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Alcohol Price Floors and Externalities: The Case of Fatal Road Crashes

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Abstract

In May 2018, Scotland introduced a minimum unit price on alcohol. We examine the impact of this policy on traffic fatalities and drunk driving accidents. Using administrative data on the universe of vehicle collisions in Britain and a range of quasi-experimental modeling approaches, we do not find that the policy had an effect on road crash deaths and drunk driving collisions. The results are robust to several sensitivity exercises. There is no evidence of effect heterogeneity by income and other predictors of alcohol consumption or cross-border effects. A brief discussion of the policy implications of our findings is provided.

JEL-Codes: D120, D620, H230, K420, R410.

Keywords: externality, alcohol, minimum unit pricing, motor vehicle collisions, driving under the influence.

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1. Introduction

One in three people drink alcohol worldwide. In 2016, alcohol use was the leading global risk factor for both deaths and disability for those aged between 15 and 49, accounting for 4% and 12% of the total death toll for women and men, respectively. The main causes of alcohol-related deaths in this age group include road injuries, self-harm, and tuberculosis (Griswold et al., 2018). To reduce the negative externalities from alcohol consumption, the World Health Organization (WHO) in 2018 launched their SAFER initiative, which rests on five components.¹ One of those components is to raise prices on alcohol through ethanol taxes and pricing policies (WHO, 2018). On May 1, 2018, Scotland (but not the rest of the United Kingdom) introduced a minimum unit price (MUP) on alcohol purchases at 50 pence per unit.² The objective of this paper is to provide a comprehensive analysis of the Scottish MUP impact on fatal road accidents, one of the main alcohol consumption externalities.

Scotland is the first nation in the world to have introduced a minimum unit price for alcohol.³ Price floors can lower socially costly alcohol intake by raising ethanol prices, but, unlike higher taxes, they may create windfall profits for firms instead of raising tax revenue. This is the main reason why they typically have not been favored by economists (Griffith and Leicester, 2010). But price floors could be effective if a large fraction of heavy drinkers who buy cheap alcoholic beverages (such as can-packaged beer and alcopops) are willing to move away from their usual consumption without switching to other more expensive drinks (e.g., wine and spirits), or make the switch while reducing consumption.

At present, there are only two papers that have assessed the consequences of the Scottish MUP reform on alcohol purchases, but not on road accidents. Using scanner data on 30,000 British households and a difference-in-difference approach with English households used as a control group, O'Donnell et al. (2019) find that the MUP led to an immediate increase in the purchase price of off-trade alcohol by 8% and to a fall in the amount of alcohol purchased by about 7.6%. They also find that the price increase was concentrated among lower income households and in households that purchased the largest amount of alcohol, and the reduction in purchases was greater at the bottom of the household income distribution. Griffith et al. (2022) use the same data and statistical design as the previous study and broadly confirm the results on alcohol prices and quantity

¹The five pillars of the SAFER initiative are: *Strengthen* restrictions on alcohol availability (S); *Advance and enforce* drink driving counter measures (A); *Facilitate* access to screening, brief interventions and treatment (F); *Enforce* bans or comprehensive restrictions on alcohol advertising, sponsorship, and promotion; and *Raise* prices on alcohol through excise taxes and pricing policies (R). See <<https://www.who.int/substance-abuse/safer/en/>>.

²The UK is made up of four “nations” (England, Scotland, Wales and Northern Ireland). Some policies are common across the four nations, while others (including those on alcohol prices) are devolved.

³In March 2020, Wales followed Scotland and implemented the same alcohol floor policy (see the next section). In October 2018, Ireland also passed similar legislation, which however has not been formally enacted yet. Moreover, a number of Canadian provinces have a system of alcohol price floors in place, although these have been typically introduced as a way to limit the competition faced by state owned retailers from private competitors and have not been extended nationally.

of alcohol purchased.⁴ In addition, they find that the larger quantity reductions occurred among heavy drinkers, because such drinkers consumed a greater share of alcohol from products previously priced below the floor. This has important policy implications driven by consumption externalities. If the marginal external cost of drinking is higher for heavy drinkers, then a price floor could achieve larger welfare gains than an ethanol tax, because the MUP — being better targeted at heavy drinkers — may yield a stronger reduction in external costs and more than offset lower tax revenues.⁵

Negative externalities from ethanol intake, however, are related not only to alcohol purchases or consumption, but also to other life domains. The main contribution of this paper is to evaluate for the first time whether the MUP policy had an effect on road traffic fatalities, an extreme form of harm.⁶ This specific line of enquiry is motivated by the well documented positive dose-response relation between blood alcohol concentration and fatal car crashes (e.g., Levitt and Porter, 2001; Francesconi and James, 2019). The findings by O’Donnell et al. (2019) and Griffith et al. (2022) that the MUP reform reduced off-trade alcohol purchases in Scotland, therefore, provide our starting point. If lower purchases due to the price floor translate into lower consumption, and drivers are more likely to be sober as a result, we expect to observe a reduction in road fatalities *ceteris paribus*. In addition, if heavy drinkers are disproportionately affected as documented by Griffith et al. (2022), this expected reduction should be even more likely, since heavy drinkers are known to be more dangerous on the road than other drivers (Levitt and Porter, 2001; Grant, 2010; Francesconi and James, 2019).

A concern could be that the Scottish MUP intervention has limited scope for our exercise if traffic fatalities and drunk driving violations are caused overwhelmingly by consumption in pubs, bars, and restaurants. There is, however, a wealth of cross-country evidence showing that most ethanol intake is through off-license purchases, including Britain (e.g., Gray-Phillip et al., 2018). Moreover, a large fraction of drunk driving offenses and road traffic fatalities are attributable to individuals who drink alcohol bought off-trade and consumed in unlicensed premises, e.g., at home and private parties, rather

⁴Using similar data but with a greater level of geographic aggregation, Xhurxhi (2020) finds that the alcohol floor led to a significant reduction in off-premise sales of about 5% on average.

⁵Besides these two works, the other existing research is about the system of already mentioned price floors introduced in a number of Canadian provinces. Confirming the results from the Scottish MUP reform, this research finds a link between minimum pricing and lower alcohol consumption (e.g., Stockwell et al., 2012a, 2012b). It also shows that minimum alcohol pricing is negatively associated with hospitalizations (Zhao and Stockwell, 2017) and with alcohol-related traffic violations (Stockwell et al. 2015), with some studies finding a decrease in night-time alcohol-related traffic offenses among men but not women (Stockwell et al. 2017), and other studies observing a decline in hospital admissions among women but not men (Sherk et al., 2018). All this research on the Canadian price floors is essentially correlational.

⁶We see traffic crash deaths as a negative externality of drunk driving. They are “internalities” when drunk drivers inflict that harm on themselves that they do not internalize because, for example, they are misinformed about the effects of alcohol on their ability to drive. From a social welfare perspective, the distinction between externalities/internalities and health harms (a result of a rational decision) is unimportant when analyzing fatal car collisions. For a useful general discussion of these notions, see Allcott et al. (2019a, 2019b).

than pubs (Lang and Stockwell, 1991).⁷ Much public health research has emphasized a specific age pattern in the locations where drunk drivers have consumed alcohol, according to which young adults tend to drink away from home, such as bars but also car parks (where they would typically consume alcohol sold off-trade), while older drunk drivers drink at home or at friends' houses more frequently (Walker et al., 2005; Morrison et al., 2002). There is also evidence of drinkers who pre-load (i.e., they drink heavily at home before going to pubs or nightclubs) and are likely to engage in drunk driving (Miller et al., 2013). If the MUP reform curbs this type of drinking behavior across all types of consumers, it could unambiguously reduce drink drive collisions.

Using official administrative data on the universe of vehicle collisions observed in Britain between November 2009 and December 2019, and estimating several specifications of difference-in-difference and synthetic control models, we do not find that the MUP has an effect on fatal road crashes. We also do not find an impact on drunk driving accidents, as well as on serious- and slight-injury collisions. Our preferred estimates from synthetic control methods reveal an increase of 8% in fatal road accidents, which goes against theory, and a reduction in drunk driving accidents of around 2%. Both impacts are statistically insignificant. These estimates are robust to several alternative definitions of the outcome variable and different functional forms used in estimation.

