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Marco Francesconi and Jonathan James

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Centre for Economic Policy Research
33 Great Sutton Street, London EC1V 0DX, UK
Tel: +44 (0)20 7183 8801
www.cepr.org

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JEL Classification: I12, I18, D62, K42

Keywords: Driving under the influence, Road collisions, Health, Alcohol, crime

Marco Francesconi - mfranc@essex.ac.uk
University of Essex and CEPR

Jonathan James - j.james@bath.ac.uk
University of Bath

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Marco Francesconi
University of Essex

Jonathan James
University of Bath

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1. Introduction

Motivation — On the 5th of December 2014 Scotland reduced the legal drink drive limit (DDL) from 80 to 50 milligrams per 100 millilitres of blood.[1] The limit in the rest of the United Kingdom (England, Wales, and Northern Ireland) did not change and continues to be at the same level at the time of our analysis. While the Scottish limit decreased, the punishment for being caught and found guilty of driving above it remained unaltered both in Scotland and in the other three UK constituent nations. The first objective of this paper is to estimate the impact of the DDL reduction on road traffic collisions. After assembling many different administrative data sources, some never used before, and using a variety of research designs, we conclude that the reform had no impact on accidents. The second objective of the paper is to understand why this was the case. The unavailability of cheaper alternative means of transportation and weak law enforcement seem to have been the main channels behind the lack of an impact.

Motor vehicle accidents are a major public health problem worldwide. In 2016, there were 1.35 million road traffic deaths globally, with a heavier burden of these deaths falling on men and the young (World Health Organization, 2018). Even though there have been large reductions in automobile crashes over recent decades in the UK, they continue to be a considerable burden on health. For instance, in 2018, over 160,000 casualties from road traffic collisions were reported, consistently representing the second or third leading cause of death among Britons aged 5–34 in the last 30 years. Driving under the influence of alcohol is the main risk factor for road crashes everywhere, and a dose-response relation is systematically observed between blood alcohol concentration and road traffic accidents (Levitt and Porter, 2001; Francesconi and James, 2019). Medical research from field and laboratory studies suggests that lowering the BAC level from 0.08 to 0.05 would significantly curtail risk taking among drivers and stop people from being killed as a result (Fell and Voas, 2006; Breitmeier et al., 2007; Phillips and Brewer, 2011; van Dyke and Fillmore, 2017; Fell and Scherer, 2017).

Limiting BAC for drivers, therefore, has become a key policy tool used by governments across the world in their attempt to save lives on the road. Having a limit of 0.05 BAC is one of the criteria for best practice set out by the World Health Organization (WHO). To date, 2.3 billion people in 45 countries have drink drive laws that align with WHO best practice. In the US, since 2017, all states have a standard limit of 0.08 BAC (except Utah, which lowered it to 0.05 at the end of 2018), and have adopted policies of lower limits for young or novice drivers. An overall abatement to 0.05 BAC has been discussed but not yet agreed (e.g., National Transportation Safety Board, 2013). In 2001, the European Commission recommended that all member states should adopt a legal maximum limit of 0.05 BAC or lower, for drivers and riders of all motorized vehicles (Official Journal of the European Communities, 2001).[2] In the UK, the government-

[1] As it is standard in the literature, we shall refer to such measures in terms of blood alcohol concentration (BAC) expressed in grams of alcohol per deciliter of blood, that is, 0.08 BAC and 0.05 BAC, respectively. Alternatively, these same two figures also correspond, respectively, to 35 micrograms (μg) of alcohol per 100 millilitres (ml) of breath and 22 μg/100 ml, or to 107 milligrams (mg)/100 ml and 67 mg/100 ml of alcohol in urine.

[2] This was followed up ten years later by the European Parliament asking the Commission to prepare EU-

commissioned *North Review* on the legal framework of drink and drug driving concluded with the recommendation to cut the legal maximum BAC from 0.08 to 0.05 (North, 2010). With the 2014 DDL reform, Scotland adopted this recommendation, with the explicit intent to make roads safer and save lives. As of July 2020 among all European countries, only England, Wales, and Northern Ireland have a standard drink drive limit greater than the recommended 0.05 BAC.[3]

What does a reduction from 0.08 to 0.05 BAC mean in practice for an average consumer planning to drive? Although this is difficult to pin down with precision, most of the information available to the public suggests that a healthy adult man of mean weight can consume two UK pints of 5% alcohol-by-volume (ABV) beer, or two large glasses of 13% ABV wine, over a meal and be below the 0.08 BAC limit.[4] The stricter 0.05 BAC threshold implies a reduction to one pint of beer or one large glass of wine. For a healthy adult woman of mean weight, all figures should be roughly halved.

One critical implication of this reduction is for the alcohol industry, which plays a vital role in the Scottish economy and contribute approximately 4% to total Scottish GDP on average since 2000 (O'Connor, 2018).[5] Scotland's case is important also because of the country's historically high mortality rates across all ages compared to the rest of Europe and most other industrialized nations. For women and men aged 15–74, Scotland has recorded the highest all-cause age-standardized mortality rates since the early 1980s, including liver disease and cirrhosis, which are specific causes of death typically associated with heavy drinking (Whyte and Ajetunmobi, 2012). Consequently, life expectancy in Scotland, whether at birth or at age 65, has been among the lowest among OECD countries, with gaps of at least 4–5 years with respect to most of the other advanced economies (OECD, 2019; National Records of Scotland, 2019). Some of the arguments leading up to the 2014 reform were motivated by this hard pre-existing reality (Granville and Mulholland, 2013; Scottish Health Action on Alcohol Problems, 2014).

Our contributions — The explicit goal of the lower drink drive limit was to save lives, improving safety on the roads, reducing road traffic accidents and deaths, and lowering convictions.[6] Economic theory suggests that, ceteris paribus, if the actual or perceived probability of convic-

wide proposals for a harmonised blood alcohol limit and a BAC limit of zero for novice and professional drivers (European Transport Safety Council, 2016).

[3]See <https://etsc.eu/blood-alcohol-content-bac-drink-driving-limits-across-europe/>.

[4]Among others, see North (2010) and <https://www.alcohol.org/bac-calculator/>. It is worth mentioning that one UK pint is equivalent to 568ml. One large glass refers to 250ml of wine. A different convention for measuring ethanol volume in the US is a "standard drink", which corresponds to 355ml of beer (156ml of wine).

[5]On the 1st of May 2018, Scotland passed another ethanol related reform, which introduced a minimum unit price (MUP) of alcohol at 50 pence per unit. The MUP was initially supported by the Scottish Parliament through the Alcohol Act 2012. But it was legally challenged by the Scotch Whisky Association and referred to the European Union Court of Justice. The response in December 2015 (after the 2014 DDL reform) required Scottish judges to consider whether alternative tax policies were ineffective in protecting public health. After almost two years, on 15 November 2017, the Supreme Court of the United Kingdom rejected the Scotch Whisky Association's case, arguing that minimum pricing was a proportionate means of achieving a legitimate aim. This reform falls outside our period of interest, and therefore is not part of our analysis.

[6]For official statements from the Justice Secretary, the Assistant Chief Constable of Police Scotland, and the Director of Road Safety Scotland, see <https://www.wired-gov.net/wg/news.nsf/articles/Lower+drink+drive+limit+in+Scotland+04122014102005?open>. See also the discussion in the next section.

tion goes up (because the DDL has been lowered) or the probability of punishment increases, we should expect to see a *reduction* in drink drive offenses and, because of the BAC-collision relationship illustrated by the empirical literature, a reduction in motor vehicle accident rates (Becker, 1968; Ehrlich, 1973; Becker and Murphy, 1988; Sah, 1991; Levitt and Porter, 2001; Hansen 2015). In the DDL context, alternative models based on behavioral insights may lead to similar predictions on traffic collisions, even though they could identify different mechanisms.[7] The BAC limits, however, are set at levels low enough that might not require us to invoke cognitive impairment, limited self-control, weak wills, or the operation of visceral influences on drink driving. Nevertheless, testing one theory against another goes beyond the scope of this paper. Rather, we aim to provide a comprehensive evaluation of the reform on the one hand and a unifying, rigorous understanding of the channels behind its effects on the other. For this latter aspect of the analysis, we use intuitions from both standard theory and behavioral models.

To evaluate the impact of the reform, we rely on highly detailed geographical data and new administrative information defined at the level of local authorities, of which there are 347 in England and Wales and 31 in Scotland.[8] In the attempt to address the issue of noncommon trends between Scotland and the rest of Britain as persuasively as possible, limit the role played by unobserved local differences, and reduce the scope for omitted variables bias, we compare the results from three approaches, namely, models based on difference-in-differences, spatial regression discontinuity design, and synthetic control methods. We supplement both the difference-in-difference analysis using a linear panel event-study design following Freyaldenhoven, Hansen, and Shapiro (2019) and the synthetic control approach combining matching and synthetic control estimators through model averaging as proposed by Kellogg et al. (2019). All estimates reveal that the lower Scottish limit had no impact on any type of road accident, from fatal crashes to collisions involving just slight injuries. For the first time for Britain, we have access to data with alcohol involvement and confirm the null result also for drink drive accidents.

This evidence holds true for various subgroups of the population (e.g., young men), and whether we consider nights, weekends, or multiple vehicle crashes. It is also robust to different definitions of the outcome variable, different functional forms used in estimation, randomization inference, and alternative definitions of drink drive collisions.

Given the relatively small BAC range over which the reform could leverage, we address issues of statistical power by looking at the fraction of accidents that are in the 0.05–0.08 BAC band and by estimating a reduced form version of the supply of offenses, which links BAC levels to the number of road traffic violations and accidents. We find there are no power issues, that is, there are enough accidents in the relevant BAC range to detect statistically significant effects. Nevertheless, the supply-of-offenses analysis reveals only a small alcohol intake-collision

[7] These include cognitive limits on decision making (George, Rogers, and Duka, 2005), lack of self-control (Bettman, Luce, and Payne, 1998), visceral factors affecting judgement (Loewenstein, 1996), social pressure to conform (DellaVigna, 2009), and present-focused preferences (Ericson and Laibson, 2019). For an overview of models in the drink drive setting, see Sloan, Eldred, and Xu (2014).

[8] Since most the data we have do not cover Northern Ireland, this constituent nation of the UK is not part of our analysis. We thus focus on Britain only.

elasticity over the same BAC interval. This implies that the reform did not have much scope for a sizeable impact ceteris paribus. The pre-existing DDL was already sufficiently low that further reductions in motor vehicle accidents could not be readily achieved, unless some other factors were triggered (e.g., stricter enforcement).

Two other papers examine the 2014 BAC reduction in Scotland on road accidents estimating difference-in-difference models. Haghpanahan et al. (2019) use weekly motor vehicle accident rates calculated at the *country* level (i.e., one rate for Scotland and one for England and Wales), while Cooper, Gehrsitz, and McIntyre (2020) compare the whole of Scotland to ten other large regions in England and Wales. Like ours, neither study finds evidence that the change in the law led to lower road accident rates. One concern with both studies is the high level of geographic aggregation, which may overlook time-varying differences within each constituent nation that are important for identification, such as differences in economic conditions. Our work addresses this issue by using a much finer spatial disaggregation whenever possible. Another concern is that there might be unobservables (e.g., alcohol abuse) that affect accident rates and, possibly, even the introduction of the DDL reform, which cannot be accounted for by standard difference-in-difference models. This is the reason why we use multiple methods that rely on different identifying assumptions. Finally, unlike the two previous papers, we examine mechanisms and potential spillovers.

In fact, because of our zero-effect results, examining mechanisms is all the more important. This can allow us not only to understand behavior better but also to inform future policy interventions on how to be more effective. To guide the interpretation as to why the DDL reduction to 0.05 BAC was ineffective, we go back to the framework proposed by Becker (1968) and Ehrlich (1973 and 1996), in which potential lawbreakers decide whether to engage in drink driving by carrying out a cost-benefit calculation under uncertainty. Individuals assess whether the expected benefit from drink driving, accounting for the probability of being caught, outweighs the expected cost. In this setup, drink driving is affected by both direct and indirect economic incentives (Draca and Machin, 2015). Among the direct incentives, we consider alternatives to driving under the influence, and explore changes in the availability of alternative means of transportation (e.g., taxis and buses) and their fares. A key indirect mechanism is through deterrence and incapacitation effects. Although the reform did not change the punishment for drink driving, we examine whether the lower limit was accompanied by changes in enforcement, specifically police numbers and breath testing at the roadside, including tests unrelated to road collisions, as well as drink drive arrests and convictions unrelated to traffic accidents. We find that alternative means of transportation were neither more available nor cheaper, and police enforcement was weak. Both channels therefore contribute to the lack of an impact of the reform.

Building on the insights from both the conventional economic model of crime and behavioral models, we then explore the possibility of unintended consequences of the stricter limit. We find the reform led to a greater anti-drink driving sentiment among the public, although this was not enough to induce people to scale back their alcohol consumption or reduce own vehicle usage. There was no increase, but also no abatement, in other types of offenses and criminal activities, including speed limit violations, illegal drug-related crimes, serious assaults, and sexual offenses. We also find no appreciable price and quantity responses from the alcohol and automobile industries.

