: Do BAC Limit Reductions Improve Road Safety?*

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January 23, 2020

**Abstract**

This study exploits a natural experiment in Scotland where the legal blood alcohol content (BAC) limit was reduced from 0.8mg to 0.5mg per 100ml of blood while staying constant in all other parts of the UK. Using a difference–in–differences design, we find that this change in the BAC level had no impact on either traffic accident or fatality rates.

***Keywords:*** Road Traffic Fatalities; Traffic Accidents; Difference-in-Differences; Blood Alcohol Content

***JEL:*** I12; I18; K42

*We gratefully acknowledge the support of The Carnegie Trust for support for this project (Grant Number VAC007905).

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# 1 Introduction

Motor vehicle crashes are the leading cause of death for those aged 15-35 in developed countries (US Centers for Disease Control and Prevention, 2015). Even non-fatal car accidents cause substantial negative externalities and financial costs in the form of injury, health care costs, car repairs, higher insurance premiums, and lost time and productivity. For the UK, Morris et al. (2006) calculate that the average traffic accident is associated with costs to the tune of £40,000-50,000. Policy makers have tried to tackle this problem through a number of measures. Higher gasoline taxes have been shown to be associated with a drop in fatalities, not least because it makes driving more costly (Grabowski and Morrisey, 2006). Because alcohol impairment is a leading contributor to traffic accidents, drink driving has received particular attention. Carpenter and Dobkin (2009) show that keeping the legal drinking age at 21 helps prevent around 400 fatalities per year. Hansen (2015) finds that drivers who experience harsher penalties for drink driving offenses are effectively deterred from recidivating. Many countries have also lowered the legally permitted blood alcohol content (BAC) which is the topic of this paper. Exploiting differential timing across US states in lowering their BAC limits from over 0.10 to 0.08 mg per 100ml of blood, Dee (2001) concludes that BAC limit reductions helped reduce road fatalities by 16.5%. Voas et al. (2000) show that these BAC laws in combination with other alcohol safety laws, such as administrative license revocation and mandatory seat belt laws, have been strong contributors to the downward trend in fatal crashes which is well documented for the US and other developed countries. However, because BAC laws have often been part of a complex set of regulations and coincided with changes in enforcement and police presence, their effectiveness has often been hard to evaluate.

Our study provides the first evidence on the effectiveness of BAC limit reductions in the UK and makes several novel contributions. First, we investigate whether a further reduction in the BAC limit – from 0.08mg to 0.05mg – still has a marginal effect on traffic fatalities. Second, the institutional setting of the UK makes for a particularly clean natural experiment. US states – the main focus of the previous literature – are very heterogeneous, rendering the decision to lower BAC limits potentially endogenous even when state and time-fixed effects are controlled for. In contrast, the UK is a more homogeneous country particularly with respect to anti-alcohol sentiment, traffic laws and policing. Almost all road safety legislation is 'reserved' to the UK

Parliament, and hence common across the UK, with the recent devolution of control of BAC limit regulation in Scotland to the Scottish Parliament a rare exception to this general rule (alongside the power to vary the national speed limits, although this power has not yet been exercised)[1]. For instance, the UK has universal primary seat belt laws that carry the same £100 fine for violations. While there are 45 territorial police forces (including Police Scotland) across the UK, a centralized Driver and Vehicle Licensing Authority (DVLA) handles the administrative process of driving license revocations for all of Great Britain (GB)[2]. Sentencing guidelines for DUIs are virtually identical across GB.

We exploit a reduction in the BAC limit that took place in one region - Scotland - in 2014 where the other ten regions of Great Britain maintained their limits. There is little indication of policy endogeneity that might jeopardize the validity of our difference-in-differences design. For instance, alcohol taxes and alcohol consumption related laws were also identical during our period of observation[3]. In contrast to the US, the same BAC limits apply to all drivers regardless of their age or experience. Similar to most US states, DUIs are penalized depending on their severity with income-dependent monetary fines (up to £5,000), demerit points, license suspensions, and prison time, with harsher penalties for repeat offenders. Finally, the UK also offers particularly detailed data. Due to data limitations, previous studies have mainly focused on fatalities even though they account for a small fraction of all traffic accidents. For this study, we are in the unique position to be able to leverage rich, geo-coded data on more than 1,176,672 accidents leading to 1,576,187 casualties and 14,737 fatalities between 2009 and 2016.