As there is evidence of an effect of the Scottish price floor on alcohol purchases among heavy drinkers and poor households, we consider the possibility of heterogeneous effects of the reform on motor vehicle accidents along dimensions where we expect differential ethanol intake, such as drivers' income, age, and gender, as well as times of collisions (hours of the day and days of the week). There is no evidence that the price floor has an impact on road crash deaths along any of these domains. We also do not find evidence of cross-border effects, with lower accident rates in the areas close to the Scottish/English border and larger effects further away. Our estimates reject the presence of these leakage effects of the reform. Our null estimates are in line with recent studies based on natural experiments which have also found no effect of other alcohol control policies on traffic fatalities (e.g., McClelland and Iselin, 2019). Strengthening this point, a comprehensive review by Roodman (2015) suggests that the prior consensus that higher alcohol prices translate into fewer fatal crashes is breaking down.

Our results have implications for policy and future research. Explaining why the MUP reform, which curtails ethanol intake, does not translate into lower fatal and drunk driving crash rates is important but goes beyond the scope of the paper. This will require, for example, a thorough analysis of the potential changes in driving habits among alcohol consumers, the availability of alternative means of transportation, and law enforcement.

Even though the evidence indicates that the alcohol price floor is ineffective to correct the externality generated by alcohol-related car crashes, it is important to keep in mind

⁷Some of the mechanisms used to explain this result include a higher ethanol tolerance among those who drink in pubs and bars and a greater police attention to drivers leaving licensed premises.

there might be other short-term negative externalities (e.g., crime) and longer term harm on health that could be sensitive to the price floor. More research is needed to test the existence of such links. Furthermore, the price floor is just one of the policy tools to harness alcohol-related harm on the road. Its success might be realized only with the introduction of other policies, such as information and awareness campaigns targeted to individuals who are likely to generate the largest externalities, as well as accident-preventing law enforcement and efficient police deployment on the road.

Related Literature — Our work contributes to the economic literature that examines the impact of alcohol control policies on road traffic collisions. Here we flesh out a brief summary of the major studies in this area of research, but a broader review is not provided due to space concerns.⁸ Alcohol control policies are usually motivated by the goal of reducing alcohol-related harm. Section 2 will explain that this is also the case for the MUP reform in Scotland. Two of the key drivers of harm identified by policy makers are affordability (prices and taxes) and availability (how easy it is to purchase alcohol).

Since alcohol affordability is perceived to be a major determinant of alcohol consumption and harm (OECD, 2015), taxation and price regulation are the main policy tools used to affect affordability. As mentioned above, there is only limited causal evidence on the link between alcohol prices and motor vehicle accidents.⁹ Ours is the first study to provide this causal evidence systematically using a credible source of an exogenous change in alcohol prices. Most of the existing literature instead focuses on changes in alcoholic-beverage taxes. Early studies find that higher beer excise taxes are associated with reductions in traffic crash deaths (e.g., Chaloupka et al., 1993; Ruhm, 1996).¹⁰ More recent contributions, however, cast doubt on this result and find no causal impact of alcohol taxes on traffic fatalities (e.g., Dee, 1999; McClelland and Iselin, 2019).

Besides weak identification, critical assessments of the early literature include the typically small size of the tax rate changes compared to the average alcohol price (Dee, 1999; DiLoreto et al., 2012) and the leeway due to avoidance behavior (Gehrsitz et al., 2021). Although not referring to traffic collisions necessarily, this last point emphasizes

⁸As several alcohol control policies have been justified to limit alcohol harm across a variety of life domains, we also do not review the contributions that focus on outcomes other than road accidents, such as work absenteeism, school performance, hospitalizations, health status, mortality, and crime. For a comprehensive coverage of economic studies, see Cook and Moore (2000), Cawley and Ruhm (2011), and Sloan (2020). Other important non-economic reviews include Martineau et al. (2013) and Burton et al. (2017).

⁹We reiterate that several studies on the MUP reforms introduced in various Canadian provinces and mentioned above are correlational. An additional study by Young and Bielinska-Kwapisz (2006) uses alcohol taxes as instrumental variables for the endogeneity of prices. It shows that traffic fatalities are negatively related to alcohol prices in the US but admits that the results may still be biased because taxes are not entirely suitable as instruments.

¹⁰Large meta-analysis reviews confirm this inverse relationship and suggest that doubling alcohol taxes reduces fatal road collisions by an average of 11% (e.g., Wagenaar et al., 2010; Xu and Chaloupka, 2011). This inference is drawn primarily from correlational studies. See also the study by Elder et al. (2010), which provides a comprehensive review of the literature to assess the effectiveness of alcohol tax (but not price) policy interventions for reducing excessive alcohol consumption and related harms.

that, after hikes in alcohol excise taxes which affect some types of beverages more than others, consumers might not reduce ethanol intake but simply switch to less expensive products whereby defying the alcohol control policy. In the same vein, Griffith et al. (2019) show that varying tax rates across different forms of alcohol can lead to large welfare gains relative to a single ethanol tax rate.

The second commonly recognized driver of harm is alcohol availability. Interventions that regulate availability are generally based on the argument that easier access to alcohol increases both consumption and negative externalities. Such interventions include a wide set of policies, from alcohol sale restrictions in specific days (e.g., Sundays) to night closing hours for bars and restaurants, and from bans on off-premise alcohol sales to establishment entry regulations in the liquor market. The evidence on road accidents is mixed. The estimates in Biderman et al. (2010) suggest that restricting night alcohol access through mandatory closing hours to bars and restaurants in Brazil reduces fatal car crashes. Green et al. (2014), instead, find that the liberalization (extension) of bar closing times in England and Wales leads to lower fatal accidents, while Lovenheim and Steefel (2011) document that Sunday alcohol sales restrictions have virtually no effect on road deaths.

If we extend the focus to other externalities, however, the evidence becomes clearer. For instance, Heaton (2012) finds that relaxing Sunday alcohol sales restrictions increases both minor crime and alcohol-involved serious crime rates. Night bans on alcohol sales at off-premise outlets (e.g., supermarkets and gas stations) reduce alcohol-related hospitalizations among adolescents and young adults (Marcus and Siedler, 2015). Greater alcohol availability through expansions in the number of licensed drinking establishments is also found to have a negative externality by increasing violent crimes (Anderson et al., 2018).

Finally, the policy landscape is populated by many other public interventions aimed at moderating the susceptibility of individual risk factors to alcohol-induced road accidents. These include drink drive limits (DDL), which set the maximum amount of alcohol an individual can have to be allowed to drive, and regulations on the minimum legal drinking age, which set the minimum age at which young adults can lawfully purchase and consume alcohol.¹¹ The findings in Carpenter and Dobkin (2009 and 2011) indicate a large increase in mortality rates at age 21 when drinking becomes legal in the United States, primarily due to motor vehicle accidents. Carpenter et al. (2016) confirm this result for Canada.¹² In the case of DDL restrictions, the evidence is more ambiguous. Dee (2001) and Albalade

¹¹Other interventions include industry regulations on alcohol marketing and labelling of alcoholic beverages, social and mass media campaigns to shape social norms, and education programs in school and higher education settings. For further discussion on these, and other, policies and their effectiveness on alcohol consumption, see Burton et al. (2017) and Eisenberg (2003).

¹²These results are also broadly confirmed by Fletcher (2019), who finds large detrimental effects of alcohol access on drink driving, violence, and other risky behaviors, especially among men. 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 et al. (2016) also find no evidence that legal access to alcohol has effects on motor vehicle collisions of any type in New South Wales.

(2008) find that lowering the limit reduces fatal accident rates in the US and in Europe, respectively. Carpenter (2004) analyzes the impact of zero-tolerance laws for the young, which set stricter DDL for individuals under age 21 in the US, and documents that these laws do not affect drunk driving. Using a reduction in the DDL in Scotland in 2014, Cooper et al. (2020) and Francesconi and James (2021) also find evidence of no effect on drink driving and road collisions.