Related Literature — There is a wide economic literature on the impact of various alcohol policies on motor vehicle collisions, with a particular emphasis on traffic fatalities.[9] One strand focuses on the impact of alcohol taxes with findings suggesting a negative association (e.g., Chaloupka et al., 1993; Ruhm, 1996). Another takes advantage of the changing legal status of drinking by age. For the United States, Carpenter and Dobkin (2009, 2011, and 2015) find increases in mortality around the age when drinking becomes legal. This increase seems to be driven, in part, by an increase in fatal crashes which go up by 15% at the age of 21.[10] Using the census of judicial records on criminal charges filed in Oregon courts, Hansen and Waddell (2018) find that crime increases at age 21, especially in the case of assaults lacking in premeditation, alcohol-related offenses, and drink driving.[11] This evidence is suggestive more of the impact of heavy alcohol use than of the impact on margins that are closer to legal drinking levels, which DDLs attempt to influence.

Other studies exploit variation in the availability of alcohol through restrictions (or relaxations) on the times of the day or days of the week when alcohol can be sold. Liberalization of Sunday alcohol laws (also known as "blue laws") have been shown to have had mixed effects on traffic fatalities (McMillan and Lapham, 2006; Lovenheim and Steefel, 2011).[12] Stehr (2010) exploits changes of blue laws in 14 states using data from 48 states from 1995 to 2008 and concludes that the repeal led to an increase in highway crash fatalities only in New Mexico. Using variation in the legalization of Sunday packaged alcohol sales across Virginia, Heaton (2012) finds no impact on arrests for driving under the influence. In 2005, England and Wales liberalized bar opening hours. Green, Heywood, and Navarro (2014) find that this change in on-trade availability led to a decrease in road accidents, with the fall concentrated among the young and at times when the policy was expected to have most bite (i.e., at the weekends and during more popular drinking hours). Restrictions of liquor availability in Brazil, in contrast, led to a decline in deaths by car accidents (Biderman et al., 2010).

[9] An earlier review of the empirical evidence on drink driving is given by Benson, Rasmussen, and Mast (1999). Sloan (2020) provides a comprehensive update.

[10] A similar effect of around 17% is found for Canada, this being driven mainly by men (Carpenter, Dobkin, and Warman, 2016).

[11] These results are broadly confirmed by Fletcher (2019), who finds large detrimental effects of alcohol access on drink driving, violence, and other risky behaviors, especially among men in the Add Health data. Examining the reduction in legal drinking age from 20 to 18 years in New Zealand, Boes and Stillman (2013) document an increase in alcohol related hospitalizations, but no increase in alcohol related road accidents. Lindo, Siminski, and Yerokhin (2016) find similar evidence for New South Wales.

[12] Anderson, Crost, and Rees (2018) show that greater alcohol availability (measured by the increase in the number of establishments licensed to sell alcohol by the drink in the state of Kansas) is associated with a 3 to 5% increase in violent crime, such as murder, rape, and robbery. Road accidents and drink driving, however, are not included in their research. Seim and Waldfogel (2013) provide additional analysis of how governments attempt to curb problematic alcohol consumption through restricting availability.

Besides the already mentioned studies on the 2014 DDL reform in Scotland (Haghpanahan et al., 2019; Cooper, Gehrsitz, and McIntyre, 2020), a small strand of research focuses on the effect of BAC limits on road accidents, mainly in the United States. Carpenter (2004) analyzes the impact of zero-tolerance laws for the young, which set stricter BAC limits for individuals under age 21. The results indicate these laws led to a reduction in heavy drinking (for men), but not drink driving. Among the closest papers to ours, Dee (2001) examines the effect of lowering DDLs for all drivers. Using variation across 48 states from 1982 to 1998, he finds that lowering the limit to 0.08 BAC reduced fatal accident rates by 7.2%, implying roughly 1,200 lives saved annually. Eisenberg (2003) confirms this result, estimating that a decline in the legal limit from 0.10 to 0.08 BAC reduced fatal crash rates by 3.1% percent, although it might take a few years for the effect to be observed. Using data on 15 European countries observed between 1991 and 2003 and taking advantage of the reduction in the BAC level down to 0.05, Albalate (2008) finds evidence of a decrease in fatal road accident rates for young drivers in urban areas, but no overall effect.

The remainder of the paper is organized as follows. Section 2 outlines the background surrounding the introduction of the 2014 Scottish reform and discusses some of the key factors that affect BAC levels through the way ethanol is absorbed and metabolized. Section 3 describes the data and methods used for the policy evaluation. Section 4 presents the benchmark estimates of the evaluation, checks for heterogenous effects, and shows the results from a broad set of sensitivity exercises and from drink drive crimes unrelated to road accidents. Section 5 discusses issues of statistical power and the results obtained from the estimation of a supply of offenses, which links motor vehicle violations and accidents to BAC levels. Section 6 explores the two main mechanisms suggested by a market model of crime, i.e., alternatives to drink driving and police enforcement of the new limit. Section 7 investigates whether the reform had unintended consequences on multiple domains, such as public attitudes toward drink driving, alcohol consumption, and own vehicle usage, or if it generated undesired spillovers to other crimes and offenses. Section 8 summarizes. Supplementary material on the data and additional results are in the Online Appendix.

2. Background

The legal limit of 80mg of alcohol per 100ml of blood was set out in the 1967 British Road Safety Act and had not changed in over 45 years.[13] On the 18th of November 2014 the Scottish Parliament voted unanimously to reduce the legal drink driving limit in Scotland, lower than elsewhere in the UK. The new law, which brought Scotland in line with other European countries,

[13]Identifying the causes of this change is challenging. It should be reminded, however, that Scotland could not legislate independently of the rest of the UK before the 1998 Scottish devolution. Since then, the first time Scotland could change DDL was not until 2012 with the Scotland Act 2012, which gave the Scottish Parliament legal powers to prescribe drink drive limits. The historically high mortality rates linked to alcohol consumption, which we mention in the Introduction, might have played a role in urging Scottish law makers to introduce the new limit in 2014, following the recommendation of the 2010 *North Review*.

came into force on the 5th of December 2014. The reduction from 0.08 to 0.05 BAC was not accompanied by any change in the punishment (measured in terms of fines, penalty points, driving disqualifications, and jail sentences) associated with breaking the law.[14]

Aside from the amount of alcohol consumed, the determination of one's BAC is a function of a number of pharmacokinetic factors, including gender, weight, age, absorption, and speed of consumption, which cannot be easily accounted for by drinkers (Baraona et al., 2001; Koob, Arends, and Le Moal, 2015). For instance, faster alcohol consumption is typically associated with quicker rises in BAC. All else equal, BAC among women is higher than among men, since women on average have less water in the body and BAC is proportional to the total body water content. For a given level of alcohol consumed, high-weight people show smaller BAC than their low-weight counterparts, since they have more water content in the body. Similarly, fatter individuals may experience higher BAC, because fatty tissues do not absorb alcohol well. Older people take longer to metabolise alcohol than younger people. Food and medicines can also lead to differences in alcohol absorption rates. Finally, the rates of metabolizing alcohol differ from person to person. On average, 10ml of alcohol is metabolised per hour. However, heavy drinkers can metabolize alcohol faster than light drinkers and thus, for a given amount of alcohol over the same amount of time, they will have lower BACs.

The reduction to 0.05 BAC obviously cuts the amount of alcohol that can be consumed. But it is unclear by how much. Given the factors discussed above, it is hard to determine precisely the level of BAC for a given amount of alcohol consumed. In the Appendix, we show this relationship distinguishing intake (defined using either the UK convention of pints or the US convention of standard drinks) over one- and two-hour periods for an average man aged 40 weighing 84kg and an average woman aged 40 weighing 70kg. For the man, drinking two pints (or, equivalently, four standard drinks) of 5% alcohol-by-volume beer over one hour would be under the legal limit in England and Wales but over the lower default in Scotland. For the woman, one pint would put her very close to the new Scottish limit, while two pints would take her clearly over both standards. These figures are in line with the *North Review*, which states that the 0.05 BAC limit "would still allow the responsible driver who wishes to enjoy a drink to accompany their pub meal or have a glass of wine or a pint of beer to do so without being in danger of breaking the law" (North, 2010, p. 7 and p. 96). It is nonetheless very difficult for people to judge the level of alcohol in their body accurately.

The new law in Scotland was accompanied with an intense TV advertising campaign in the weeks around the introduction of the reform, aimed at raising awareness of the new law for all Scottish citizens and particularly those living near the border. ITV Borders, which broadcasts to the south of Scotland and the north of England, ran adverts starting from 17 November 2014 up to 2 January 2015.[15] This was backed by a coordinated public information campaign on all

[14] In Scotland, as in the rest of the UK (and other countries), there is a menu of punishments, with sanctions increasing with the severity of the offense and designed in such a way to deter drunk driving. See <https://www.gov.uk/drink-driving-penalties>.

[15] The campaign message was "The best advice is none, when it comes to drinking and driving." There is some anecdotal evidence suggesting that drivers might have been affected by the campaign with increased sales

other TV channels, radio stations, and social media, by electronic road signs across Scotland and on key border roads between England and Scotland, and by posters and digital screens in all Scottish public venues, such as train stations, main airports, medical centers, supermarkets, tourist information agencies, car hire companies, petrol pumps and garages, and all alcohol retailers.[16]

With a contribution in excess of 3.5% to the Scottish GDP, the alcohol industry plays a key role in the country's economy. Soon after the introduction of the lower limit, both businesses and commentators claimed the reform might have depressed alcohol consumption in pubs and restaurants and could have had an effect on the industry and even the slower economic activity registered in Scotland in the first quarter of 2015 (e.g., Green, 2015; Wright, 2015). It is worth stressing that the new DDL was introduced in the aftermath of the global financial crisis with the UK government pursuing economic austerity and severely cutting public service spending in real terms (Crawford and Zaranko, 2019).

If one deems the Scottish environment — defined for instance in terms of pre-reform alcohol consumption levels and trends, road traffic accident rates, crash fatalities, and legal DDL — drastically different from all other environments, the results we describe in the next sections will not be generalizable to other countries. If instead we have good reasons to believe it to be sufficiently similar, then one can justify the transportability of our findings to new targets. These may include countries like the US, Canada, Singapore, and Mexico, as well as England and Wales, where the patterns of either per capita alcohol intake, or accident rates, or both, are similar to Scotland's, and the drink drive limit is currently at 0.08 BAC.

3. Data and Methods

Data Sources — For the evaluation of the 2014 DDL reform, we use several data sources.[17] The first, which serves as the main input for our dependent variables, is the Road Accidents Data (RAD), the British official administrative source for all motor vehicle collisions reported to the police and recorded using the STATS19 accident reporting form. The RAD are collected by police officers on behalf of the Department for Transport (DfT) whenever an accident involves at least one personal injury, however minor this might be. We use all monthly records from January 2009 to December 2016 on over 1.2 million accidents. Each record contains details about the accident and the individuals involved, including their age and sex, the exact time and location of the accident, and its severity, and this in turn is distinguished into fatal, serious, and

of breathalyzers in the areas north and south of the border within the first month of reform enactment (see https://www.itv.com/news/border/update/2014-12-30/breathalyser-sales-up-in-border-region/). Although this could be a mechanism responsible for the lack of an impact of the reform (because drivers may reduce the uncertainty about their own BAC levels), the accuracy of breathalyzers is suspect, and we cannot find any reliable official statistics on breathalyzers purchases.

[16] Just the two central train stations in Edinburgh and Glasgow see an average footfall of about one million commuters and visitors per week. It is noteworthy therefore that, in spite of the heavyweight media campaign, we cannot detect any impact of the reform.

[17] Others will be described in the following sections. For additional details on all data sources used, see the Online Appendix.

slight.[18]

The data also contain information on alcohol involvement, which has never been used before. This information was collected from surviving drivers or riders, who were breath tested at the roadside. The level of alcohol in the breath is not given, but we know whether the test was negative, positive, or whether the driver(s) refused to take the test. A drink drive accident is defined as an incident reported on a public road in which someone was killed or injured (even slightly) and at least one of the drivers involved met one of the following criteria: (i) failed a roadside breath test by registering above 35μg of alcohol per 100ml of breath in England and Wales, or 22μg/100ml in Scotland from December 2014 onwards (and above 35μg/100ml before December 2014 in the whole of Britain); or (ii) refused to give a breath test specimen when requested by the police, except when incapable of doing so for medical reasons.[19]

The cross-sectional unit of observation is the local authority district (or local council). In Britain there are 378 local authorities in total, 347 in England and Wales and 31 in Scotland. Our outcomes are accident rates (either total, by level of severity, or drink drive), defined as the number of accidents for each category in a given local authority and a given month per 1,000 vehicles registered in the same local authority.[20]

Figure 1 shows the monthly accident rates observed over the sample period by country (i.e., Scotland versus England and Wales) by accident type averaged over all local councils for all motor vehicle collisions. We stress two points. First, Scotland experienced lower crash rates than the rest of Britain. This is especially clear for all accidents and slight injury accidents, but it is not obvious in the case of fatal crashes. Second, the introduction of the reform does not seem to be followed by a noticeable slowdown in collision rates in Scotland, for all accidents together and each of the four specific types. These two observations suggest that the DDL reform took place in an environment with already lower accident rates where the stricter limit might have had no impact. Accounting for seasonality effects yields the same results.

Road traffic collisions are likely to be correlated with a variety of factors other than BAC, which we control for in the analysis. These are: (a) weather conditions, proxied with the monthly regional average temperature range, i.e., the difference between the maximum and

[18]Fatal accidents involve the death of at least one individual. Serious accidents are those in which at least one individual is seriously injured but no person is killed. A serious injury occurs when at least one individual either is hospitalized as in-patient or, even if not detained in hospital, suffers from a series of injuries including fractures, concussions, internal injuries, crushings, burns, and severe cuts. A slight injury is an injury of a minor character, such as sprains, neck whiplashes, bruises, minor cuts, and all other injuries that do not require medical treatment. The definition of a crash as serious or slight is first recorded by the police on the basis of information available within a short time of the accident. This information is then passed to the DfT for final checking and analysis. For more information, see <https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment-data/file/743853/reported-road-casualties-gb-notes-definitions.pdf>.