## 2 Data and methods

Our data come from the Department of Transport's STATS19 police report system on road accidents and road casualties. This provides us with data on the universe of police-reported traffic accidents alongside demographic details on everyone involved, including drivers, passengers, and pedestrians. We combine the accident data with annual population estimates for each of Great Britain's eleven NUTS1 regions, of which our treated region Scotland is one, in order

[1] This document provides a useful summary of the responsibilities for road safety across the UK: `https://www.gov.uk/government/publications/road-safety-powers-and-devolution-summary-of-responsibilities`.

[2] Great Britain comprises the whole United Kingdom except Northern Ireland.

[3] The only major difference is that Scotland introduced a statutory minimum price for alcohol, but the law only went into effect in May 2018.

to calculate monthly fatality and accident rates per 100,000 people for every month from 2009 to 2016 for each region. We match these data to regional weather indicators obtained from the MET office, in particular information on the mean temperature, number of days with sunshine per month, amount of rainfall, and number of days with air frost.

Table 1 shows the means and standard deviations of our main outcome variables for both our treatment region, Scotland, and our 10 control regions in England and Wales. We show fatality and accident rates for treatment and control for the 24 months leading up to the reform as well as the 24 months following the change in the BAC limit.

The BAC reduction was largely unanticipated. The legislation was introduced in the Scottish Parliament at the end of October 2014; it was unanimously approved and went into effect on 5th December 2014. Table 1 shows that accident and fatality rates were at very similar levels across Scotland and the rest of Great Britain in the pre-treatment period. One concern with our difference-in-differences identification strategy is that Scotland may have lowered its BAC limit because it had more traffic fatalities to begin with, thus rendering the policy change endogenous. However, row 2 of Table 1 shows that there were no statistically significant differences in fatality rates between Scotland and the other regions. Nor are there any differences for vulnerable subgroups such as young drivers. In fact, Scotland experienced fewer accidents per capita than the rest of Britain in the years leading up to the reform.

Moreover, a simple comparison of the means in Table 1 indicates that the lower BAC limit had little effect on either accidents or fatalities. In order to further test this hypothesis while explicitly accounting for potentially confounding factors, we implement a difference-in-differences design of the following form:

$$y_{rt} = \beta_0 + \beta_1 Scot_r + \beta_2 Post_t + \beta_3 Scot_r \times Post_t + \tau_t + \theta_r + \gamma X_{rt} + \epsilon_{rt} \tag{1}$$

where $y_{rt}$ are the accident and fatality rates in region $r$ during month $t$, $Scot_r$ is an indicator for the treatment region, $Post_t$ is dummy that is equal to 1 in post-December 2014 months, $X_{rt}$ represents our weather covariates. We also control for a full set of month-year fixed effects and region fixed effects. We also experimented with several specifications of region-specific time trends, neither of which changed our results. Our main coefficient of interest is $\beta_3$. It will yield the unbiased effect of the BAC limit reduction on our outcomes of interest as long as the