Another strand of research relevant to our work is the literature on externality-correcting policies, including the burgeoning body of work on the impact of “sin taxes” imposed to discouraged behaviors, such as smoking cigarettes or drinking alcoholic and sugar-sweetened beverages, that are thought to harm individuals and society (see Allcott et al. [2019a] for a review). The fact that we find no impact of the Scottish alcohol price floor reform on road accidents opens up a number of policy issues that go beyond alcohol control interventions. We shall return to this point in the final section.

The rest of the paper is organized as follows. In the next section, we outline the institutional background surrounding the introduction of the MUP reform, which helps to understand our focus. This is fully developed in Section 3. Section 4 describes the data and the econometric framework used in the policy evaluation. Section 5 presents the results, checks for heterogeneous effects, and shows the estimates from a broad set of sensitivity exercises. Section 6 concludes. Supplementary material on the empirical results is in the Online Appendix.

2. Background

On the 1st of May 2018, Scotland was the first nation in the world to enforce a price floor for alcohol, known as the minimum unit price (MUP). This policy, which did not apply to England and Wales, makes it unlawful to sell alcohol products priced below a floor equal to £0.50 per unit of alcohol.¹³ A unit of alcohol is 10ml of ethanol, which is the standard metric in the UK and many other European countries. The MUP was legislated by the Scottish Parliament through the Alcohol Act in May 2012, but its implementation was delayed by a legal challenge.¹⁴ The six-year gap between the initial legislation and the actual passing of the law suggests that the precise timing of the reform was plausibly exogenous.

¹³As mentioned in the Introduction, a floor of £0.50 per unit of alcohol was also introduced in Wales on March 2, 2020. We do not consider this reform, however, for it is outside of our analysis period. Furthermore, the implementation of the Welsh floor happened during the outbreak of the Covid-19 pandemic and was accompanied by extensive “lockdown” policies, which largely restricted driving. It will be the focus of future research.

¹⁴The challenge from the Scotch Whisky Association was based on trade discrimination and was referred to the European Union Court of Justice. The response in December 2015 required Scottish judges to consider whether alternative tax policies were ineffective in protecting public health. After almost two years, on November 15, 2017, the UK Supreme Court rejected the case against the MUP proposal, arguing that minimum pricing was a proportionate means of achieving a legitimate aim.

The policy was introduced in order to reduce harm caused by alcohol, especially alcohol-related deaths and hospitalizations (Scottish Government, 2018a, 2018b). One of the main motivations for a price-based intervention was affordability. The Government estimated that alcohol was 64% more affordable in 2017 than it was in 1980.¹⁵ This indicates that firms did not adjust their prices upwards in anticipation of the reform after the 2012 Alcohol Act and before the actual introduction of the price floor. In Section 5, we will perform a sensitivity exercise on this possibility and rule out the impact of a possible pre-reform price change on road traffic outcomes.

Further analysis showed that affordability differed massively between off- and on-trade premises. Compared to 1980, alcohol sold in off-trade premises (e.g., supermarkets) was about 150% more affordable in 2017. The affordability of alcohol sold by on-trade premises (e.g., pubs and restaurants) was instead much more contained at about 30%, with on-trade alcohol prices increasing in real terms between 2008 and 2017 (Institute of Alcohol Studies, 2019). This indicates that the price floor was set at a level that increased the price of off-trade alcohol, but left on-trade alcohol prices relatively unchanged. This differential bite of the reform is important for the interpretation of our empirical results. We shall come back to this point in Section 4, where we set up the empirical analysis, and in the Conclusion.

The evidence from O’Donnell et al. (2019) and Griffith et al. (2022) documents that the MUP reform succeeded in reducing off-trade alcohol consumption. But this reduction per se does not necessarily tackle most of the key externalities that may arise from alcohol misuse. A vast medical literature shows that the positive dose-response relation we referred to in the Introduction emerges also across several health domains, including the risks of developing tuberculosis (Lönnroth et al., 2008), liver cirrhosis (Rehm et al., 2010), and cancer (Bagnardi et al., 2013). The impact of the MUP policy on this sort of externalities, however, is likely to become detectable only in the longer run.

Focusing on the effect on road fatalities and drink drive accidents, instead, may pick up externalities from alcohol consumption that are observable even in the short term. As shown in Figure 1, the incidence of road traffic deaths involving alcohol in Britain in 2017 was moderately low by international standards, and comparable to the rates observed for Chile, Norway, and Switzerland. Although higher than the rates in Germany, Japan, and Israel, the incidence of alcohol-related traffic fatalities in Britain was substantially lower than in France, Australia, Mexico, and the United States.¹⁶

This focus is policy relevant in and of itself, and also because the legislation contains a “sunset clause”. That is, the MUP will expire by April 31, 2024, unless the Scottish Parliament votes for it to continue. Evidence-based evaluations, such as ours, will inform the review to be presented to the legislators.

¹⁵See <<https://www.gov.scot/policies/alcohol-and-drugs/minimum-unit-pricing/>>.

¹⁶Considering all road fatalities (and not just those related to alcohol), Appendix Figure A.1 reveals the rates of accidents per 100,000 population and per 100 million miles travelled in Britain are low relative to other advanced economies.

Three other related interventions are worth keeping in mind, even though they are likely to affect neither the analysis nor the results we find. The first is the already mentioned drink drive limit (DDL) law. On December 5, 2014, Scotland reduced the DDL from 80 to 50 milligrams per 100 millilitres of blood or, equivalently, from 0.08 blood alcohol concentration (BAC) to 0.05 BAC, when expressed in grams of alcohol per deciliter of blood. Since there is overwhelming evidence of no impact of the DDL reform on motor vehicle crashes (Francesconi and James, 2021), we do not expect any interaction between the ineffective DDL change and the more recent MUP reform. In Section 5, nonetheless, we shall directly address this possibility.

The second intervention is the 2008 duty escalator, which increased alcohol duties by 2% above inflation each year. The escalator was repealed in 2014. Also for this policy we expect no impact, not only because it was revoked by the time of actual introduction of the price floor but also because it applied uniformly to all constituent nations of the UK. The last intervention is the 2011 ban on multi-buy promotions of alcohol in retail stores. Nakamura et al. (2014) find no significant effect of the ban on the volume of alcohol purchased, even among large pre-ban purchasers, while Robinson et al. (2014) find a small reduction of 2.6% in off-trade alcohol sales in Scotland. Given these results, the ban might have had an impact on consumption too limited to affect motor vehicle accidents substantially and could have been easily circumvented by stores by lowering the price of one given promotion item (Burton et al., 2017).

3. Conceptual Framework

When considering if it makes sense to set (or increase) a minimum price floor for alcohol, the critical issue is determining whether the decrease in consumer’s surplus due to the binding price floor that is transferred to firms is more than offset by the increase in welfare for those bearing the alcohol-related externalities (Allcott et al., 2019b). The most direct way to make this comparison is to estimate the change in consumer surplus and compare it to the possible decline in harm as measured in monetary units.

As shown by Griffith et al. (2022), this comparison requires three key parameters, which in our case pertain to the subpopulation of drivers involved in fatal accidents. The first is an estimate of the own-price elasticity of products in the set of alcoholic beverages for which the price floor binds; another is the cross-price elasticity of products that are not affected by the MUP reform with respect to a price change of those that are affected; and the last is the share of consumption that drunk drivers get from beverages whose price is below the floor pre-intervention.¹⁷ The problem is that there are no publicly available data to estimate those three objects structurally.

For this reason we implement an alternative approach of estimating the harm from

¹⁷If social welfare includes firms’ profits, one should account the change in total welfare for the loss in profits to firms due to consumers adjusting their demands. This will complicate the computation of the net surplus.

motor vehicle collisions imposed on other people or themselves (externalities and internalities jointly).¹⁸ Specifically, let the externality from alcohol consumption be an increasing function of Q_a , $\mathcal{E}(Q_a)$, where Q_a is the total demand for all externality-generating products in local authority district a . Now, suppose Q depends on the policy environment, \mathcal{P} . In our case, \mathcal{P} is an indicator function that takes value 1 if the MUP reform is binding and 0 otherwise. Besides the immediate change in prices due to the reform, we abstract away from other price adjustments which might have affected some of the products in Q . Given the evidence reported by O’Donnell et al. (2019) and Griffith et al. (2022), this seems plausible.