[19]In supplementary checks, we complement this analysis using information on drink drive accidents adjusted by the DfT for underreporting due to death and additional data from coroners in England and Wales and public prosecutors in Scotland. These data, however, have a greater level of aggregation, in terms of accident time and location. See Section 4.

[20]The information on the time series of the number of vehicles by local council comes from the DfT. As discussed in the next section, we also use other definitions of rates based on different populations at risk, such as the entire population and road availability.

minimum temperatures in degree Celsius, recorded in each UK climate region (obtained from the Metereological Office);[21] (b) road congestion, which is proxied by council-level population density and road length in kilometers (obtained respectively from the Office for National Statistics (ONS) and DfT);[22] (c) socioeconomic status of the population in each local authority, which is measured in terms of four separate domains, that is, the proportion of residents aged 16 or more with no educational qualification (from the 2011 Census), the proportion of individuals with bad or very bad health (2011 Census), the median total hours worked and the fraction of residents aged 16–64 claiming Job Seeker's Allowance (both obtained from NOMIS, the ONS labor market statistics); and (d) the availability of alcohol, proxied by the total number of licensed alcohol premises in each local council (obtained from the UK Department for Digital, Culture, Media and Sport, the Home Office, and the Scottish Government).

Summary statistics of the pre-reform outcomes and explanatory variables are reported in Table 1, where we distinguish between treatment and control groups for the three main statistical methods used in the analysis (see below). Confirming the evidence shown in Figure 1, monthly accident rates were on average lower in Scottish districts (0.27 versus 0.40 per 1,000 registered vehicles), mainly due to smaller value for slight accident rates (0.22 versus 0.34). However, differences in outcomes are virtually eliminated when looking at the subsamples used with the synthetic control design. Significant differences between Scotland and their English and Welsh counterparts emerge also for nearly all the explanatory variables, irrespective of the distance from the border. For instance, Scottish local councils had a lower population density and a greater fraction of residents with no educational qualifications, working fewer hours, in bad health, and claiming unemployment benefits. Again, most of such differences shrink substantially when we consider the subsamples selected by the synthetic control approach.

Methods — Before describing the research designs used in the empirical analysis, it is instructive to outline the simple framework that guides our interpretation. We start with the observation that a stricter DDL is expected to lead to an increase (either actual or perceived) in the probability of conviction (Becker, 1968). This would imply lower alcohol consumption, which in turn would translate into lower drink drive accident rates and more generally lower road traffic collisions (Levitt and Porter, 2001; Haghpanahan et al., 2019). If we fail to find an impact on motor vehicle crashes, one simple explanation is that drivers do not reduce their alcohol intake before driving, i.e., there is no behavioral change in consumption. If this were the case, we would expect to observe no impact on overall road accident rates and a mechanical *increase* in drink driving accidents (those with a positive or refused breath test). This is simply due to the change in the classification of what constitutes a drink drive collision. Now, the lack of effect (or the lack of increase in the case of drink drive crashes), however, may arise for several reasons. One

[21] We performed the analysis also using separately minimum and maximum temperatures as well as the monthly amount of rainfall (in millimetres), the number of days with rainfall greater than 1 mm, the number of days in which air frost was recorded, and the total monthly number of hours of sunshine. Since most of the results were virtually identical to those presented below, we opted for a more parsimonious specification.

[22] Density is defined as the mid-year population estimate for individuals aged 17 (the age at which individuals can start driving in the UK) or more divided by the local authority area measured in hectares.

is that drinkers change their driving strategies (e.g., they drive less or they ride as passengers in sober friends' vehicles). Another possibility is that drinkers substitute away from their own and others' vehicles and use alternative means of transportation (e.g., taxis or buses). Yet another channel is that policing is less effective in detecting drink drive offenses after the reform. We will analyze each of these potential mechanisms. A final explanation may be related to the share of drivers at risk between the 0.05 and 0.08 BAC levels around the policy intervention, which could be too small to yield detectable (significant) effects. We shall also address this possibility.

Our main empirical goal is to evaluate whether the Scottish 0.05 BAC law was effective in saving lives on the road. To this end we employ three separate (but related) statistical designs. The first is a standard difference-in-difference (DD) model. Letting y_{cm} be the road accident rate in local authority c in month m, S_c an indicator variable equal to 1 if local council c is in Scotland and 0 otherwise, and $\mathbb{I}(z)$ a function indicating that the event z occurs, the DD model is given by

$$y_{cm} = \alpha_0 + \alpha_1 S_c + \alpha_2 \mathbb{I}(m \geq \tau) + \beta S_c \times \mathbb{I}(m \geq \tau) + \psi(t) + \theta_c + \mathbf{X}'_{cm}\gamma + \varepsilon_{cm}, \tag{1}$$

where τ coincides with the month-year when the reform was introduced (December 2014), $\psi(t)$ denotes time fixed effects, θ_c refers to local council fixed effects, and $\mathbf{X}_{cm}$ is a vector of possibly time varying characteristics at the local authority level that can affect accidents, including the monthly regional average temperature range, population density, road length, the socioeconomic status of the population, and the number of alcohol premises. To account for pre-reform trend differences and seasonality in accident rates, we allow the $\psi(t)$ function to include both group-specific linear month-year trends and group-specific month dummies.[23] Finally, we consider five different outcomes, that is, all types of collisions, crashes by severity (i.e., fatal, serious, and slight accidents), and drink drive accidents.

As discussed in the Introduction and illustrated in Figure 1, the trends in Scottish road accident rates were different from the trends in the rest of Britain, even before the enactment of the new DDL in December 2014. As an alternative way to account for pre-reform trends, we therefore supplement the DD approach using the linear panel event-study design suggested by Freyaldenhoven, Hansen, and Shapiro (2019). This model allows for unobserved confounders to be correlated with both the outcome variables and the 2014 reform.

In the second approach, we take into account the fact that the differences in accident rates and their determinants between Scotland on the one hand and England and Wales on the other may be driven by unobserved local (geographic) differences. In order to address this potential concern, we employ a spatial regression discontinuity (henceforth, spatial RD) design framework combined with the previous DD approach. The idea is to give more weight to observations that are closer to the Scottish-English border versus those farther away. Besides a better alignment

[23] Because of the long pre-reform time frame, we also experimented with a full set of month-year dummies rather than separate month dummies and linear group-specific trends. We also repeated the analysis adding quadratic trends. In both cases, we find results that are qualitatively similar to those reported below and are thus not reported. They can be obtained upon request.

in weather conditions, which are controlled for in (1), such a comparison is likely to pick up unobserved or hard-to-measure cultural similarities between neighboring Scottish and English regions, such as food and drink norms, recreational habits, and attitudes toward the law. As in other spatial RD applications, the running variable is the distance from the border, specifically the Euclidean distance between the centroid of each local authority and the border (e.g., Lalive 2008). Following Gelman and Imbens (2019), we estimate a local linear RD polynomial which controls linearly for distance from the border and weights local authorities by proximity to the border using a triangular kernel. In particular, for the same five outcomes mentioned above, we estimate

$$\begin{aligned} y_{cmb} &= \alpha_0 + \alpha_1 S_c + \alpha_2 \mathbb{I}(m \geq \tau) + \beta S_c \times \mathbb{I}(m \geq \tau) \\ &\quad + \delta_1 \text{Distance}_c + \delta_2 \text{Distance}_c \times S_c + \psi(t) + \theta_c + \mathbf{X}'_{cmb}\gamma + \epsilon_{cmb}, \end{aligned} \tag{2}$$

where Distance_c is distance from the Scottish-English border, with English distances taking negative values. The weights we use are equal to $\max\{h - |\text{Distance}_c|\}$, where h denotes the bandwidth of local authorities around the border. We will present results for $h = \{200\text{km}, 100\text{km}, 50\text{km}\}$.

To further contain the scope for omitted variable bias and increase the similarity between treatment and control local councils, the third design follows the synthetic control method introduced by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010). With this approach, we weight local authorities in the control group to construct a synthetic counterfactual that replicates the basic predictors of accidents for Scottish local authorities before the 2014 DDL reform (see also Abadie, Diamond, and Hainmueller [2015]).

Adjusting the previous notation to Rubin's (1974) potential outcome framework, we define y^1_{cm} as the accident rate in month m if the local council c is in Scotland and y^0_{cm} the corresponding outcome if the local authority is not in Scotland, so that the treatment effect of the reform is given by $\beta_{cm} = y^1_{cm} - y^0_{cm}$. The synthetic control estimator compares the outcome in the treated region (Scotland) averaged over all local councils, y^1_m, to a weighted average of the outcome over all local authorities in the control group, that is:

$$\widehat{\beta}_m = y^1_m - \sum_{c \in C} \omega_c y^0_{cm}, \tag{3}$$

where $\omega_c \geq 0$ is the weight attached to each local authority c in the control group C. Since treated and control units are observed in different states after the introduction of the reform at month τ (i.e., with and without the 0.05 BAC law, respectively), (3) becomes

$$\widehat{\beta}_m = \beta_m + \Big(y^0_m - \sum_{c \in C} \omega_c y^0_{cm}\Big), \quad \text{for all } m \geq \tau. \tag{4}$$

The accuracy of this approach therefore relies on minimizing the difference in parentheses in (4). A way to achieve this is to minimize the difference between treated and control local councils over the pre-reform period, when none of them was exposed to the reform. As long as the weights reflect features that do not change in the absence of the DDL reduction, the synthetic control approximates the (unobserved) counterfactual evolution of the potential outcome y_m^0 from τ onwards.[24] To compute both the weights and the mean squared prediction error needed in the procedure, we use the entire pre-intervention period from November 2009 to November 2014, 61 months in total, and take the average of y and $\mathbf{X}$ over this period. More details are in the Online Appendix. See also Abadie (forthcoming).

An important advantage of the synthetic control method over the DD and spatial RD models is that it limits extrapolation bias, which can emerge when untreated local authority districts (in England and Wales) display different pre-reform characteristics and trends with respect to their treated (Scottish) counterparts. As observed by Abadie, Diamond, and Hainmueller (2010), however, the synthetic control estimator may suffer from interpolation bias as it uses a weighted average of the untreated local councils to create a synthetic untreated Scotland with pre-reform characteristics similar to those observed for Scotland. Other estimators instead, such as nearest-neighbor matching, have the opposite properties, that is, they curb interpolation bias but suffer from extrapolation bias, extrapolating too much when suitable untreated districts are unavailable. Kellogg et al. (2019) suggest to optimize the strength of the two estimators and combine matching and synthetic control (MASC) procedures through model averaging. In the analysis below, we also employ this more recent approach.

4. Results on the DDL Reform Evaluation

A. Benchmark Estimates

Table 2 reports the DD estimates of the effect of the 2014 Scottish 0.05 BAC law using the STATS19 information contained in the RAD records. We show the results from five different specifications of (1), depending on whether we include controls, group-specific linear month-year trends, group-specific month fixed effects, and local authority fixed effects, and for five different definitions of the outcome variable.

The estimates in panel A, where we examine all cases included in the RAD records, reveal there is a 1.3 percentage point (nearly 5%) reduction in total accident rates as a result of the reform. But this is true only when we consider the raw DD specification (column (a)). Including controls in column (b) leads to an insignificant estimate, while including month-year trends, month fixed effects, and local authority fixed effects leads to small positive and statistically insignificant effects (columns (d) and (e)). We also find a 13% reduction in serious injury accident rates (panel C), but this impact disappears when we control for time trends, month and local authority fixed effects (specifications (c)–(e)), when it also switches sign. Irrespective of the specification, all the estimates for fatal and slight injury accidents are quantitatively

[24] An analogous identifying assumption, namely that unobserved differences between treated and non-treated local authorities are time-invariant, is also imposed by the DD model described above (see Abadie, Diamond, and Hainmueller, 2010). In fact, the synthetic control method generalizes the DD model by permitting the effect of unobserved confounders to vary over time according to a flexible factor representation of the potential outcomes of the treated local authorities.

modest and statistically insignificant (panels B and D, respectively). Thus, it is not the case that the tighter BAC limit, which might have had a greater influence on low levels of alcohol intake, had an impact on less severe crashes. Finally, and crucially, we find no effect on drink drive collisions across all five specifications (panel E).

Among all crashes in which drivers were breathalyzed, we can also separate cases in which there was at least one positive test from those in which drivers refused to be tested at the roadside. The results for these two cases are reported in panels E1 and E2, respectively. While there is no change in accident rates with positive tests, we detect a statistically significant increase in those involving refusals. Since refusals occurred only in 11% of all pre-reform breathalyzed collisions, this may be why we do not find an impact when positive tests and refusals are not separated out, as in panel E. Following the arguments presented in Section 3, this (modest) increase could be the mechanical result of unchanged alcohol consumption among drivers who might have been aware they were above the new limit. Section 7 will explore this possibility further.

To weigh up the overall null results in terms of statistical power, we notice that the standard errors found from the most comprehensive specification (column (e)) suggest that the points estimate on all accidents has to be about 5% of the mean to be detectable, as it is in column (a). The effect size must be of the same order of magnitude for slight injury accidents. It grows instead to 10%, 20%, and 30% in the case of serious injury, drink drive, and fatal accidents, respectively. These, and especially the last two, may seem large. It is worth stressing, however, that all estimates — except the one on slight injuries — are positive and thus against the prediction from theory and the expectations of the policy makers, while they are all negative in column (a). The sign switch occurs invariably in column (c), when we include both the full set of controls and group-specific linear trends. Accounting for differences in the time patterns of accident rates and other observables between Scotland and the rest of Britain is important.