| | FATALITIES | | | | ACCIDENTS | | | |
|---|---|---|---|---|---|---|---|---|
| | Scotland | | England & Wales | | Scotland | | England & Wales | |
| | ***Pre-Reform**** | ***Post-Reform*** † | ***Pre-Reform**** | ***Post-Reform*** † | ***Pre-Reform**** | ***Post-Reform*** † | ***Pre-Reform**** | ***Post-Reform*** † |
| **#/Month** | 14.96 | 15.25 | 129.88 | 132.67 | 740.63 | 704.13 | 11123.17 | 10861.17 |
| | (3.96) | (4.20) | (19.01) | (14.99) | (46.87) | (44.30) | (1064.53) | (649.92) |
| **Total Rate** | 0.28 | 0.28 | 0.23 | 0.23 | 13.88 | 13.07 | 19.46 | 18.70 |
| | (0.07) | (0.08) | (0.03) | (0.03) | (0.88) | (0.83) | (1.84) | (1.15) |
| **16-25 Rate** | 0.42 | 0.44 | 0.41 | 0.39 | 30.95 | 29.21 | 50.21 | 47.63 |
| | (0.29) | (0.31) | (0.08) | (0.11) | (3.02) | (2.80) | (4.67) | (3.63) |
| **Over 25 Rate** | 0.31 | 0.30 | 0.25 | 0.25 | 14.44 | 13.69 | 20.00 | 19.36 |
| | (0.10) | (0.07) | (0.04) | (0.03) | (1.08) | (0.91) | (1.91) | (1.12) |
| **Night-time Rate** | 0.11 | 0.10 | 0.09 | 0.10 | 3.81 | 3.61 | 5.52 | 5.39 |
| | (0.06) | (0.04) | (0.02) | (0.02) | (0.36) | (0.37) | (0.54) | (0.42) |
| **Daytime Rate** | 0.18 | 0.18 | 0.13 | 0.13 | 10.07 | 9.47 | 13.94 | 13.31 |
| | (0.07) | (0.07) | (0.02) | (0.02) | (0.67) | (0.63) | (1.36) | (0.85) |
| **Weekend Rate** | 0.11 | 0.11 | 0.09 | 0.87 | 4.00 | 3.81 | 5.57 | 5.31 |
| | (0.06) | (0.04) | (0.02) | (0.02) | (0.61) | (0.56) | (0.75) | (0.59) |
| **Weekday Rate** | 0.17 | 0.17 | 0.14 | 0.14 | 9.88 | 9.26 | 13.89 | 13.39 |
| | (0.04) | (0.06) | (0.02) | (0.02) | (0.70) | (0.67) | (1.41) | (1.02) |
| **Male Rate** | 0.42 | 0.43 | 0.34 | 0.35 | 18.06 | 16.92 | 26.5 | 25.53 |
| | (0.13) | (0.12) | (0.05) | (0.04) | (1.26) | (1.39) | (2.78) | (1.68) |
| **N** | 24 | 24 | 24 | 24 | 24 | 24 | 24 | 24 |

* Pre Reform denotes the period December 2012 to November 2014
† Post Reform denotes the period December 2014 to November 2016

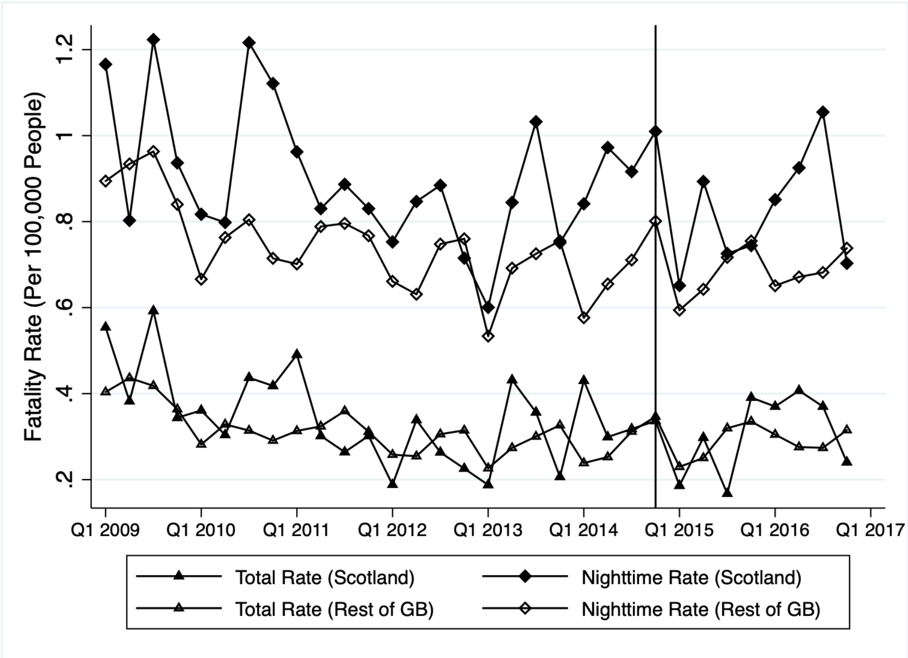
Table (1) Outcome variable means and standard deviations pre-treatment residuals in Scotland and the other regions follow parallel time-trends ("common time-trends assumption"). Figures 1 and 2 show fatality and accident rates over time. They indicate that our main identifying assumption is likely to be met. Indeed, accident rates follow strikingly similar trajectories and show identical seasonal patterns. The data for fatalities are slightly noisier, but the Scotland and England/Wales time series move closely together.