Considering each function separately, we begin by assuming the externality function to be linear in Q , so that $\mathcal{E}(Q_a) = \delta + \rho Q_a + \nu_a$, where ν is an idiosyncratic shock independent of the externality induced by alcohol consumption in area a .¹⁹ We then assume the demand also depends linearly on \mathcal{P} , i.e., $Q_a(\mathcal{P}) = \theta + \psi \mathcal{P} + \xi_a$, where ξ captures demand shifters orthogonal to the quantity of alcohol consumed in each district. Substituting the latter expression into the first, we obtain a reduced-form version of the externality function given by

$$\mathcal{E}_a = \pi_0 + \pi_1 \mathcal{P} + \eta_a, \tag{1}$$

where $\pi_0 = \delta + \rho\theta$, $\pi_1 = \rho\psi$, and $\eta_a = \rho\xi_a + \nu_a$. The coefficient π_1 is an intention-to-treat (ITT) effect and captures the combination of the direct effect ρ of alcohol consumption on the alcohol externality (road traffic fatalities) with the impact ψ of the MUP policy on alcohol consumption.

According to the estimates found by O’Donnell et al. (2019) and Griffith et al. (2022), we expect ψ to be negative and significant, that is, the introduction of a binding price floor on alcohol reduces alcohol consumption. By assumption, ρ is positive, that is, alcohol consumption generates undesirable externalities. This means the reduced-form effect of the reform on fatalities, π_1 in (1), is negative, i.e., the introduction of the MUP should be followed by a reduction in fatal road accident rates.

A number of factors, however, may lead to a nonnegative π_1 in our application. One is that the existing negative estimates of ψ refer to off-license purchases, which might be driven by the higher affordability of off-license alcohol discussed in Section 2. It is possible that some of the observed alcohol-related road fatalities are driven by alcohol purchased and consumed in pubs and restaurants, for which we do not have an estimate of ψ .²⁰ If

¹⁸Fletcher et al. (2010) use a similar argument to estimate the effect of soft drink taxes on consumption and weight of children and adolescents. Their results suggest that the reduction in soft drink consumption is fully offset by increases in consumption of other high-calorie drinks.

¹⁹For simplicity, we abstract away from possible nonlinearities of \mathcal{E} in the total amount of alcohol consumed. In the empirical analysis, however, we shall indirectly address this possibility through heterogeneity.

²⁰While this is entirely plausible, it is worth stressing that off-trade consumption represents about three-quarters of all alcohol consumption in the UK (Institute of Alcohol Studies, 2019). See also the related discussion in the Introduction.

this were the case, there is no guarantee that π_1 be negative.

Similarly, lower alcohol consumption does not imply that drivers are necessarily more likely to be sober. Post-intervention, drivers (but not the rest of the adult population) may continue to drink as much as they did prior to the reform.²¹ Yet another factor is related to the specific type of externality we focus on. Road fatalities in fact may continue to occur after the enactment of the MUP reform if drivers substitute away from alcoholic beverages to other driving-impairing substances, such as illegal drugs. If such a substitutability were at work as suggested by several empirical studies (e.g., DiNardo and Lemieux, 2001; Crost and Guerrero, 2012; Anderson et al., 2013; Dragone et al., 2019), a reduction in Q would not lead to a reduction in \mathcal{E} , because ρ is close to zero. This implies π_1 would be close to zero as well.²²

Although providing estimates for on-license alcohol consumption or documenting the substitutability between alcohol and illegal drugs is beyond the scope of this paper, the previous observations suggest us to look at dimensions of the externality other than fatal road accident rates. Thus, we will extend our measure of \mathcal{E} to include the likelihood of positive breath testing, and, for completeness, we shall also analyze traffic collisions involving serious and slight injuries. The same observations also suggest us to explore the possibility of heterogeneous responses along various dimensions, such as income.

4. Data and Methods

A. Data Sources

The main data source needed to construct our outcome variables, i.e., our measures of \mathcal{E} , 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, reducing potential issues of differential geographic coverage or measurement error. We use all monthly records from November 2009 to December 2019 on over two million accidents.²³ 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

²¹To bolster this argument, as discussed in Section 2, there is no evidence that the lower drink drive limit introduced in Scotland in 2014 deterred drunk driving (Francesconi and James, 2021).

²²This substitutability is consistent with the estimates found by Griffith et al. (2022), according to which heavier drinkers and lower-income households have larger cross-price elasticities, i.e., they are more willing to switch away from a particular variety if its price increases (even though they may be more likely to switch to another alcohol variety than to not buying alcohol at all). We do not know, however, if this finding persists among drunk drivers.

²³Our sample stops in December 2019 because we want to avoid interactions with the arrival and spread of Covid-19. Extending this analysis to cover the period affected by the pandemic is left for future research.

severity, and this in turn is distinguished into fatal, serious, and slight.²⁴ Our focus is on fatal collisions.

For non-fatal accidents, the RAD data contain information on alcohol involvement. This is collected from surviving drivers who are breath tested at the roadside. The level of alcohol in the breath is not given, but we know whether the test is 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 is killed or injured and at least one of the drivers involved either fails the roadside breath test by registering above the existing legal DDL or refuses to give a breath test specimen when requested by the police, except when incapable of doing so for medical reasons.²⁵

The cross-sectional unit of observation is the local authority district. In Britain there are 376 local authorities in total, 345 in England and Wales and 31 in Scotland. Our key outcomes are the fatal and drunk driving accident rates defined as the number of fatal and drunk driving collisions in a given local authority and a given month per 100,000 vehicles registered in the same local authority.²⁶

Figure 2 shows the deseasonalized monthly fatal and drunk driving accident rates over the sample period by country (i.e., Scotland versus England and Wales) averaged over all local councils. Appendix Figure A.2 displays the deseasonalized rates across all types of accidents as well as those with serious and slight injuries. We stress two points. First, fatal accident rates in Scotland are similar, in level and gradient, to the rates observed in the rest of Britain.²⁷ Drunk driving rates instead are lower for Scotland, where they also display a more pronounced declining trend throughout the sample period, even before the MUP enactment. This trend may partly reflect the procyclical nature of motor vehicle accidents (e.g., Ruhm, 2015), a feature that might have been slightly stronger in Scotland

²⁴Fatal 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. One could take this type of injuries as good proxies of hospitalizations and long-term health problems (e.g., Burton et al., 2017). 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>.

²⁵As mentioned in the previous section, the legal DDL remained the same in England and Wales throughout the sample period at 0.08 BAC. Scotland instead reduced the threshold in December 2014 from 0.08 to 0.05 BAC. From that date onwards, the Scottish data account for the stricter limit. Notice also that, if we exclude drivers who refuse to take the test from the definition of drunk driving, all our main results do not change.

²⁶The information on the time series of the number of vehicles by local council comes from the DfT. As discussed in Section 5, we perform a few tests using other definitions of rates based on different populations at risk, such as the entire population and road availability.

²⁷This is not case for slight injury collisions, which make up the bulk of road accidents and thus affect the patterns for the aggregate (all) accident rates. In that case, Scotland experiences lower crash rates than England and Wales, but all nations face a comparable declining trend over the sample period.

than in the rest of Britain. In the analysis below, we address this issue by allowing for nonlinear trends in drink drive collisions. We also perform several checks, from testing for parallel pre-reform trends to including Scotland fixed effects (see the next section). Second, the MUP intervention in May 2018 does not seem to be followed by a noticeable slowdown in Scotland in either fatal or drunk driving collision rates. This last observation suggests that the reform might have had no impact. The raw (un-deseasonalized) rates display the same qualitative patterns (see Appendix Figure A.3).