As mentioned, the effect size for drink drive and fatal accidents has to be somewhat large (albeit wrong-signed) in order to be statistically detectable. Since, ex ante, the reform is salient primarily for drivers who fall into the 0.05–0.08 BAC range (abstracting from spillovers due to uncertainty about precise BAC levels), the group of drivers at risk may be too small to yield detectable effects, even if all of them reduced their alcohol intake after the reform passed. We shall come back to this issue in Section 5, while now we turn to the results found with the other two approaches.

The spatial RD estimates from equation (2) are presented in Table 3 showing results for progressively smaller bandwidths, from 200 down to 50km.[25] The reform did not have any significant impact on accident rates as a whole, as well as on serious and slight accident rates. We find an effect on fatal accidents and those accidents with at least one driver who had a

[25]In square brackets, Table 3 reports wild bootstrapped p-values of the treatment effects, which are estimated when the spatial dimension (number of local councils) is small (Webb, 2014). For completeness, however, we report p-values for all bandwidths. The maps in the Online Appendix display how the control areas change as we vary the distance from the border.

positive (or refused) breath test, but this emerges only when we consider further distances from the border (up to 200km). Differently from the DD estimates, distinguishing crashes between those in which drivers refused to be tested and those in which drivers were breathalyzed and resulted positive leads to the same null effect for both types of accidents (see the results in the Online Appendix). By and large, therefore, these results uphold those found earlier.

Figure 2 displays maps of Britain, in which the light shaded areas represent English/Welsh local authority districts in synthetic Scotland along with their weights, for all motor vehicle collisions and each of the other four accident types. All unshaded districts in the potential control group are assigned zero weights. Figure 3 show district-specific accident rates for Scotland and synthetic Scotland. They suggest the reform had essentially no effect on all crash rates and by type. The estimates for synthetic Scotland closely track the trajectory of accident rates in Scotland for the whole pre-intervention period. But, after the enactment of the Scottish 0.05 BAC law, the two lines continue to overlap substantially regardless of the type of collision, revealing that the alcohol restriction led to no change in road traffic accidents.[26]

This conclusion is in line with the results shown by Haghpanahan et al. (2019) and Cooper, Gehrsitz, and McIntyre (2020) on the same 2014 Scottish reform we study. In comparison with those two studies, however, the inferential evidence from our evaluation is likely more credible for it is drawn from finer geographic details, is based on more data, and is robust across three complementary research designs. Our conclusion is also consistent with the findings reported in Carpenter (2004) for drink driving among individuals around the legal drinking age, but it differs from the results found by Dee (2001) and Eisenberg (2003) for fatal crashes in the US.

B. Heterogeneity

The null results in the benchmark analysis above may mask considerable effect heterogeneity. The reform may have an impact in specific cases where alcohol is likely to be most consumed. Age and context are known to be key alcohol reinforcers, which may affect brain activities and trigger drink driving (Zironi et al., 2006; Corbit, Nie, and Janak, 2012). We therefore distinguish accidents by the timing of when they occur and by drivers' age. We also look for differential impacts by gender, by the number of vehicles involved in crashes, and by the population density which can be correlated with the likelihood of accidents (proxied by either an urban/rural stratification of the data or the number of licensed premises).

For each of the next five pieces of analysis, which also contain a few combinations of different margins, Table 4 presents 100 synthetic control treatment effects averaged over the 25 months after the change in the DDL law, with pseudo standardized p-values reported in parentheses. To

[26]In the Online Appendix, following Abadie, Diamond, and Hainmueller (2010), we discuss inference performing the falsification test based on the distribution of the (placebo) effects estimated for all local authority districts in the control group. In the pre-reform period, the difference between Scotland and synthetic Scotland falls in the middle of the placebo tests. This continues to be the case even after the passing of the new legislation. Evaluated against the distribution of the gaps for the placebo districts, therefore, the gap for Scotland does not appear to be unusual. This confirms that the 2014 DDL reform had no effect on all types of collisions. The same null estimates are found even when we distinguish road accidents in which the driver refused to be tested from crashes in which the driver/s was/were breathalyzed and resulted positive.

ease comparisons, the first row of the table shows the estimates from the baseline specification, which correspond to the estimates displayed in Figure 3 and compare well with the DD and spatial RD estimates in Tables 2 and 3, respectively.[27]

Timing of Accidents — We first consider the impact of the tighter BAC limit at different hours of the day and different days of the week when alcohol consumption is more or less likely to play a role (Francesconi and James, 2019). The analysis divides observations during the day (8:00am till 8:00pm) from those during the night (8:00pm till 8:00am in the following morning). There is no evidence of a difference in the impact of the new limit by time of the day. Neither does there appear to be an impact of the DDL on road accidents that happen at weekends (Fridays, Saturdays and Sundays). Defining day/night times and weekends differently yields results of much the same order of magnitude and statistical significance.

Age of Drivers — We next examine the possibility that drinking varies by age (Naimi et al., 2003), and so does risk taking behavior in drink driving (Levitt and Porter, 2001). Both channels may lead to heterogeneous effects of the reform. Estimates for those accidents that involved at least one driver aged 18–30 and 50 years or more do not detect any impact of the lower Scottish BAC limit on any type of motor vehicle crashes across all age groups.[28]

Gender of Drivers — Men take more risks when driving and drink more than women (Holmila and Raitasalo 2005; Rhodes and Pivik, 2011), although recent research documents increased drinking among women (White et al., 2015; Wilsnack et al., 2018). We thus estimate the impact of the new DDL on road accidents by sex, and find no impact for either sex. Restricting attention to crashes in which only men were involved leads to the same result.

We also analyze a series of combinations of these potential sources of heterogeneity, e.g., accidents involving male drivers at night or young male drivers during the weekend. None of such estimates is statistically significantly different from zero at conventional levels.

Number of Vehicles Involved in Accidents — Multiple vehicle crashes could have larger externalities and may affect individuals who were sober at the time of the accident. Therefore, we analyze whether the effect of the BAC reduction was different in one vehicle accidents as opposed to multiple vehicle crashes. Once again, the results document a zero effect of the reform on both types of collisions.

Urban/Rural Roads and Number of Licensed Venues — The lowering of the legal DDL may have a different effect in urban and rural areas, which may be systematically correlated to traffic density and road conditions (Albalate, 2008). Similarly, there might be effect heterogeneity depending on the number of premises licensed to sell alcohol. We therefore stratify the sample into urban

[27] The estimated gaps in road accident rates, which compare Scotland with its synthetic counterpart, along with the corresponding placebo gap effects are not shown for convenience but are available upon request. Likewise, we do not report the estimates found on positive tests and refusals separately, since they are both similar to those shown in the last column of Table 4.

[28] The same results emerge if we consider accidents with individuals aged 18–25 and 30–50 years.

and rural roads and, separately, into local authorities with a number of licensed premises above and below the overall median.[29] We then repeated the analysis for each of these four groups. The estimates confirm the baseline results, showing there is no effect regardless of whether accidents are observed in urban or rural roads or in areas with a high or low concentration of premises.

Of the 100 estimates reported in Table 4, none is statistically distinguishable from zero. The lowest p-value of 0.092 is found for night collisions involving young males. All the other estimates have larger standardized p-values. Based on this evidence, therefore, we confirm that the stricter BAC Scottish law had no effect on road traffic accidents, even in circumstances that are more likely to be associated with greater alcohol consumption (such as weekends, multiple vehicle crashes, urban areas, and local authorities with a large concentration of premises) or among individuals who may experience heavier drinking (such as young adults and men).

C. Robustness Checks

We performed ten detailed exercises to check whether the finding that the reform had no impact is sensitive to either the definition of the dependent variables, functional forms used in estimation, estimation approaches, or methods of inference. The estimates from these ten exercises are available in the Online Appendix or upon request. First, we used five new definitions of the outcome variables (based on number of monthly accidents per 10,000 of the population, per 10,000 of the adult population, per kilometer of road in the local council, and per vehicle miles travelled, and based on the logarithm of the number of accidents). Second, we re-estimated the synthetic control models matching on annual frequencies, matching only on outcomes and using demeaned data. Third, we repeated the DD and spatial RD analyses using count data models, in which the dependent variables are defined by the number of collisions in a given month. Fourth, we re-estimated our baseline specifications using Fisher's randomization test (Fisher, 1935). Fifth, we refitted the benchmark analysis but only on motor vehicle crashes in which at least one of the drivers tested positive (i.e., above the legal limit) or refused to be tested.

Sixth, we performed the full analysis using weights based on the local authority districts' population size to account for the possibility that the use of disaggregated geographic data be characterized by noise in low-population councils, where the changes in the number of accidents could have a higher impact on the outcome variables. Seventh, we re-estimated the spatial RD model using more flexible distance patterns, allowing for different functional forms on either side of the border and for the distance to have time varying differential effects by treatment group. Eighth, we used more detailed data on drink drive accidents, containing precise information on hit-and-run accidents and toxicology information on fatalities from coroners, but available only at annual (not monthly) frequencies and coarser geographic level (11 regions rather than 378 local councils). Ninth, we employed a linear panel event-study design similar to the one

[29] Urban roads are defined to be those within an area of population of 10,000 or more. For the definition of urban/rural roads, see <https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment-data/file/743853/reported-road-casualties-gb-notes-definitions.pdf>.

proposed by Freyaldenhoven, Hansen, and Shapiro (2019), in which causal inference is valid even when there are pre-reform trends in the outcome variable. Tenth, we estimated matching and synthetic control (MASC) models as formulated by Kellogg et al. (2019) to deal with the potential interpolation bias induced by the synthetic control method mentioned in Section 3.

All ten exercises produce the same null results found in the benchmark analysis and emphatically reiterate that the stricter DDL had no impact on road accidents.

5. Statistical Power and Supply of Offenses

We now go back to the issue of statistical power mentioned in the previous section. To do this, we focus on the empirical relationship between BAC levels and the number of road traffic violations and accidents. If between the 0.05 and 0.08 BAC levels the relationship were steep and positive, we expect an intervention like the 2014 reform in Scotland to be effective in reducing road accidents and deaths. If instead the relationship were flat, the potential for the same intervention would be considerably more circumscribed.

A data problem is that individual breath test information with exact levels of BAC is not available for Scotland. We therefore use highly detailed individual-level digital breath test data collected between 2009 and 2014 by all police forces in England and Wales, the only data in Britain that report the precise reading, time, and location of each breath test administered. Breath tests were carried out because of a moving traffic violation, or another road code violation (e.g., illegal parking), a road traffic collision, or suspicion of alcohol. We find that almost 15% of all pre-reform accidents in England and Wales were concentrated in the 0.05–0.08 BAC range.[30] If the same proportion were to apply to Scotland and there were perfect compliance, we would expect to have a large enough sample of accidents to detect significant impacts across all collision types in Table 2. In general, therefore, statistical power is not an issue, keeping also in mind that the estimates in the table are wrong-signed.

This is confirmed by the comparison of the size of our estimates to the size of those found in the literature for fatal accidents. For instance, our DD estimate in column (a) of Table 2 implies an insignificant 2% reduction, while the estimate in column (e) implies an insignificant 10% increase. The synthetic control estimate in the first row and first column of Table 4 implies an insignificant 20% increase. The significant estimates found by Dee (2001), Eisenberg (2003), and Albalate (2008) imply reductions in fatal crashes of 7, 3, and 4%, respectively, which are arguably of comparable order of magnitude in absolute value to ours.

Another related exercise is to perform a reduced form analysis of the aggregate supply of offenses formulated by Becker (1968) and Ehrlich (1973). The starting idea is that there is, at the individual level, a function relating the number of offenses, O_j, to the probability of conviction, p_j, the punishment if convicted, f_j, and other variables, u_j, such as the relative returns to drink driving with respect to other legal activities and the willingness to drive under the influence. If

[30] Interestingly, had the BAC been reduced to 0, the fraction of accidents that could have been prevented would have increased to 40%.

all individuals were identical, this function could be regarded as an aggregate supply function in a given time period, $O = O(p, f, u)$, and could be interpreted as the cumulative distribution of a density function showing differences across individuals with respect to the minimum expected net gain that is sufficient to induce them to become drink drivers.

Since we have no information on p or f in the data, we assume that both p and f are increasing linear functions of BAC levels. Substituting these two expressions into $O(\cdot)$ leads to our reduced form specification, which thus links the number of violations and collisions to BAC levels. Besides the already mentioned unavailability of individual breath test information for Scotland, we have an additional data issue. For each recorded accident, we ideally need to have breath test information from at least another random driver in the exact location where the crash was observed, on the same day of the week and at the same time of the day, one week after the recorded accident to replicate external conditions which are as close as possible to those that occurred at the time of the recorded accident. This will give us information about the relative crash risk among drivers with different blood alcohol concentration levels. To address this problem, we use the estimates of the relative risk of a crash provided in Compton et al. (2002), who collected breath test data on drivers involved in crashes of all severities in California and Florida and on two additional random drivers at the same location, day of the week, and time of the day a week after the crash, serving as control group. Compton and colleagues then estimated relative crash risk models as a function of BAC levels using logistic regression techniques.