## 3 Results and discussion

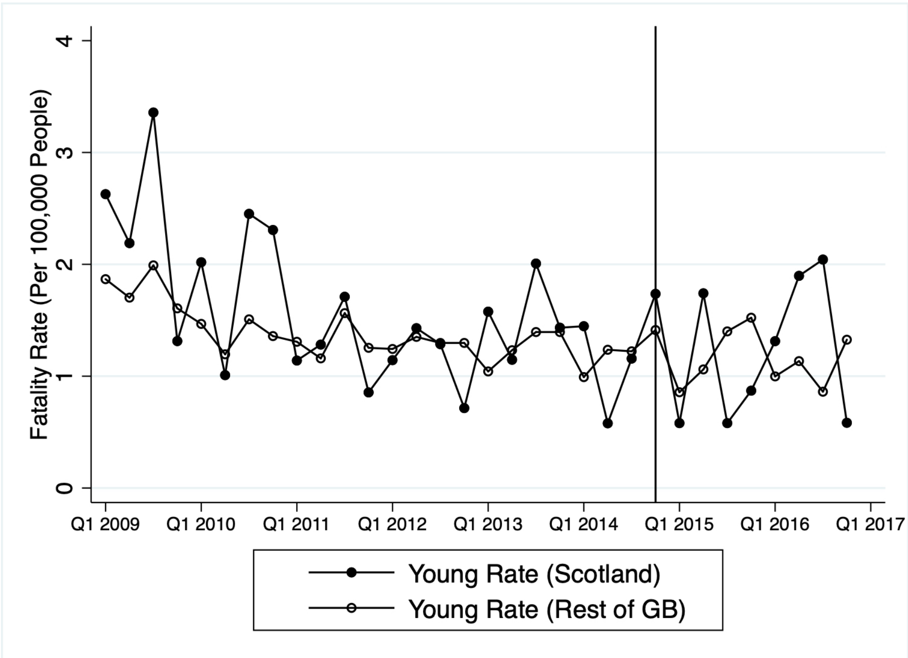
Figures 1 and 2 are consistent with the comparison of means in Section 2. There is little in the way of a break in the series after the introduction of the policy, and even two years later accident and fatality rates remain flat in both Scotland and the rest of Great Britain. Our regression results in Table 2 confirm this. Column (1) of Panel A indicates that lowering the BAC limit has not led to a statistically significant drop in fatalities. Even though the effect is very precisely estimated, the coefficient is not statistically significant at even the 10% level. We can all but rule out a reduction in the fatality rate of more than 0.15 standard deviations. Adding covariates (column 2) does not change this result, and our finding is robust to the inclusion of region-specific time trends (column 3).

In order to explore these results in more detail, we make use of the detailed information we had on each accident. We begin by analysing whether there was an effect on fatalities occurring at different times of the day. Previous studies (e.g. Young and Bielinska-Kwapisz (2006)) have shown that alcohol-related fatalities spike during night-time and weekends[4]. However, we find no statistically significant evidence that the change in BAC limit had an effect on fatality rates in Scotland at night-time (column 4); daytime (column 5); or at weekends (column 6). There is a small negative effect for fatality rates on weekdays (column 7). Anecdotally, this weekday-effect may be driven by changes in attitudes towards post-work alcohol consumption after the introduction of the new BAC limit. A widely held rule of thumb is that one pint of lager - the most common beverage and serving size - would put one just above the new BAC limit while it did not under the old regime. This may, in turn, have prompted some after-work pub-goes to cut back resulting in fewer weekday fatalities. But in any case, this effect is small and not statistically significant at the 1% level (although significant at the 5% level). We also

[4]Night-time is defined as any time after 6pm and before 6am; weekend is defined as any time after 6pm on Friday to 6am on Monday.