As suggested in the literature reviewed in the Introduction, road traffic collisions are likely to be correlated with a variety of factors other than alcohol prices, 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 minimum temperatures in degree Celsius, recorded in each UK climate region (obtained from the Meteorological Office);²⁸ (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);²⁹ (c) socioeconomic status of the population in each district, 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 local-council average total number of licensed alcohol premises (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 two statistical methods used in the analysis (see below). Confirming the evidence shown in Figure 2, the average monthly fatal accident rates are similar in local districts between nations (0.485 in Scotland versus 0.450 in England and Wales per 100,000 registered vehicles), while the drunk driving rates are higher in England and Wales (0.996 against 0.741 per 100,000 vehicles). The differences in mean outcomes are essentially eliminated if we consider the subsamples used with the synthetic control design, reported in the last two columns of Table 1. Significant differences between Scotland and their English and Welsh counterparts emerge in a few explanatory variables. Scottish local councils, on average, have a lower population density and a greater fraction of residents with no educational

²⁸We 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, which proxies the variation in light conditions induced by differences in sunrise and sunset times as in Bünnings and Schiele (2021) and Smith (2016). Since all the results are identical to those presented below, we use a more parsimonious specification.

²⁹Density 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.

qualifications, working fewer hours, in bad health, and claiming unemployment benefits. Again, most of such differences are considerably reduced when we use the subsamples selected by the synthetic control approach.

B. Empirical Specifications

We take a modified version of (1) to the data. In particular, we allow the demand function Q to account explicitly for the differential impact of the MUP reform enactment across areas and over time, so that $Q_{at} = \psi_0 + \psi_1 \text{post}_t + \psi_2 S_a + \psi_3 (\text{post}_t \times S_a) + \mathbf{X}'_{at} \boldsymbol{\gamma} + \zeta_a + v_{at}$, where post_t is a dummy variable equal to one if the MUP reform is in place, and 0 otherwise; S_a is an indicator variable that take value 1 if an observation refers to a Scottish district, and 0 otherwise; \mathbf{X}_{at} is a vector of control variables described in the previous section; and ζ_a denotes district fixed effects. Substituting this expression into the externality function $\mathcal{E}(Q)$, we arrive at a familiar difference-in-difference (DD) specification, which compares the treated local councils in Scotland to the control districts in England and Wales before and after the introduction of the Scottish price floor policy

$$y_{at} = \phi_0 + \phi_1 \text{post}_t + \phi_2 S_a + \beta (\text{post}_t \times S_a) + \mathbf{X}'_{at} \boldsymbol{\lambda} + \varphi_a + u_{at}, \quad (2)$$

where y is one of our measures of \mathcal{E} (e.g., fatal crash and drink drive accident rates), φ_a is local authority fixed effects, and $\beta (= \rho\psi_3)$ is the ITT effect of interest. As part of the controls in \mathbf{X} , we allow for a highly flexible time component with group-specific trends and differential seasonality effects.³⁰ We also estimate an event-study variant of (2), which allows us to check for common time trends.

Statistical inference from this DD approach relies on asymptotic approximations associated with the assumption that the number of local authorities grows large (Wooldridge, 2006). Scotland comprises 31 different districts, which may have been exposed to different treatment intensities depending on the unobserved levels of pre-intervention alcohol consumption and drunk driving propensities. This is why, in the baseline analysis, we estimate (2) using the full set of local councils at the national level.

However, we shall also account for the fact that the treatment occurs in one country (Scotland) across all its districts at the same time. In one exercise, we re-estimate (2) using only two large areas, one as treated (Scotland) and the other as control (England and Wales). In another exercise, we implement a variant of Fisher's permutation or randomization test to address this inference problem with greater sophistication (Fisher, 1935). In particular, we consider the whole of Scotland as one single treated area and pretend that another 31 random English and Welsh councils are treated rather than the 31 districts in Scotland. With this new control group, we estimate equation (2). We then randomly select another set of 31 English and Welsh districts and repeat the estimation

³⁰We generally fit linear trends. However, because of the nonlinear trends in drunk driving collisions shown in Figure 2, we will use a quadratic specification for that type of accidents.

again. This process is iterated 1,000 times, each time using a different random set of 31 districts. This produces a distribution of “permutation” effects, yielding p -values for the hypothesis that the treatment effect of the Scottish MUP is different from zero. We summarize the permutation effects distribution focusing on its 5th and 95th percentiles. The resulting hypothesis tests are based on much more conservative confidence intervals than those produced using standard clustering alternatives.³¹ With 1,000 permutation estimates, achieving 10% significance from a two-tailed test requires that Scotland be ranked 50th from the top or bottom of the permutation distribution, while 5% significance requires Scotland be ranked 25th at the top or the bottom.

An alternative method for causal inference with aggregate data is synthetic control analysis, which is a generalization of the DD framework (Abadie et al., 2010; Abadie, 2021).³² Unlike the DD model, this approach uses only a subset of units (local authorities) for controls and selects control districts that exhibit the same pre-treatment dynamics in y and \mathbf{X} as the average Scottish districts.³³ If there is a concern that the whole of England and Wales is not the right control group, then this model addresses that issue.

Following Abadie et al. (2010), we use an inferential technique based on several placebo exercises. Appropriate inference can be established by performing a falsification test based on the distribution of the (placebo) effects estimated for the 345 local authority districts in the donor pool, i.e., the set of potential comparisons or the collection of untreated local councils not affected by the intervention. The null hypothesis that the effect of the Scottish MUP reform is equal to zero is rejected if the effect estimated for Scotland as a whole is abnormal relative to the distribution of placebo estimates. If instead the distribution of placebo effects yields effects that are similar to those found for synthetic Scotland, then it is likely that the alcohol price floor did not have any impact. We therefore replicate the synthetic control estimates for all possible sets of local councils in the control group, pretending that each placebo district experienced the treatment in May 2018. Clearly, it is possible that some of the placebo effects are implausibly large if councils are not well matched in the pre-intervention period. To minimize this potential problem, in our estimation we restrict the comparison set of local councils to only those

³¹See also Barrios et al. (2012) and Ferman and Pinto (2019a). In additional sensitivity analysis, we also replicated the procedure used by Buchmueller et al. (2011) and Cunningham and Shah (2018), whereby we estimated (2) an additional 345 times replacing Scotland with an indicator for one of the 345 districts in England and Wales. We then compared the Scotland estimate to the alternative permutation estimates obtained, treating the 345 permutation estimates as the sampling distribution for β . Since the results from this exercise are qualitatively similar to those described in the text, they are not reported for the sake of space concerns. Notice also that rather than “permutation”, others use the term “placebo” (e.g., Buchmueller et al., 2011; Cunningham and Shah, 2018). We use “permutation” simply to avoid confusion with the term “placebo” which is used when we deal with the synthetic control approach.

³²Relative to the regression-based DD counterfactuals, this approach presents a few advantages, such as no extrapolation outside the support of the data, transparency of the fit and of the counterfactual, and sparsity which plays an important role for the interpretation of the estimated counterfactual (Abadie, 2021). See also Firpo and Possebom (2018).

³³In the estimation, we account for all the collision predictors in \mathbf{X} described in Section 3 and listed in Table 1 as well as the pre-intervention values of the outcome variables.

that match extremely well and remove all the comparisons with a pre-treatment mean squared prediction error (MSPE) that is more than two times the corresponding MSPE found with the synthetic control.³⁴

Although the synthetic control method 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, it 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 (Abadie et al., 2010). Other estimators, 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. (2021) suggest to optimize the strength of the two estimators and combine matching and synthetic control (MASC) procedures through model averaging. Furthermore, Abadie and L’Hour (2021) propose an alternative bias-corrected synthetic control method which introduces a penalization parameter that aims at reducing interpolation bias. It prioritizes the inclusion of units in the synthetic control close to the treated unit along the lines of the matching variables. In the analysis below, we will present the evidence obtained from these two more recent approaches.

5. Results

A. Baseline Estimates

Difference-in-Difference Estimates — We begin with the results from an event-study variant of (2). Specifically, we estimate

$$y_{at} = \sum_{m \in \mathcal{M}} \alpha_m + \sum_{m \in \mathcal{M}} \beta_m S_{at} + \mathbf{X}'_{at} \boldsymbol{\lambda} + \varphi_a + u_{at}, \quad (3)$$

where m indexes year-months, \mathcal{M} is the set of months in the sample (except one to be used as base month; this is given by April 2018, the month before the enactment of the new policy), and the rest of the notation is, for simplicity, kept identical to the one used in (2). We restrict the sample to 20 months before and 20 months after the reform.