For each level of BAC observed in our data, we thus compute the relative risk of offenses, multiplying the proportion of police administered breath tests by the relative risk of a crash estimated by Compton et al. (2012) and normalising the relative risk to 1 when no alcohol was consumed. The results of this exercise are presented in Figure 4, where we show one relationship in which all road traffic violations and collisions in the data are used and another in which we exclude the tests performed as a result of suspicion of alcohol. The two vertical lines are drawn in correspondence to the old and new DDLs (35 and 22μg, respectively, or equivalently 0.08 and 0.05 BAC). The inset in the figure zooms in on the interval between the two limits, where we expect to observe the impact of the reform.

Given the supply of offenses is flat up to about 40 micrograms of alcohol per 100 milliliters of breath, a reduction from 35 to 22μg could not curtail the risk of motor vehicle accidents and road traffic violations substantially. The relative risk does decline, from 1.64 to 0.67 (or from 2.20 to 0.96, when excluding tests done on suspicion of alcohol), but the reduction is arguably relatively small.[31] Put differently, the implied semi-elasticity of the risk of collisions to BAC is only 0.21 (s.e.=0.036) between the new and old limits, i.e., a 10% increase in BAC over this range

[31] This is contrary to the information used by the Scottish government to increase awareness of the new DDL (see <https://www.wired-gov.net/wg/news.nsf/articles/Lower+drink+drive+limit+in+Scotland+041220 14102005?open>). Notice also that, when we consider all tests in Figure 4, the estimated relative risks around both thresholds (35 or 22μg) are below 1, which corresponds to the risk when no alcohol is consumed. A large number of offenses are observed even when drivers are legally sober or when they have no alcohol involvement at all.

augments the relative risk of collision by one-fifth of a point. This compares to substantially greater elasticities found at higher BAC values. For instance, the elasticity over the 0.08–0.10 BAC interval (which is relevant to most of the reforms in US states since the 1990s) is almost three time larger at 0.61 (s.e.=0.047), and the elasticity for BACs above 0.10 is a staggering 19.4 (s.e.=2.82), even higher than the estimates reported by Levitt and Porter (2001) and Romano et al. (2018) for fatal accidents only. In order to be able to see an impact of the reform, therefore, we would need to observe a dramatic reduction in alcohol consumption (from, say, at least 0.08 to less than 0.05 BAC) among a large fraction of drivers who would have been involved in a collision before the enactment of the stricter DDL. Figure 4 suggests that the potential for this impact at those BAC levels was already limited ex ante. In addition, Section 7 will provide evidence of no change in ethanol intake, which makes it ever harder to detect an impact on road accidents.

Provided the patterns in the breath test data for England and Wales be good proxies for Scotland's and provided the relative crash risks estimated in the US be generalizable to Britain, the evidence in Figure 4 suggests the 2014 Scottish DDL reform did not have much ex-ante scope for a sizeable impact.

6. Channels: Why Was the DDL Reform Ineffective?

Addressing this question is crucial for both our understanding of human behavior and improving policy design. To find answers, we resort to the insights of the market model of crime à la Becker (1968) and Ehrlich (1973), which yields an equilibrium where marginal costs equal marginal benefits of drink driving. Put differently, equilibrium in this market hinges on the notion of equality between the slope of an opportunity boundary (the production transformation curve of the composite good which individuals care about when they decide to drink and drive as opposed to driving sober, drinking without driving, or using other means of transportation) and the slope of an indifference curve, which embeds individual preferences and the probability of being caught driving under the influence.

Economic incentives affect the drink drive decision in a number of direct and indirect channels (Draca and Machin, 2015). We examine two of such channels that might have affected the market equilibrium through the DDL reform. Among the direct incentives, which could have changed the net return of engaging in drink driving, we consider the availability and prices of buses and taxis. These represent the two most important means of transportation other than one's own vehicle. We also explore the possibility of having a lift with friends or other close individuals, such as relatives and colleagues. Among the indirect incentives, which operate through deterrence and incapacitation, we analyze measures of enforcement, such as the number of police officers, breath tests unrelated to motor vehicle collisions, and arrests and convictions for other crimes related to drink driving. Below we give a summary of the key estimates, some of which are in the Appendix while others are available upon request.

A. Alternative Means of Transportation

Greater provision and/or lower fares of means of transport, other than own vehicles, increase the opportunity cost of driving under the influence. An increase in the opportunity cost might encourage people to shy away from drink driving. No change in availability or prices, instead, will provide a possible explanation for why the reform did not save lives.

Taxi Availability — To gauge the extent of this alternative supply of transportation means, we combine information on taxis and private hire vehicles (which we refer to as 'taxis' or 'cabs'), and use two separate measures of availability, that is, the number of driver licenses and the number of vehicle licenses.[32] The data are collected every two years from 2009 to 2015 by the DfT and aggregated at the local authority highway level, of which there are 314 in England and Wales and 32 in Scotland. Regardless of whether we consider drivers or vehicles, there were more taxis per capita available in Scotland relative to the rest of Britain. Between 2009 and 2013, however, Scotland experienced a slow decline in both types of license, whereas in England and Wales the average number of licenses was stable. After the reform, cab driver licenses continued to fall in Scotland, while remaining unchanged in England and Wales; vehicle license rates instead did not change in post-reform Scotland, but increased south of the border.

To examine the impact of the reform on cab availability, we estimate DD models as in (1) separately for each type of license. The tighter BAC law led to a reduction in availability in Scotland of about 7–9%, with or without controls and even after accounting for local authority fixed effects. But for both license types, the inclusion of group-specific trends wipes out the differences between Scottish districts and their English and Welsh counterparts, yielding small and statistically insignificant treatment effect estimates. These no-impact results are corroborated by the estimates found with both the spatial RD framework and the synthetic control approach.

Taxi Tariffs — Information on taxi fares comes from the Private Hire and Taxi Monthly, the official newspaper of the UK National Private Hire Association.[33] The tariffs recorded in the data refer to the maximum that can be charged in a given council and are grouped by average day rates and average night rates. Our main results are based on monthly fare average tariffs for 2-mile journeys at the local authority district level from January 2009 up to November 2016. We also analyze four other categories, distinguishing the average cost of hailing a cab (or minimum fare), the average cost of 1- and 10-mile journeys, and the mean charge per mile travelled after the initial pull-off distance (or running mile fare). For such four categories, however, we can only use monthly fare averages at the regional level, since the information at the geographically more disaggregated council level is not available.

Basic DD estimates point to a modest increase of about 2.5% in taxi tariffs, although this

[32] Taxis are available for immediate hire and can be hailed in the street or pre-booked. Private hire vehicles (or minicabs), instead, must be pre-booked, cannot ply for hire, and cannot use taxi ranks. Driver licenses are issued to the driver, while vehicle licenses are issued to the owner of the cab (who may be the same as the driver, or another individual, or a company).

[33] See <https://www.phtm.co.uk/taxi-fares-league-tables>.

impact becomes smaller and significant only at the 10% level when group-specific trends are accounted for. The spatial RD estimates reveal a larger increase of 6%, but at the margin of statistical significance and only for councils that are 50km from the border. The estimates found with greater bandwidths are always smaller and statistically indistinguishable from zero. The synthetic control placebos corroborate the evidence that the reform had no effect on taxi tariffs. Finally, the DD results found for the other four types of tariffs at the more aggregate regional level confirm the null effect.

Bus Availability and Bus Fares — For availability, we examine data from the DfT Public Service Vehicle Survey, which are available annually from 2004/05 to 2016/17 for each of the three constituent nations of Great Britain (England, Wales, and Scotland). With this high level of aggregation (in time and space), we can only rely on difference-in-difference models. We have three different measures of bus usage, i.e., the number of bus journeys per capita, bus miles per capita, and average bus occupancy (i.e., passenger miles divided by vehicle miles). The estimates reveal no evidence that the 2014 reform had an impact on the three measures. We only detect a weak treatment effect increase in bus miles traveled per head, but this impact was economically negligible, representing merely a 1% increase, and statistically significant only at the 10% level.[34]

Local bus fare indices (both in current and constant prices) are published by the DfT from 2004/05 to 2016/17 at the country level. Although bus fares increased in all three countries throughout the sample period, our estimates unambiguously indicate that the reform led to no differentials in bus prices, whether we consider fares in current or constant prices and irrespective of the definition of the post-reform period.

Car Sharing as a Passenger — A simple and cost-efficient alternative means of transportation is to car share with sober friends, colleagues or family members. To explore this channel, we perform two exercises. First, we use the UK Time Use Survey (UK-TUS) 2014/2015 and examine changes in time spent in a vehicle as a passenger after the reform. Although the UK-TUS does not contain information on alcohol involvement for either passengers or drivers, we can focus on car sharing in specific times of the day or days of the week and following activities that might be correlated with alcohol intake (e.g., after an evening in a pub or a restaurant). Second, we distinguish between collisions that involve only one individual from accidents that involve multiple individuals. If we observe a rise in accidents with multiple individuals involved, this might be the result of increased car sharing, even though we cannot determine whether the drivers are sober. We find no evidence of an increase in riding as a passenger either for all days, or at the weekend, or after an evening out in pubs or restaurants. We also find no effect of the reform on crash rates for those with one causality or those with two or more causalities.

Putting together the results so far, we conclude that, after the enactment of the 0.05 BAC law

[34]Since we have annual (April to March) data, the post-reform period could be defined in reference either to 2015/16 only or to both 2014/15 and 2015/16. The results in the Online Appendix show that this distinction makes no difference to the results.

in Scotland, Scottish drinkers did not have greater opportunity costs of driving their own vehicles as bus fares and taxi tariffs did not fall, and bus and taxi availability did not go up. They also did not seem to have car shared as passengers more than in the past. The reduction in cab provision and the modest increase in taxi tariffs, which we find in some specifications, may indicate an inward supply shift suggesting a possible increase in incentives in driving while intoxicated post intervention.[35] The unavailability of (cheaper) alternative means of transportation therefore may be one of the channels that frustrated the efficacy of the reform.

B. Enforcement of the New Limit

In a market model of crime, the optimal amount of enforcement depends, among other things, on the cost of catching and convicting offenders, the responses of offenders to changes in enforcement, and the nature of punishments (Becker, 1968; Mookherjee and Png, 1994). Punishment (criminal or otherwise) remained unchanged everywhere in the UK after the stricter DDL. Also, there was no explicit anti-drink drive crackdown in Scotland (such as "hot spot" policing in the most crime-prone locations or random checking across widely spread locations) that might have affected lawbreakers in a predictable way, e.g., switching to unpoliced roads.[36] We focus on police numbers, breath tests unrelated to car crashes (which are thus not part of the RAD STATS19 records examined in Section 4), as well as drink drive arrests and convictions unrelated to road accidents that are not recorded in the RAD data.

Police Numbers — Due to restructuring of the police force in Scotland and to changes in recording police activities around the reform, we can only examine police numbers overall, and not the number of police officers deployed in specific activities, such as traffic duties, or hours worked in such activities. From the Police Officer Quarterly Strength Statistics, we have annual information on police numbers for the 13 police forces in Scotland between 2013 and 2016, while for the 43 English and Welsh police authorities we obtain the same information from the Home Office Police Workforce. Irrespective of the estimation approach, we always find that the reform led to no change in the number of police officers per 100,000 of the population.

Breath Testing — Another, perhaps more direct, measure of enforcement can be inferred from the number of breath tests actually carried out at the roadside.[37] These tests include not only

[35]The modest rise in taxi fares along with no change in taxi availability may also indicate a slightly greater demand for taxis with an inelastic supply. This would be consistent with our conclusion that alternative transportation availability did not change, even if there could have been greater demand for it. In turn, this might have contributed to the lack of a decrease in alcohol-involved crashes.

[36]Haghpanahan et al. (2019) conjecture that the lack of an impact on road traffic accidents was due to the lack of enforcement of the legislative change in Scotland. They, however, do not document this claim. Lindo, Siminski, and Yerokhin (2016) also speculate that the lack of evidence that legal access to alcohol has effects on motor vehicle accidents in New South Wales be attributable to high levels of enforcement. The importance of enforcement is explicitly shown by Banerjee et al. (2019), who analyze the effect of an anti-drink driving campaign in the Indian state of Rajasthan which was implemented in a randomized fashion. They find that random checking was highly effective, reducing night accidents by 17% and night deaths by 25%. See also the discussions in Draca and Machin (2015) and Chalfin and McCrary (2017).

[37]Breath testing is one of the key policies recommended by the WHO to reduce the harmful use of alcohol (World Health Organization, 2010) and endorsed by campaigners and charity organizations working to prevent

those administered after of a car crash, which we analyzed in Section 4, but also those unrelated to collisions. Although *random* breath testing is not permitted, the police across Britain do not need to give a specific reason to stop a vehicle. To breathalyze a driver, police officers must have a reasonable suspicion that the driver has consumed alcohol. During a routine stop, this can be judged for instance by smelling alcohol or whether the driver appears intoxicated. Even without suspicion of alcohol intake, breathalyzation can occur when the driver has committed a traffic violation.

For England and Wales, the data are published by the DfT and are collected using digital breath testing devices by each of the 43 police forces. For Scotland, we do not have access to the same type of data. We instead use the only available information that is published by the Parliamentary Advisory Council for Transport Safety. Every year, there are normally two periods when Police Scotland collect and release data on breath tests, during the summer and the winter festive season around Christmas and New Year. We have data from 2013 to 2016. The data collection can last either a fortnight or four weeks. To make the data as comparable as possible, we take the Scottish collection periods and select the equivalent time windows from the English and Welsh data. Two week campaigns are scaled up to their four week equivalents.

We analyze two outcomes, the number of breath tests administered per 1,000 population and the proportion of positive tests. According to the DD model, there is no statistically significant treatment effect estimate for both outcomes. The synthetic control estimates confirm that the gaps in both outcomes for Scotland and the placebos are nonexistent. Repeating the whole analysis using the number of breath tests per 1,000 drivers leads to the same conclusion.