(a) Quarterly total and night time fatality rate




(b) Quarterly young fatality rate

Figure (1) Quarterly total, night time and young fatality rate

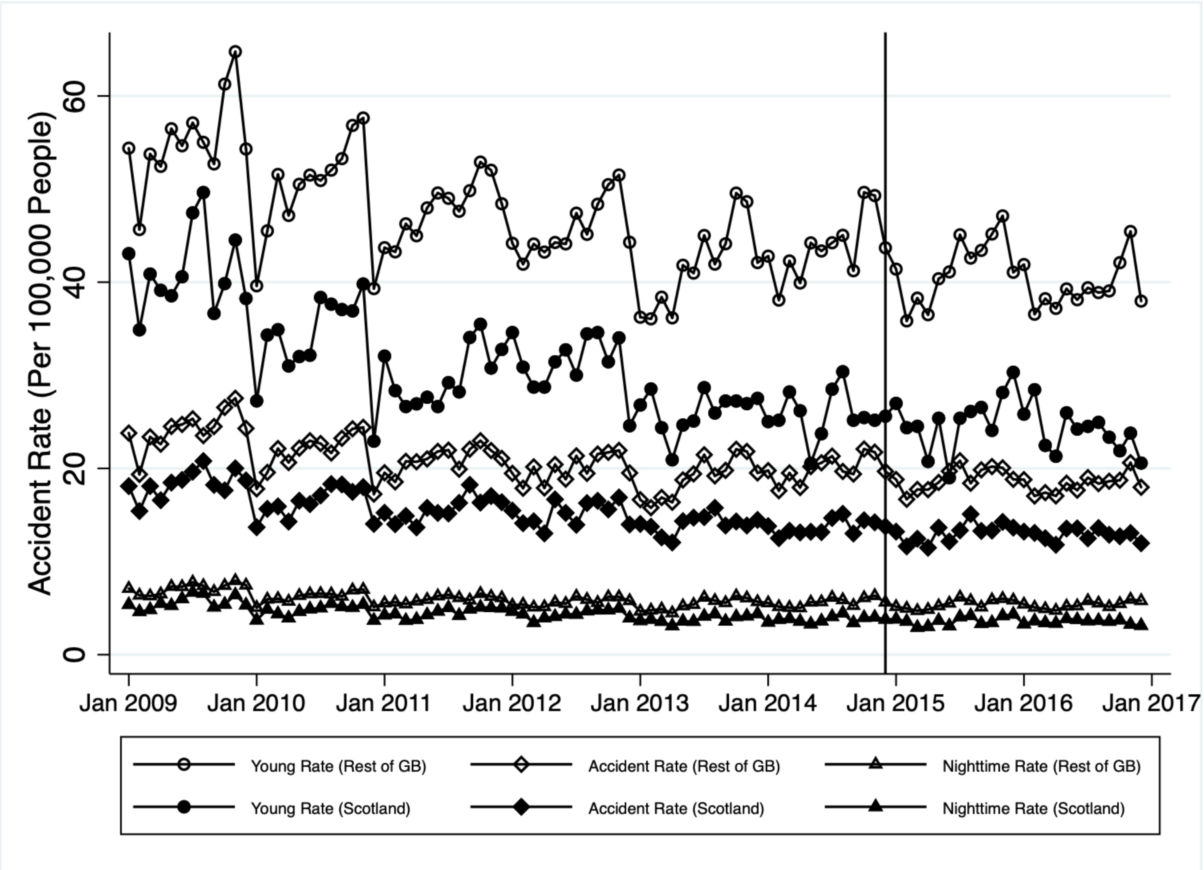



Figure (2) 2009-2016 monthly total, night time and young accident rates considered whether younger drivers (15-25 year olds) or male drivers were more affected by the policy change. Table 1 shows that both groups are at a particularly pronounced risk of getting involved in both fatal and non-fatal accidents. Focusing explicitly on 16-25 year old drivers (column 8) and on male drivers (column 9), we again find no evidence that the BAC limit reduction had an effect on fatality rates among these groups.

Similarly, the policy change has not had any effect on overall accident rates. Column (1) in Panel B of Table 2 indicates that the BAC limit reduction in fact slightly increased the monthly accident rate by 0.18 accidents per 100,000. Given a pre-treatment mean of 13.88, this translates into a 1.3% increase, but of course this effect is not statistically significant. Neither the inclusion of control variables, nor a break-down across drivers' demographic characteristics reveals a drop in accidents. The estimates are also precise enough to rule out any large undetected effects. For instance the upper bound of the 95% confidence interval of the point estimate in column (1) would indicate a drop in the accident rate of about 5.5%, which is small compared to Dee's (2001) finding of a 16.5% drop in fatalities. We therefore interpret our estimates as reasonably precisely estimated zero effects that all but rule out that the policy change led to substantial improvements in road safety. The size of the coefficients and standard errors for fatalities (Panel A) corroborate this interpretation. For instance, the results of column (1) of Panel A show that the upper bound of the 95% confidence interval of the point estimate suggests a drop in the fatality rate by just 0.018 or about 6.4%. It is only for young drivers - admittedly a population that is of particular interest, but crucially not a population that was specifically targeted by the policy change - where our standard errors are too large to rule out substantial reductions.