Figure 3 displays the β_m estimates from equation (3) and the 95% confidence interval around them in the months before and after the introduction of the price floor for road traffic fatalities and drink drive collisions. Prior to the enactment of the policy, 18 of the

³⁴As emphasized by Abadie et al. (2010), this is a conservative cutoff that discards districts with extreme values of pre-intervention MSPE for which the synthetic control method would be ill-advised. An additional issue is the monthly frequency of the data, which could expose the analysis to idiosyncratic shocks making the comparisons problematic. To smooth out the potential influence of the shocks, therefore, we repeated the whole analysis after aggregating the monthly data up into quarters or using three-month moving averages. In both cases, we found the same results as those shown in the next section. These alternative estimates, therefore, are not reported but could be obtained upon request.

20 β_m coefficients for both types of accident rates are not statistically different from zero, providing broad support for parallel pre-trends in collision rates. Once the policy came into force, we find no change in collision rates in Scotland relative to the rest of Britain, suggesting no impact of the reform. This result is particularly striking for drunk driving accidents, since this type of collisions displays a declining trend in Scotland, both before and after the MUP reform (see Figure 2). The same evidence emerges also for serious- and slight-injury accidents.³⁵

Table 2 reports the DD estimates of the effect of the price floor obtained using equation (2). The table presents the results from four specifications. Column (a) shows the results from a basic specification that includes group-specific month dummies (to account for seasonality) and group-specific monthly trends, but sets $\lambda = \varphi_a = 0$, while in column (b) we add both local authority district fixed effects and the full set of controls. From both specifications, we find that the price floor led to an *increase* in fatal road crashes of at least 5% (or a rise from 4.9 to 5.1 road fatalities per 1 million registered vehicles) and to a decline in drunk driving accidents of up to 5% (or, equivalently, a reduction from 7.4 to 7 drunk driving collisions per 1 million vehicles). Although both impacts could be seen as quantitatively important and the increase in road deaths goes against the theoretical considerations laid out in the previous section, they are both statistically insignificant. In what follows, we devote special attention to the inference associated with such estimates.

Since the tests for pre-trends may be underpowered against meaningful violations of parallel trends, potentially leading to undercoverage of conventional confidence intervals, we follow Kahn-Lang and Lang’s (2020) suggestion regarding how the parallel trends assumption might be tested, especially when the levels of the dependent variable vary. They suggest including covariate-specific trends to account for changes in the relationship between covariates and the dependent variable over time. This involves including interaction terms between covariates and linear time trends. Column (c) of Table 2 shows the results from a specification in which we include linear time trends separately interacted with four control variables (i.e., population density, proportion of residents aged 16 or more with no educational qualification, Job Seekers’ Allowance rate, and number of alcohol licensed premises). The estimates confirm the null results found with the two previous specifications, with the standard errors barely changing.³⁶

Using the same three specifications in columns (a)–(c), we perform the Fisher’s randomization inference analysis mentioned in the previous section. In each panel of Table 2, below the DD results, we present the estimates of the 5th and 95th percentile confi-

³⁵The reduction in slight-injury accidents, which is detected for some of the months following the reform in Appendix Figure A.4, is an interesting finding that could deserve further attention. However, it is never confirmed by the estimates found with the other methods (see below). Moreover, since the focus of this paper is on major externalities, its exploration is left for future research.

³⁶We have estimated additional models with covariate-specific trends for one variable at a time, and one in which we include trend interactions for all the covariates listed in Table 1. Every model delivers estimates that are consistent with those shown in column (c) and are therefore not reported. These results can be obtained upon request.

dence intervals from the permutation effects distribution as well as the p -values from a two-tailed test based on the same distribution. Irrespective of the specification and of the type of accident, the 5th and 95th percentiles of the distribution fall around zero almost symmetrically. From the p -value results, it is clear we cannot reject the null that the alcohol price floor had an effect neither on fatal collisions nor on drunk driving accidents.

Figure 4 provides a graphical illustration of the permutation-based inference results showing Scotland’s position in the full distribution of permutation effects for the two outcomes of interest obtained from specification (b). Similar results emerge from the other specifications, which we do not report for convenience. The vertical dashed bars show the 5th and 95th percentile critical values (excluding Scotland) and the solid vertical line represents the corresponding DD estimates for Scotland. In the graphs, estimates that achieve 5% significance are identified by their position outside the span of the permutation histogram. The figure illustrates clearly that the estimates for Scotland are close to zero, similar to the average English/Welsh control district and well within the span of the confidence intervals. In fact, Scotland ranks 269th from the top and 162nd from the bottom for fatal and drunk driving collisions, respectively, far from 25th position required to achieve 5% statistical significance. Put differently, there is nothing exceptional about road accident outcomes in Scotland, confirming that the MUP reform had no effect on road vehicle crashes.

One can take an even more extreme view on treated and control clusters and consider all 31 local authorities in Scotland as one single unit, with all the districts in England and Wales forming another single untreated cluster. Column (d) reports the results when we have just two clusters, one for Scotland and another for the rest of Britain, for which we take the country-specific means of outcomes and controls. For both outcomes, the point estimates are essentially identical to those shown in column (a), while the standard errors are 14% smaller in the case of fatal accidents and 14% greater in the case of drunk driving accidents. In both cases, nonetheless, we reach the same conclusion that the introduction of the price floor did not lead to any sizeable change in accident rates in Scotland.

In spite of all the inference exercises we performed, it is still possible that we over-reject the hypothesis that the Scottish MUP had an impact, because of inappropriate clustering when there is only one treated area (i.e., the whole of Scotland). An additional way to assess inference in this case is the procedure suggested by Ferman (2021).³⁷ The results from this rejection probability test are displayed in the bottom panel of Table 2. They show that the assessment for a 5%-level test is less than 6% for each of the specifications (a)–(d) and both outcomes. This unambiguously indicates that the inference based on

³⁷This requires us to redefine a new dependent variable for which the assumptions for the inference method being assessed are satisfied, i.e., clustering at the local authority level as we do in specifications (a)–(c) or using heteroskedasticity-robust standard errors as in specification (d). After regressing this new dependent variable on the treatment and control variables (i.e., $\text{post}_t \times S_a$, post_t , S_a , and \mathbf{X} in (2)), we can test the null that the treatment variable is significant at a given significance level, say 5%. Repeating this process 1,000 times allows us to calculate how often the null is rejected. If the rejection rate is close to the pre-specified significance level (i.e., 5%), then this implies that the test is asymptotically valid.

clustered-robust standard errors at the local council level as used in columns (a)–(c) is reliable. Overall, therefore, the results in Table 2 document that the price floor introduced in 2018 in Scotland prevents neither traffic crash deaths nor road accidents induced by drunk driving.

We draw an analogous conclusion from the estimates on the other types of motor vehicle collisions, for which alcohol involvement is unknown. The results from the same previous specifications are in Table 3. Against theory, serious-injury crashes increase by nearly 10% (with effect estimates edging towards conventional levels of statistical significance), while slight-injury accidents go down by 4% but statistically insignificantly. The positive effect on the former type of accidents, which could have a direct link to hospitalizations, suggests that the short-term response to the reform may have unintended externalities on public health resources.

DD Results from Precise Information on Alcohol Involvement — In Section 3, we mentioned that the information on alcohol involvement in the RAD records is incomplete. This is because police officers do not breathalyze those who die at the roadside and cannot check drivers who leave the accident scene before the arrival of the police in hit-and-run cases. If the measurement error induced by this incompleteness is random and independent of the MUP implementation, then this does not bias our estimated baseline coefficients, but it may yield larger standard errors. This is actually not reflected in Table 2, where the standard errors for drunk driving accidents generally have the same size of the standard errors found for fatal crashes.