Drink Drive Arrests Unrelated to Road Accidents — In the UK, data on all drink driving offenses are not published on a monthly basis at the local (council or regional) level. To gather such data, we therefore contacted every police force in Great Britain submitting a Freedom of Information (FOI) request. For the period January 2010–December 2016, we have data for the whole of Scotland from Police Scotland, disaggregated into 13 police force areas, and for 13 police forces in England and Wales. We also have data from nine other English and Welsh forces but over shorter time frames. Our outcome variable is the monthly regional crime rate, defined as the number of drink drive arrests or offenses divided by 100,000 heads of population in each of the correspondent police force areas. Given the data at the police force level are geographically coarser than those based on local councils, there is not much scope to perform the analysis using the spatial RD design.

All the DD estimates suggest a lessening in drink drive crime rates in Scotland although none is statistically significant. From the synthetic control analysis, there appears to be a reduction in arrest rates immediately after the introduction of the 2014 reform, but the series for Scotland and synthetic Scotland begin to overlap again after a couple of months from the reform. The absence of an impact is also confirmed when we inspect the gaps in arrest rates for Scotland and the placebos.

and cut alcohol-related harm (see <https://www.alcohol-focus-scotland.org.uk/campaigns-policy/>).

Drink Drive Convictions Unrelated to Road Accidents — Data on convictions, made available from the Criminal Proceedings in Scotland and the Ministry of Justice in England and Wales, are only published at an annual frequency (from 2008 to 2016) and at the country level (i.e., Scotland on one hand and England and Wales on the other). This means we cannot perform any analysis using spatial RD models or synthetic control methods. Nonetheless, the lack of an effect on arrest rates following road traffic collisions found earlier emerges also in the case of annual conviction rates for driving under the influence from the DD estimates.

In spite of the challenges imposed by some of the data, the evidence on enforcement is compelling. Compared to the rest of Britain, Scotland did not experience a relative increase in police officers per capita and Scottish drivers did not face greater odds of being breath tested after the stricter 0.05 BAC limit was imposed in December 2014. Similarly, neither arrest rates nor conviction rates related to drink drive offenses that did not end up in a motor vehicle crash show differences in Scotland as opposed the rest of Britain after the policy intervention. Everything else equal, therefore, potential offenders in Scotland did not see a significant change in the opportunity cost of driving while intoxicated, as their expected marginal cost of punishment remained unaltered.

This evidence and the very absence of an anti-drink drive crackdown suggest that the inefficacy of the DDL reform to save lives may be due to lack of enforcement. This adds to the mechanism linked to availability and prices of alternative means of transportation documented earlier. Although it is hard to determine which of the two channels played a bigger role, we observe they both unambiguously worked in the same direction.

7. Unintended Consequences and Spillover Effects

Section 4 shows that the 2014 DDL reform was ineffective in reducing all sorts of motor vehicle accidents on Scottish roads. The reform, however, might have triggered unintended consequences on other important domains of behavior. In what follows, we summarize the results along two of those domains, namely attitudes towards drink driving and alcohol intake. We also explored other domains, such as own car usage, food consumption, and smoking. And we analyzed potential spillovers on offenses and crimes other than drink driving (including speeding, illicit drug usage, robbery, and sexual offenses) as well as aggregate responses from the alcohol and car industries. In all such dimensions, whose estimates are available in the Online Appendix, we cannot detect any impact of the stricter limit.

Public Attitudes Toward Drink Driving — Using repeated cross-sectional data from the British Social Attitudes Surveys 2009–2016, we examine two attitude domains. The first is in relation to the statement "If anyone has drunk any alcohol they should not drive", and the second refers to "Most people don't know how much alcohol they can drink before being over the legal drink drive limit". Answers to both questions are recorded on a five-item Likert scale. Given the annual frequency of the data and their coarse geographic details, we can only estimate difference-in-difference models. The results are reported in Table 5. They show that the reform did not affect the public's perceptions about DDL knowledge, but it increased attitudes against drink driving among Scottish respondents, which effectively implied a zero-tolerance sentiment.

Alcohol Consumption — If would-be offenders perceived the reduced legal limit as a factor that lowered the expected utility of drink driving, they might have cut alcohol intake before driving, even though we do not see lower road accident rates. To address this possibility from the individual consumer's perspective, we analyze data from the Health Surveys of England and the Scottish Health Surveys over the period 2008–2016, which allow us to fit only DD models. From the estimates in Table 6, we infer that there is no evidence of an impact of the stricter DDL law on a raft of measures of alcohol intake among Scots. This null result is also corroborated by an analysis of time diaries, which shows that Scottish people did not change the time spent in pubs and restaurants after the passage of the 2014 reform, either during the week or at weekends, among both men and women, and among the young or the less young. The unchanged patterns in alcohol consumption may explain the increase in crash rates in which drivers refused to be breathalyzed at the roadside, which we find with the difference-in-difference model shown in panel E2 of Table 2. The same behavior, however, was not strong enough to affect other types of collisions.

In sum, the 2014 DDL reform induced an anti-drink drive sentiment among the Scottish public. But this did not lead to any reduction in alcohol consumption. It also did not lead to any other economically relevant externality. Scots did not drive less or more. Their healthy diets, and smoking habits did not change. The reform had no spillovers on motor vehicle offenses that might have not been related necessarily to drinking, such as speeding or driving while using a mobile phone. It did not generate displacement effects towards other types of criminal activities, including illegal drug usage, robberies, and sexual offenses. The alcohol industry remained unscathed, with no changes in production, prices, or employment. Similarly, the car industry faced no variation in automobile registration rates, petrol prices, and insurance premiums.

8. Conclusion

This paper evaluates the impact on motor vehicle crashes of a 2014 reform that reduced the drink drive limit from 0.08 to 0.05 BAC in Scotland, while in the rest of Britain the limit stayed at 0.08 BAC. Assembling several new data sources for the first time and using careful research designs, we conclude that the reform had no effect on accident rates, the main target of the Scottish lawmaker. This is the case for all types of accidents, from fatal crashes to collisions with only slight injuries, and regardless of whether drivers were drunk or sober. This null result holds for young and old drivers, men and women, whether crashes occurred during the day or at night, in weekends or workdays, and if they involved one or multiple vehicles. The result is also robust to several sensitivity checks, including manifold redefinitions of the outcome variable and finer categorizations of drink drive crashes, allowing for randomization inference, using count data models and linear panel event-study design, and combining matching and synthetic control approaches.

Although there is no issue of statistical power, that is, there are enough accidents in the relevant 0.05–0.08 BAC range pre-intervention, we find evidence that the reform could not have had much scope for a sizeable impact on road traffic accidents. The estimates from a reduced form version of the supply of offenses reveal only a modest collision elasticity to alcohol consumption over the critical 0.05–0.08 BAC range. This suggests that the pre-existing maximum legal level was already sufficiently low that further abatements in motor vehicle crashes could not have been easily reachable, *unless* the reform were accompanied by changes in other factors, such as stricter enforcement of the new DDL or hot-spot policing.

Establishing why the stricter BAC limit had no effect on the intended policy outcomes is a key contribution of our work. Using the insights of the canonical market model of crime, we focus on two mechanisms, namely alternative means of transportation and law enforcement. Taxis and buses were neither more available nor cheaper, as a result of the reform. Similarly, we find evidence of no impact on enforcement, measured broadly in terms of police numbers, breath tests carried out at the roadside, and drink drive arrests and convictions unrelated to motor vehicle crashes. Both channels, therefore, work jointly to explain why the new DDL law was ineffective in saving lives on Scottish roads.

Accompanied by a heavyweight media campaign, the reform ramped up anti-drink drive attitudes close to unanimity, but this was not enough for Scots to reduce alcohol consumption. We also cannot detect any economically meaningful positive externality of the stricter DDL. Scots did not drive their cars less, nor did they switch to being driven by friends or others. They also did not improve their diet or their smoking habits. There was no reduction in road offenses other than drink driving, such as speeding and driving while using a mobile phone or without a seat belt. And finally the reform neither reduced other crimes, including illegal drug use, robberies, and sexual offenses, nor did it affect the alcohol and automobile industries, either negatively or positively. Taken together, our no-effect results defy pre-reform expectations as well as most of the existing medical evidence, which is predominantly correlational. They underline the crucial role played by environmental channels (such as alternative means of transportation and policing) to uphold the success of a small reduction in the drink drive limit from 0.08 to 0.05 BAC. This in turn is likely to be an important lesson to consider in similar future interventions.

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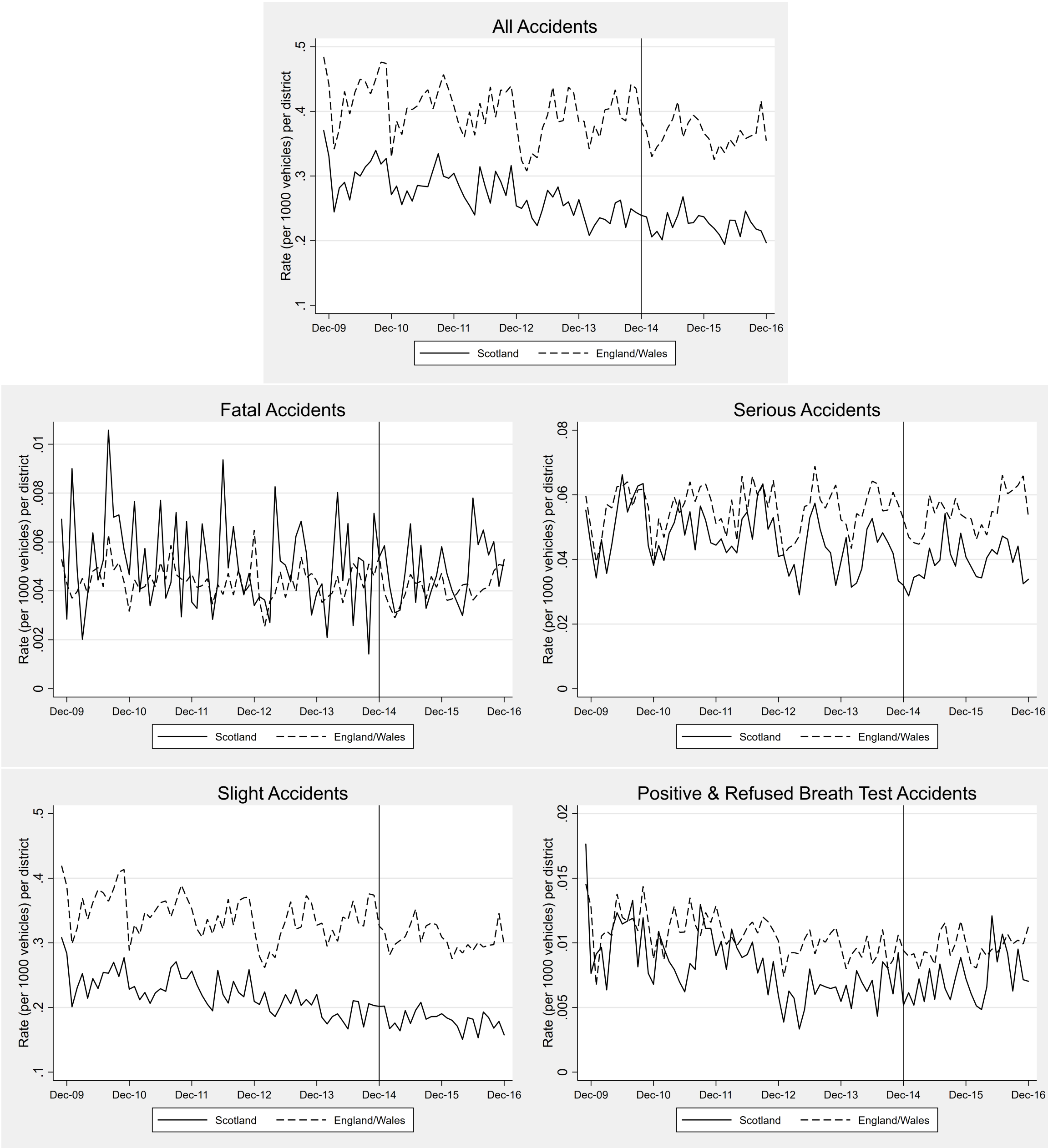
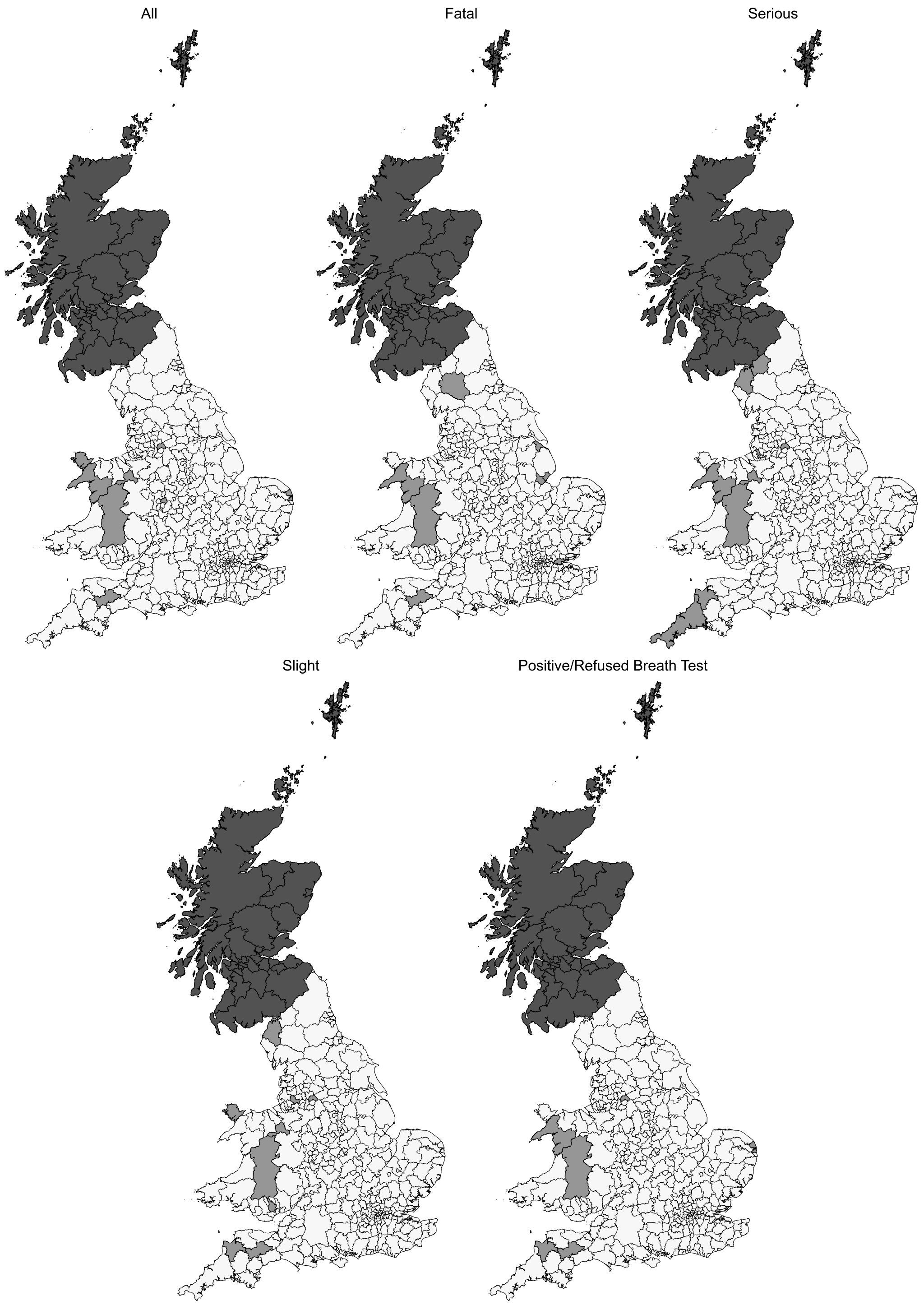
Figures and Tables