Another concern is that the BAC reduction may have coincided with more random checks or stronger police presence. Indeed, the policy change was introduced during the festive season during which anti drink-driving traditionally are launched. However, there is no evidence that Scotland saw either fiercer campaigning or a differential increase in enforcement than the rest of GB. Moreover, such a confounding factor would bias our estimates away from zero, i.e. increase the likelihood of erroneously finding an effect. Note that all standard errors account for clustering at the region-level. Calculating p-values using the wild-bootstrap procedure suggested by Cameron et al. (2008) - which may be more appropriate if the number of clusters is small – also does not change the results.

**Panel A: Fatality Rate**

| | (1) Total | (2) Total | (3) Total | (4) Night-time | (5) Daytime | (6) Weekend | (7) Weekday | (8) Young | (9) Male |
|---|---|---|---|---|---|---|---|---|---|
| $Scot_r \times Post_t$ | -0.008 | -0.008 | -0.016 | -0.003 | -0.005 | 0.006 | -0.014** | -0.038 | -0.015 |
| | (0.005) | (0.006) | (0.012) | (0.005) | (0.004) | (0.004) | (0.006) | (0.032) | (0.013) |
| | [0.736] | [0.693] | [0.717] | [0.491] | [0.492] | [0.791] | [0.683] | [0.423] | [0.826] |
| Observations | 1,056 | 1,056 | 1,056 | 1,056 | 1,056 | 1,056 | 1,056 | 1,056 | 1,056 |
| R-squared | 0.394 | 0.397 | 0.4 | 0.263 | 0.327 | 0.326 | 0.281 | 0.267 | 0.368 |
| Region Specific Time Trends | No | No | Yes | No | No | No | No | No | No |
| Covariates | No | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

**Panel B: Accident Rate**

| | (1) Total | (2) Total | (3) Total | (4) Night-time | (5) Daytime | (6) Weekend | (7) Weekday | (8) Young | (9) Male |
|---|---|---|---|---|---|---|---|---|---|
| $Scot_r \times Post_t$ | 0.182 | 0.193 | -0.591 | -0.150 | 0.343 | 0.040 | 0.153 | 0.321 | 0.071 |
| | (0.471) | (0.475) | (1.188) | (0.142) | (0.340) | (0.147) | (0.337) | (0.193) | (0.336) |
| | [0.440] | [0.341] | [0.347] | [0.540] | [0.354] | [0.596] | [0.377] | [0.447] | [0.395] |
| Observations | 1,056 | 1,056 | 1,056 | 1,056 | 1,056 | 1,056 | 1,056 | 1,056 | 1,056 |
| R-squared | 0.655 | 0.656 | 0.753 | 0.715 | 0.629 | 0.667 | 0.664 | 0.661 | 0.678 |
| Region Specific Time Trends | No | No | Yes | No | No | No | No | No | No |
| Covariates | No | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Standard errors in parentheses are heteroscedasticity robust and clustered at the NUTS1 regional level.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$, alternative p-values from cluster wild bootstrap in square brackets
Results correspond to $\beta_3$ in regression equation (1), region and year-month fixed effects are included in all specifications.

Table (2) Difference-in-Differences Regression Results

# 4 Conclusion

We study the effect of reducing blood alcohol content (BAC) limits from 0.08mg to 0.05mg per 100ml of blood. We exploit a natural experiment in Great Britain that was created when one region, Scotland, lowered its BAC limit in 2014 while all other regions kept their BAC limits unchanged. With data that not only cover fatalities but all police-recorded road traffic accidents and that allow us to evaluate this policy change in a homogeneous policy environment, Great Britain offers the ideal setting for this study. Even though our difference-in-differences model is well identified and yields precise estimates, we find little evidence that a reduction of the BAC limit led to a drop in either road traffic accident or fatality rates.

While there may be other reasons to pursue a reduction in the BAC limits, our results suggest that further BAC limit reductions might disappoint policymakers' expectations with respect to improvements in road safety. This is not to say that previous BAC limit reductions have been an ineffective means of preventing traffic fatalities. Instead, our results indicate that the marginal returns to further BAC reductions in terms of road safety are small, which is a result that policy makers should take into account when weighing the costs and benefits of alcohol-control policies.

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