These results notwithstanding, we perform further analysis to address this issue using new data compiled by the DfT. Such data, like the RAD records, are derived from STATS19 forms but are supplemented with detailed information on hit-and-run accidents and toxicology data on fatalities from coroners in England and Wales and public prosecutors (or procurators fiscal) in Scotland.³⁸ These data, however, are coarser than those used earlier in two important dimensions: they come at an annual frequency from 2009 to 2019 (not permitting us to define accident rates at the monthly level) and at a less granular geographic detail (i.e., instead of 376 local councils, we only have nine English regions, the whole of Wales and the whole of Scotland). This means we cannot perform the analysis on such data using the synthetic control design.

We then estimate DD models that follow the baseline specification as closely as possible, accounting for differences in annual regional average temperature range, population density, road length, proportion of residents with no qualification, proportion of individuals with bad or very bad general health, median hours worked per week, Job Seeker’s Allowance rate, and the number of licensed alcohol premises. The estimates for fatal

³⁸In this data set, the specific definition of a drink drive accident not only is based, as before, on reporting a positive roadside breath test or refusing to give a breath test when requested by the police, but also includes cases of individuals who die at the roadside and, within 12 hours of the accident, are found to be above the legal limits.

crashes and drink drive collisions obtained from the same four specifications described above for Table 2 and the same inference exercises, which might be more pertinent here because of the greater geographic aggregation, are presented in Table 4. These estimates confirm the null result found in the baseline analysis, corroborating the conclusion that the Scottish price floor does not moderate the negative externalities attributable to either road fatalities or drunk driving.³⁹

Synthetic Control Estimates — We begin with Figure 5, which displays maps of Britain where the light shaded areas represent English/Welsh local authority districts in synthetic Scotland along with their weights, for fatal and drink drive crashes. All unshaded districts in the potential control group are assigned zero weights.⁴⁰ Figure 6 shows the district-specific rates for Scotland and synthetic Scotland over time. They suggest the price floor policy has no effect on both types of collisions. 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 MUP reform in May 2018, the two lines continue to overlap substantially regardless of the type of collision.

The inference results for the synthetic control model are reported in Figure 7, which shows the distribution of estimates for the placebo and treated local councils. In every panel, the black line is our treatment effect, i.e., the gap in accident rates between Scotland and synthetic Scotland. The gray lines instead represent the gaps associated with each of the runs of the placebo test. 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 MUP policy. Evaluated against the distribution of the gaps for the placebo districts, therefore, the gap for Scotland does not appear to be unusual.

Averaging over the 20 months after the introduction of the policy leads to a point estimate of 0.039 for fatal crashes and -0.017 for drunk driving accidents, which imply an 8% rise and a 2% decline in the two types of collisions, respectively. The former estimate is slightly larger than the corresponding DD estimates and continues to go against theory

³⁹Another inference issue of these DD estimates is the possibility that the error term u_{at} in (2) be serially correlated. This is more likely to arise when we have fewer geographic units of observation than time periods as it is the case in the models presented in Table 4. Re-estimating them using a Prais-Winsten local authority specific AR(1) error structure confirms the null result of the baseline estimates. We find the same results for the models reported in Tables 2 and 3.

⁴⁰Two observations are in order. First, we end up with 6–7 councils in the actual donor pool out of 345 units, many fewer than the 102 pre-treatment periods, in line with Abadie’s (2021) recommendations. Second, two local councils, Wrexham and Gwynedd (both in Wales), stand out with large weights in all comparisons. Both districts are very similar to the Scotland average along many dimensions. For example, their temperature range is just within 0.3% of Scotland’s range. Both councils are also within 0.5% of the Scottish mean for the education control as well as for all types of accident rates. We should stress that we cannot find any evidence that those local authorities, including Wrexham and Gwynedd, introduced specific changes to their transport policies after 2018 which other councils did not adopt. Furthermore, to check the sensitivity of our main estimates to the presence of these Welsh districts, we also estimated models in which the whole of Wales is dropped from the analysis. The results from this estimation support the baseline estimates presented below.

predictions, whereas the latter estimate is substantially smaller than its corresponding DD counterparts. Both estimates are well within the inferentially conservative interval determined by the two-times-MSPE cutoff. We take this evidence, therefore, as indicating that the price-based alcohol control policy does not lead to a quantitatively and statistically significant change in road traffic collisions.

Appendix Figures A.6, A.7, and A.8 show the full set of results for serious- and slight-injury accidents. They reveal the same patterns as fatal and drink drive crashes, confirming that the 2018 alcohol price floor has no sizeable effect on accident rates. It is worth noting that, in contrast with the DD results in Table 3, the synthetic control estimates for serious-injury accidents do not detect any impact of the reform.

To deal with the potential interpolation bias induced by the classic synthetic control method mentioned in Section 4, we estimated matching and synthetic control (MASC) models as formulated by Kellogg et al. (2021) and the penalized synthetic control approach of Abadie and L’Hour (2021). Figure 8 shows Scotland and MASC Scotland, as well as the treatment effects by accident type. The figure reveals that the collision rate estimates for MASC Scotland replicate those in Scotland extremely well in the pre-reform period, suggesting high statistical reliability of this approach. After the implementation of the alcohol floor reform, as with the classic synthetic control approach, the two lines overlap and the gaps fluctuate very closely to zero, indicating no impact of the MUP policy on road collisions. A comparison of the treatment effects of the classic synthetic control and the bias-corrected version of Abadie and L’Hour (2021) is shown in Figure 9. There is very little difference in the estimated treatment effects between the two approaches, confirming the finding that the alcohol floor does not have any impact on fatal or drink drive crashes.⁴¹

Statistical Power — Before moving on to the robustness and heterogeneity analyses, we briefly return to the inferential issues linked to the DD evidence illustrated above from a different perspective. Specifically, we ask whether the decline in alcohol consumption is too small for us to detect a significant reduction in accident rates. This is important because it allows us to put our null result into perspective, depending on whether it is driven by an ineffective price floor or precisely estimated zero effects.

To assess this statistical power issue, we observe that the MUP decreased alcohol purchases for at-home consumption by 7.6 to 11% (O’Donnell et al. [2019] and Griffith et al. [2022], respectively).⁴² Let us begin with the more conservative figure of 0.076

⁴¹Repeating both the MASC analysis (Appendix Figure A.9) and the estimation based (Appendix Figure A.10) on the bias-corrected synthetic control approach for serious accidents leads to the same conclusion. A small gap appears to open for slight accidents in the MASC analysis, however, this is not the primary focus of the paper, and it does not emerge in either the classic or bias-corrected synthetic control, so the body of evidence is not in favor of a claim that there was a reduction in slight accidents.

⁴²The 11% reduction found by Griffith et al. (2022) may seem large. A closer look, however, reveals that this corresponds to an average 0.6 unit reduction per adult per week. Since a pint of lager (5.2% ABV) contains three units of alcohol, the 11% estimate implies that the average consumer reduced alcohol

and suppose drunk driving accidents are linear in alcohol consumption.⁴³ We would then expect a 7.6% reduction in drunk driving collisions. In the data shown in Table 1, the average monthly drunk driving accident rate is 0.741 per 100,000 registered vehicles in Scotland. A 7.6% reduction would imply a point estimate of -0.056 , which could be taken as the expected benchmark effect. The smallest (in absolute value) DD point estimate is in column (d) of Table 2 and is equal to -0.025 , which is less than half of the expected benchmark but its corresponding 95% CI $[-0.176; 0.126]$ includes the benchmark. This CI implies that, although the MUP could have led to a reduction in drunk driving accidents of nearly 24% (three times the benchmark), it might equally be responsible for a rise of 17%, twice the benchmark in absolute value.

If instead we use the 11% figure for the MUP effect on consumption, the expected benchmark effect on drunk driving becomes -0.082 , which in absolute value is considerably greater than all the point estimates reported in Table 2 for drunk driving, just within their 95% CIs. This exercise — especially the latter based on the 11% impact on consumption — indicates that the size of standard errors relative to the expected benchmark effect is appropriate for us to detect an effect. The fact that we do not find a significant impact leads us to reiterate that the null effect of the price floor on road accidents is on the mark.