Figure 1: Trends in Road Accident Rates: Scotland versus the Rest of Britain



Sources: Road Accidents Data, Department for Transport, STATS19. Vehicle Licensing Statistics, Department for Transport.

Figure 2: Map of Great Britain: Scotland versus Synthetic Scotland



Notes: Local authority districts in dark grey identify Scotland. Local authorities in light grey make up synthetic Scotland. These are as follows (weight ω_c in parentheses):
All: Oldham (0.049), Walsall (0.057), Great Yarmouth (0.056), Castle Point (0.083), Mid Devon (0.216), Isle of Anglesey (0.155), Gwynedd (0.097), Wrexham (0.168), Powys (0.12).
Fatal: Eden (0.083), North East Lincolnshire (0.258), Boston (0.067), Castle Point (0.042), Thurrock (0.184), Mid Devon (0.092), Gwynedd (0.155), Powys (0.12).
Serious: Allerdale (0.07), Carlisle (0.218), Knowsley (0.061), Oldham (0.259), Torridge (0.099), Cornwall (0.063), Gwynedd (0.06), Wrexham (0.109), Powys (0.06).
Slight: Allerdale (0.096), Oldham (0.106), Wigan (0.105), Mid Devon (0.082), Torridge (0.056), Isle of Anglesey (.088), Wrexham (0.167), Caerphilly (0.142), Powys (0.158)
Positive/Refused Breath Tests: Oldham (0.292), Great Yarmouth (0.149), Castle Point (0.01), Mid Devon (0.142), Torridge (0.091), Gwynedd (0.309), Powys (0.006).

Figure 3: Trends in Road Accident Rates, by Type: Scotland versus Synthetic Scotland

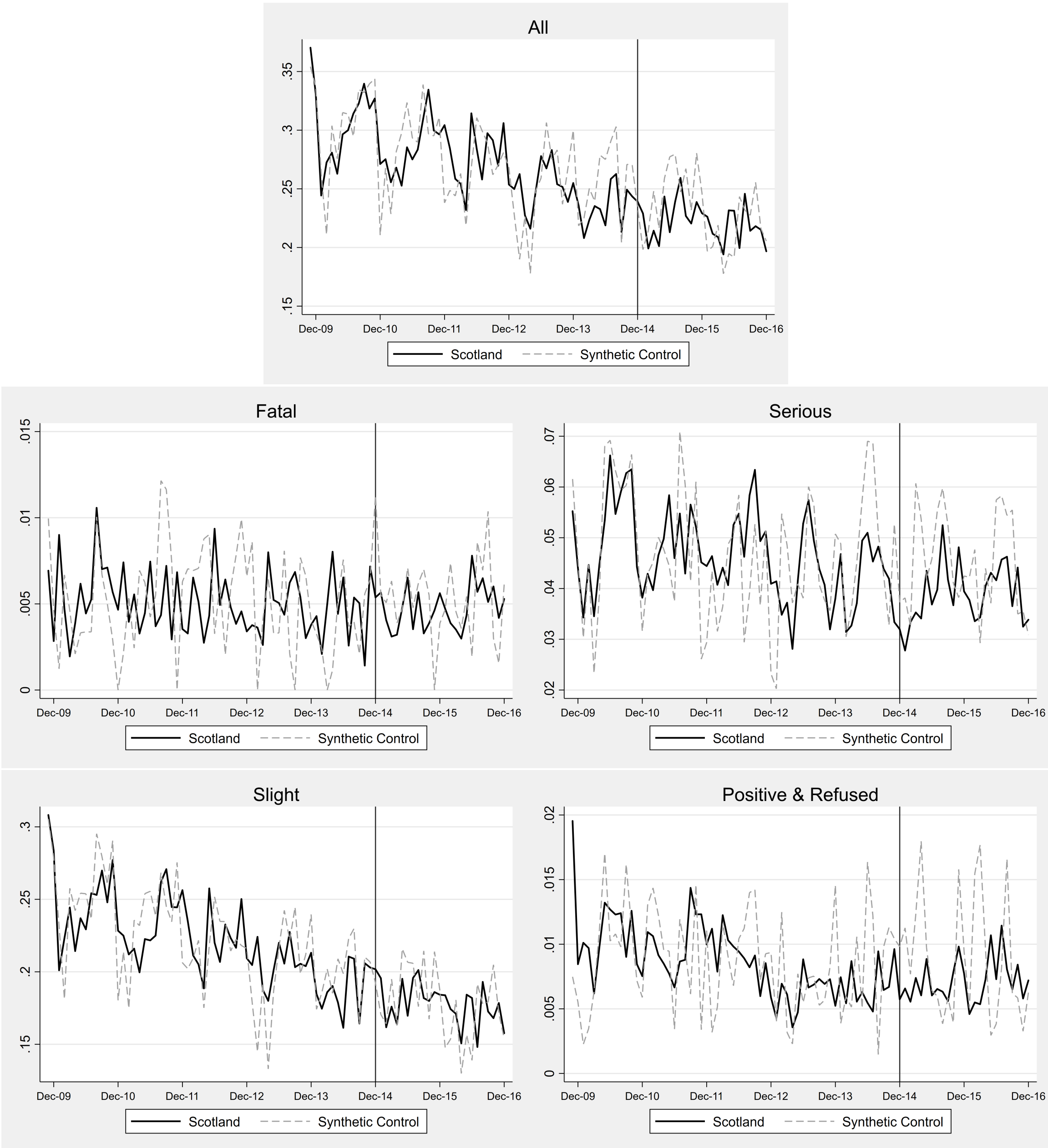
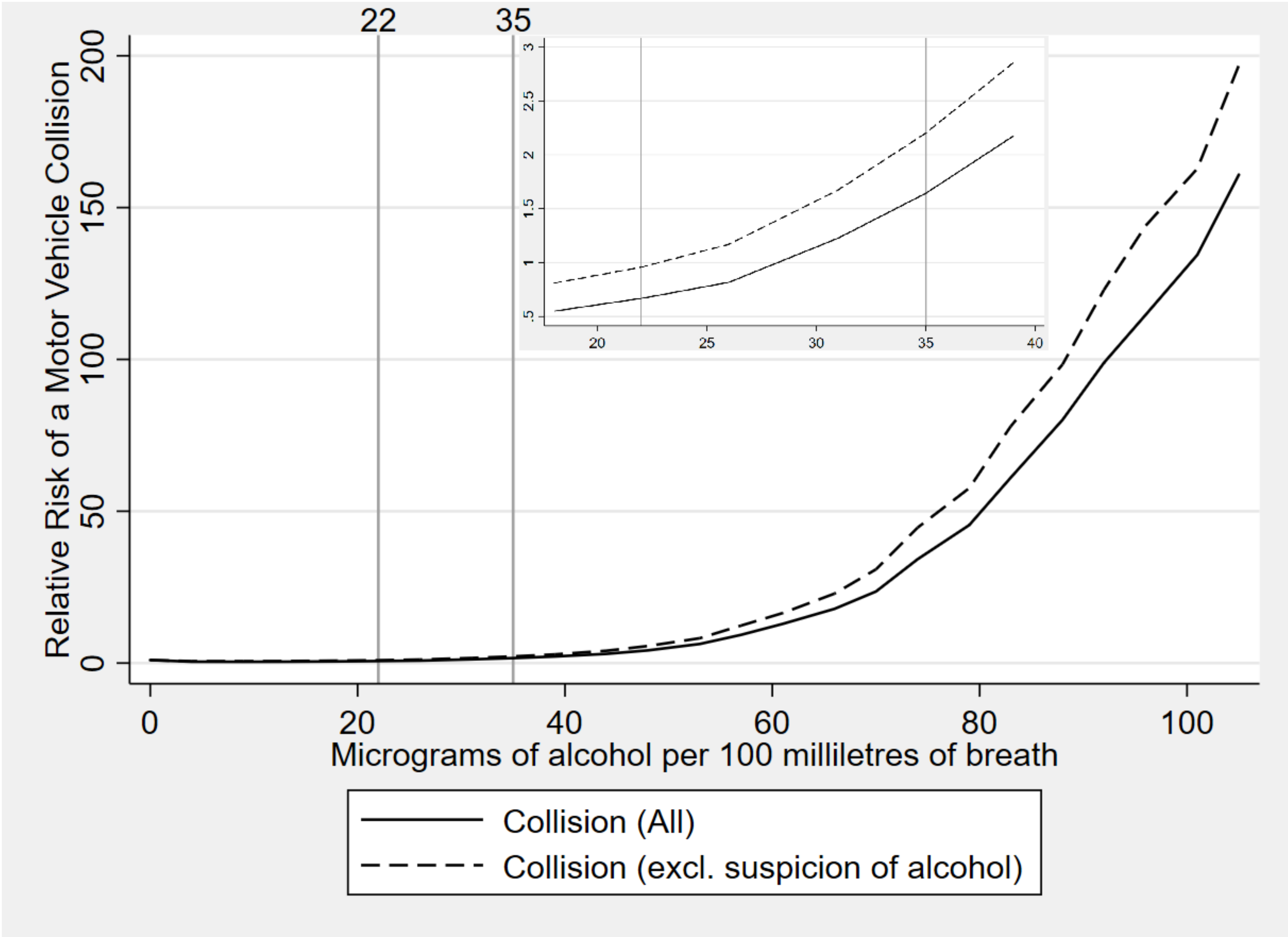


Figure 4: Adjusted Relative Risk of a Road Traffic Collision



Source: Road Safety Data, Digital Breath Tests, 2009–2014 (England and Wales).
Note: The breath test screening information comes from digital breath testing devices, as provided by police forces in England and Wales. The reasons for police-administered digital breath tests are: moving traffic violation; other road code violation (e.g., illegal parking); road traffic collision; suspicion of alcohol. The relative risk of a collision is calculated by multiplying the proportion of breath tests administered by the relative risk of a crash estimated by Compton et al. (2002), normalising the relative risk to 1 for cases in which no alcohol is consumed. The two vertical lines are drawn in correspondence to the old and new DDLs (35 and 22μg, respectively, or equivalently 0.08 and 0.05 BAC). The inset zooms in on the interval 17 and 38 BAC, which includes the two limits of interest.

Table 1: Pre-treatment Characteristics in Scotland, England and Wales, and Synthetic Scotland

	All LAs		LAs <100km		Synthetic Scotland				
	Scotland	England and Wales	Scotland	England and Wales					
Accident rates (per 1,000 vehicles)[a]									
All accidents	0.272	0.403	0.299	0.390	0.274				
	0.031		*0.000*						
Fatal accidents	0.005	0.004	0.004	0.006		0.005			
	0.595		*0.418*						
Serious injury accidents	0.046	0.056	0.047	0.052			0.046		
	0.328		*0.462*						
Slight injury accidents	0.221	0.343	0.247	0.332				0.223	
	0.021		*0.000*						
Positive/refused breath test accidents	0.009	0.011	0.008	0.012					0.009
	0.309		*0.067*						
Controls									
Temperature range (°C)[b]	6.828	7.688	6.971	7.207	7.186	7.412	6.940	7.007	7.123
	0.000		*0.340*						
Population density (pop./ha)[c]	4.038	12.69	4.326	7.108	4.022	4.035	4.576	4.039	4.635
	0.000		*0.006*						
Road length (km)[d]	1873	966.8	1836	1,617	1876	1685	1871	1872	1651
	0.000		*0.000*						
No qualifications (%)[e]	26.85	22.47	27.51	24.88	26.01	26.48	26.82	26.85	26.68
	0.000		*0.000*						
Very bad/bad health (%)[e]	0.053	0.054	0.054	0.066	0.059	0.054	0.065	0.067	0.060
	0.431		*0.000*						
Median working hours[f]	35.88	36.76	35.92	37.02	36.63	36.54	36.58	36.87	36.03
	0.000		*0.000*						
Job Seeker's Allowance (%)[f]	3.509	3.027	3.863	3.552	3.265	3.510	3.505	3.510	3.618
	0.009		*0.055*						
Nr. of licensed premises[g]	526.7	584.3	594.7	712.6	527.3	553.7	729.4	590.3	682.8
	0.282		*0.146*						

Sources: [a] Road Accident Statistics STATS19 Department for Transport; [b] Met Office; [c] Office for National Statistics; [d] Department for Transport; [e] 2011 Census; [f] NOMIS (www.nomisweb.co.uk/); *g* Department for Culture Media and Sport, the Home Office, and the Scottish Government.

Notes: Italicized numbers are p-values of the t-test of equality between groups in the relevant columns. 'Temperature range' is in degrees Celsius at the month-Met Office region level (9 regions). 'Population density' is defined as the population aged 17 or more divided by the area (in hectares) and is measured at the annual level by local authority (LA). 'Road length' is the total road length (in kilometres) measured annually at the LA level. 'No qualifications' is defined as the percentage of usual residents aged 16 or more with no qualifications measured at the 2011 Census. 'Very bad/bad health' is the percentage of all usual residents with bad or bad good health measured at the 2011 Census. 'Job Seeker's Allowance' is the percentage of the LA resident population aged 16–64 claiming Job Seeker's Allowance every month. 'Nr. of licensed premises' is the yearly number of premises registered in the LA with a legal license to sell alcohol.

Table 2: Effect of the DDL Reform on Road Accident Rates — Difference-in-Difference Estimates

	Mean	(a)	(b)	(c)	(d)	(e)
		A. All Accidents				
β	0.272	-0.0130	-0.0113	0.0208*	0.0112	0.0015
		(0.0070)	(0.0096)	(0.010)	(0.0091)	(0.0068)
		B. Fatal Accidents				
β	0.005	-0.0001	-0.0001	0.0005	0.0006	0.0005
		(0.0004)	(0.0004)	(0.0008)	(0.0008)	(0.0008)
		C. Serious Injury Accidents				
β	0.046	-0.0061*	-0.0062*	0.0028	0.0028	0.0015
		(0.0017)	(0.0019)	(0.0033)	(0.0031)	(0.0023)
		D. Slight Injury Accidents				
β	0.221	-0.0069	-0.0051	0.0175*	0.0079	-0.0004
		(0.0061)	(0.0082)	(0.0073)	(0.0068)	(0.0071)
		E. Accidents with Positive/Refused Breath Test				
β	0.009	-0.0006	-0.0006	0.0013	0.0014	0.0014
		(0.0006)	(0.0006)	(0.0009)	(0.0009)	(0.0009)
		E1. Positive Breath Test				
β	0.008	-0.0010	-0.0010	0.0007	0.0008	0.0008
		(0.0005)	(0.0005)	(0.0009)	(0.0009)	(0.0009)
		E2. Refused Breath Test				
β	0.001	0.0004	0.0004	0.0006*	0.0007*	0.0006*
		(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0003)
Observations		32,508	32,508	32,508	32,508	32,508
Scottish LAs		31	31	31	31	31
English/Welsh LAs		347	347	347	347	347
All LAs		378	378	378	378	378
Controls		N	Y	Y	Y	Y
Month-year trend		N	N	Y	Y	Y
Month year trend $\times$ Scotland		N	N	Y	Y	Y
Month FEs		N	N	N	Y	Y
Month FEs $\times$ Scotland		N	N	N	Y	Y
LAs fixed effects		N	N	N	N	Y

Notes: Observations are at the LA-month-year level. The dependent variable is the number of accidents per 1,000 registered vehicles. The sample period goes from November 2009 to December 2016. Standard errors in parentheses are clustered at the LA level. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. 'Controls' are LA monthly averages of temperature range, population density, proportion of residents aged 16 or more with no educational qualification, proportion of residents with bad or very bad health, median total hours worked, median gross pay, Job Seekers' Allowance rate, alcohol licensed premises, and total road length (see the text and the note to Table 1 for more details). 'LAs' denotes local authorities, 'FEs' denotes fixed effects. * $p < 0.05$

Table 3: Effect of the DDL Reform on Road Accident Rates — Spatial Regression Discontinuity Estimates

	(1) All	(2) Fatal	(3) Serious	(4) Slight	(5) Positive/Refused Breath Test
<200km	0.0031	0.0013	-0.0004	0.0022	0.0018
	(0.0078)	(0.0008)	(0.0027)	(0.0073)	(0.0012)
	[0.686]	[0.110]	[0.896]	[0.768]	[0.178]
Mean	0.286	0.005	0.048	0.232	0.009
<100km	0.0079	0.0019	-0.0037	0.0097	0.0012
	(0.0150)	(0.0017)	(0.0041)	(0.0148)	(0.0027)
	[0.622]	[0.358]	[0.427]	[0.548]	[0.702]
Mean	0.299	0.004	0.047	0.247	0.0084
<50km	0.0274	0.0044	-0.0086	0.0317	0.0018
	(0.0202)	(0.0032)	(0.0067)	(0.0245)	(0.0041)
	[0.362]	[0.367]	[0.312]	[0.404]	[0.719]
Mean	0.284	0.006	0.055	0.224	0.008

Notes: Observations are at the LA-month-year level. The dependent variable is the number of accidents per 1,000 registered vehicles. The sample period goes from November 2009 to December 2016. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. Standard errors in parentheses are clustered at the LA level. Due to the small number of LAs, wild bootstrapped p-values computed using Webb weights (Webb, 2014) and 5,000 replications are in square brackets. For completeness, however, these are shown also for large bandwidths. The numbers of Scottish LAs are 27, 12, and 3 in the first, second, and third row, respectively. The corresponding numbers for England/Wales are 69, 14, and 4, respectively. From the top to the bottom panel, the numbers of observations are 8,256, 2,236, and 602. Besides the set of controls reported in the notes to Table 2, distance from the Scottish/English border and distance from the border interacted with Scotland (with English distances taking negative values) are also included. All regressions are weighted by proximity to the border using a triangular kernel.

Table 4: Heterogeneity in the Synthetic Control Effects

	All	Fatal	Serious	Slight	Pos./refused
Baseline	0.007	-0.001	-0.007	0.004	-0.003
	(0.386)	(0.87)	(0.51)	(0.447)	(0.217)
8am-8pm	0.007	-0.001	-0.007	0.004	-0.003
	(0.383)	(0.873)	(0.499)	(0.441)	(0.214)
8pm - 8am	-0.002	0.000	-0.002	-0.004	-0.001
	(0.265)	(0.741)	(0.657)	(0.207)	(0.312)
Sat/Sun	0.000	0.000	-0.002	0.001	-0.001
	(0.96)	(0.625)	(0.277)	(0.81)	(0.543)
Fri/Sat/Sun	0.000	-0.001	-0.002	0.005	-0.001
	(0.818)	(0.568)	(0.401)	(0.781)	(0.396)
Aged 18-30	0.006	0.000	-0.004	0.005	-0.001
	(0.582)	(0.772)	(0.19)	(0.888)	(0.272)
Aged 50+	-0.006	-0.001	-0.002	-0.003	0.000
	(0.383)	(0.441)	(0.568)	(0.429)	(0.254)
Male	0.003	-0.001	-0.009	0.003	-0.003
	(0.524)	(0.68)	(0.291)	(0.697)	(0.217)
Female	0.009	-0.001	0.000	0.009	-0.001
	(0.231)	(0.144)	(0.862)	(0.265)	(0.104)
Male × 8pm - 8am	-0.004	0.000	-0.003	-0.004	0.000
	(0.282)	(0.585)	(0.434)	(0.256)	(0.236)
Male × 8pm - 8am × sat/sun	-0.001	0.000	-0.001	-0.004	-0.001
	(0.559)	(0.38)	(0.579)	(0.193)	(0.458)
8pm-8am × Sat/Sun	0.000	0.000	-0.001	-0.003	-0.001
	(0.862)	(0.418)	(0.504)	(0.375)	(0.553)
Male × Sat/Sun	-0.001	0.000	-0.002	0.000	0.000
	(0.997)	(0.614)	(0.349)	(0.905)	(0.504)
Male × 18-30 × 8pm-8am	-0.003	0.000	-0.001	-0.002	-0.001
	(0.144)	(0.648)	(0.565)	(0.464)	(0.092)
Male × 18-30 × 8pm-8am × Sat/Sun	-0.002	0.000	-0.001	-0.002	-0.001
	(0.314)	(0.312)	(0.421)	(0.401)	(0.225)
One vehicle	-0.008	0.000	-0.004	-0.007	0.001
	(0.45)	(0.524)	(0.202)	(0.674)	(0.772)
Two+ vehicles	0.008	0.000	-0.004	0.011	-0.002
	(0.392)	(0.697)	(0.202)	(0.357)	(0.682)
Urban	0.032	0.000	-0.004	0.038	0.000
	(0.315)	(0.999)	(0.935)	(0.087)	(0.451)
Rural	-0.012	-0.001	-0.006	-0.004	-0.004
	(0.607)	(0.497)	(0.785)	(0.840)	(0.191)
Licences (above the median)	-0.009	0.000	-0.008	-0.005	-0.002
	(0.842)	(0.492)	(0.412)	(0.898)	(0.667)
Licences (below the median)	-0.010	-0.002	-0.007	-0.009	-0.002
	(0.494)	(0.706)	(0.618)	(0.535)	(0.260)

Notes: Each point estimate is the average treatment effect in the 25 months after the change in the drink drive law. In parenthesis are psuedo standardised p-values. These are the proportion of effects from control units that have a post-treatment RMSPE at least as great as the treated unit scaled by the corresponding pretreatment RMSPE.

Table 5: Effect of the DDL Reform on Attitudes toward Drink Driving — Difference-in-Difference Estimates

	Mean	(a)	(b)	(c)
		A. Should Not Drive If Drunk		
β	0.899	0.073*	0.085*	0.097*
		(0.027)	(0.027)	(0.042)
		B. DDL Knowledge		
β	0.755	0.006	0.004	0.034
		(0.042)	(0.042)	(0.061)
Observations		7,329	7,329	7,329
Controls		N	Y	Y
Linear annual trend		N	N	Y
Linear annual trend × Scotland		N	N	Y

Source: British Social Attitudes Surveys, 2009–2016.
Notes: Observations refer to the number of individuals in the sample. The dependent variables take value 1 if agreeing with the following statements: "If anyone has drunk any alcohol they should not drive?" (panel A, Should Not Drive If Drunk), "Most people don't know how much alcohol before being over legal limit?" (panel B, DDL Knowledge), and 0 otherwise. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. Robust standard errors in parenthesis. In both panels, 'controls' are: age, sex, education (degree or more, higher education qualifications, A-levels (or equivalent), GCSE/O-levels (or equivalent), or foreign qualifications, with no qualification as the base category), ethnic origin (White, Black, or Asian, with others as the base category), and married/cohabiting.
* $p < 0.05$.

Table 6: Effect of the DDL Reform on Alcohol Consumption — Difference-in-Difference Estimates

	(a)	(b)	(c)	(d)	(e)	(f)
	A. Units (on Heaviest Day)			B. Days Drank		
β	0.208*	0.208*	0.053	0.098*	0.070*	-0.024
	(0.081)	(0.077)	(0.112)	(0.033)	(0.032)	(0.045)
Mean		3.94			2.10	
Observations	128,898	128,898	128,898	118,176	118,176	118,176
	C. Units Usually Drunk per Week			D. 10+ Units		
β	0.498	0.477	0.530	0.006	0.007	-0.008
	(0.290)	(0.282)	(0.485)	(0.005)	(0.005)	(0.007)
Mean		10.80			0.128	
Observations	79,558	79,558	79,558	128,898	128,898	128,898
Controls	N	Y	Y	N	Y	Y
Linear annual trend	N	N	Y	N	N	Y
Linear annual trend × Scotland	N	N	Y	N	N	Y

Sources: Health Survey of England (England) and Scottish Health Surveys (Scotland), 2008–2016.
Notes: Observations correspond to the number of individuals over the sample period. The dependent variables are: the number of alcohol units drunk on heaviest day in the previous 7 days (top left); the number of days the interviewee drank over the past 7 days (top right); the number of alcohol units usually drunk per week, conditional on drinking, available in both England and Scotland from 2011 (bottom left); drinking 10 units of alcohol or more on the heaviest day (bottom right). 'Mean' refers to the Scottish pre-reform mean of the dependent variable. Robust standard errors are in parentheses. In all panels, 'controls' are: indicators of sex, marital status (married/cohabiting), ethnic minority (White, Black, or Asian, with others as the base category), education (leaving school at age 17 or after), and age (15 3-year age band groups). * $p < 0.05$.