Moving to fatal car crashes, with a 7.6% reduction in alcohol consumption, the expected benchmark linear effect would be -0.037 . As before, also this impact falls within the 95% CI of all the estimates in panel A of Table 2. But here the problem is that such estimates identify an *increase*, rather than a decline, in fatal accident rates. As before, therefore, the size of the standard errors relative to the benchmark effect is not too large and the response to the alcohol floor in terms of fatal collisions is not necessarily too small, rather wrong-signed.

In sum, the results from this exercise confirm that we cannot reject the null hypothesis that $\beta = 0$, not because the MUP intervention is inherently ineffective, rather because we find precisely estimated zero effects. Even though the alcohol price floor had an impact on ethanol consumption, its effect on road traffic accidents is statistically indistinguishable from zero.

purchases by one-fifth of a pint of lager per week. This is arguably a small impact, which could explain our null results. On the other hand, heavier drinkers, such as those in the top 5% of the long-run drinking distribution, are found to have reduced their purchases by 6 units (or two pints of beer) per weeks. This impact implies a more sizable first stage, which may plausibly lead to a negative externality on road accidents.

⁴³Assuming linearity may be strong. Levitt and Porter (2001) discuss evidence according to which the relative likelihood of causing a fatal crash is about two times higher for drivers with BACs between 0.05 and 0.099, 10 times higher for BACs between 0.10 and 0.149, and 30–40 times greater for BACs over 0.15. Francesconi and James (2019 and 2021) find estimates consistent with that evidence. Despite such results, however, when considering the changes implied by the 7.6 or 11% reductions in alcohol purchases that correspond to an average marginal decrease of only 0.5–0.6 units of alcohol, our linearity assumption is sensible. Naturally, we do not account for on-trade consumption, which may affect the first-stage estimates and, ultimately, the plausibility of our assumption.

B. Sensitivity Checks

The baseline analysis establishes that the 2018 enactment of the minimum unit pricing of alcohol does not contribute to the reduction in motor vehicle collisions in Scotland. Below we present a number of exercises to check whether this result is an artifact of either our definition of the dependent variable, functional forms used in estimation, estimation approaches, institutional changes other than the MUP reform, and the geographic distance from the border. Although all the estimates discussed here and in the next subsection refer to fatal and drunk driving accidents, the same results emerge also for serious- and slight-injury collisions.⁴⁴

Alternative Definitions of the Dependent Variable — In the benchmark analysis, our outcomes are based on the number of monthly accidents per 100,000 registered vehicles in each local authority district. We redefine them using four variants, that is, the number of monthly accidents per 100,000 of the population, the number of monthly accidents per 100,000 of the adult population, the number of monthly accidents per kilometer of road in the local council, and the number of monthly accidents per vehicle miles travelled. Each of these alternative definitions aims to identify possibly different dimensions of the ‘populations’ at risk of experiencing a car crash. All four measures produce results similar to the baseline estimates shown above.

Alternative Specifications of the Synthetic Control Design — In the benchmark synthetic control analysis, we use covariates as well as the outcome variable to create our synthetic controls. We perform two checks of this approach. First, we create synthetic control groups that only match on outcomes as proposed by Botosaru and Ferman (2019). Second, we use demeaned data that can help improve the quality of the match (Ferman and Pinto, 2019b). Both checks emphatically uphold the benchmark estimates.

Count Data Models — An additional strategy is to estimate the impact of the reform using count data models, in which our new dependent variable is the number of collisions in a given month. When estimating such models, we control for either registered vehicles (or population) by local authority district to account for variation in size across local councils. The estimates found with this alternative approach reveal the same null results found in the baseline analysis.

Accounting for the DDL Reform — In earlier sections we have already mentioned the 2014 drink drive limit (DDL) law, which reduced the maximum amount of alcohol an individual can have to be allowed to drive from 0.08 to 0.05 BAC. Since the DDL reform has not impact on road accident rates (Francesconi and James, 2021), our baseline analysis

⁴⁴For the sake of space concerns, most of the following results are not presented but are available upon request. For all the classic synthetic control models, here and in the next subsection, we remove the placebo local authority districts whose pre-treatment MSPE is more than two times the corresponding MSPE found with the synthetic control.

leverages the longest possible time horizon, including pre-DDL time periods, in order to achieve the best statistical fit of the data. But to limit the possibility of an interaction between the 2014 DDL law and the 2018 MUP policy, we restrict the analysis only to the post-DDL reform period. Focusing the analysis to this shorter time period does not alter the results. Notice that this selection allows us also to address the other two policy interventions mentioned in Section 2, namely the repeal of the alcohol duty escalator in 2014 and the 2011 ban on multi-buy promotions of alcohol in retail stores.

Anticipation Effects — Another possible concern is that the six year delay between the passage of the legislation through the 2012 Alcohol Act and the actual implementation of the MUP reform in May 2018 could lead to biased estimates. The reason is that firms may have started adjusting their prices upwards in anticipation of the policy. If this is true, the price change around May 2018 would likely be biased downwards, which may bias our estimates downwards too. As mentioned in Section 2, the existing evidence on alcohol prices suggests that prices did not go up after the 2012 Alcohol Act, while alcohol affordability continued to increase. We nonetheless assess this possibility assuming that the effective date of the MUP introduction was May 2012, and repeat the estimation over the 2009–2015 period only. All the baseline results are confirmed.

Substitution Effects — A potential issue, especially with the difference-in-differences approach, is that cross-border shopping could contaminate our estimates. That is, Scottish consumers who live close enough to the border may drive into England and buy cheaper alcohol. Griffith et al. (2022) do not find evidence of this contamination, while Beatty et al. (2009) identify results that are consistent with (higher tax) avoidance behavior among household who live near the border between Norway and Sweden. Roberto et al. (2019) also detect substitution to purchases outside of Philadelphia in response to a tax on sugar-sweetened beverages in Philadelphia. The presence of this leakage could reduce the effectiveness of the price floor in Scottish districts close to the border. Its existence will then mask the benefits in districts far away from the border, if we rely on the aggregate analysis we have performed so far.

To contain this possible contamination bias, therefore, we repeat our estimation after excluding the local authorities within 50km or 100km of the border between Scotland and England. Consumers in local districts located further away from the border would find it harder to engage in cross-border shopping. In both cases, we continue to find no evidence of an impact of the price floor on fatal car crashes and drunk driving (see the estimates in Appendix Table A.1 and Figures A.11 and A.12).

C. Heterogeneity

The null results in the benchmark analysis and across all the robustness checks may mask considerable effect heterogeneity. Because the MUP reform raises the prices of

cheap products favored by heavy drinkers, we may expect the largest demand reductions among this group of consumers (O’Donnell et al., 2019; Griffith et al., 2022). If this is the case, the same group may experience a reduction in fatal accident or drunk driving rates. Our fatal accident data, however, do not have alcohol involvement information. We thus use proxies of heavy drinking, such as driver’s gender and age as well as the timing of the accident. The existing evidence on the effect of the Scottish price floor on alcohol consumption also documents that the reform affects low income households more than other households (Griffith et al., 2022). We then look for differential impacts by income at the local authority level. All the results from these exercises are reported in the Online Appendix.

Driver’s Gender and Age — We first distinguish fatal accidents and drink driving crashes by the gender of the driver. We find no impact for either sex (shown in Appendix Figures A.14 and A.15). We then separate out collisions (either fatal or due to drunk drinking) that involve at least one driver aged 18–30 years, from those in which the driver is 31–49 or 50+ years. We cannot detect any impact of the MUP policy on either road traffic deaths or drunk driving across the three age groups (see Appendix Figures A.16, A.17, and A.18).

Timing of Accidents — Nights and weekends tend to be characterized by greater alcohol consumption (Francesconi and James, 2019). We thus divide up fatal and drink drive accidents into those during the day, 8:00 a.m. till 8:00 p.m., and those in the night, 8:00 p.m. till 8:00 a.m. in the following morning (as shown in Appendix Figures A.19–A.22). There is no evidence of a difference in the impact of the price floor by time of the day. The same emerges if we use different definitions of day and night hours. Neither does there appear to be an impact of the MUP reform on road traffic collisions that happen at weekends (as well as weekend nights) whether we define them as Saturdays and Sundays only or we include Fridays as well (presented in Appendix Figures A.23–A.25).

Income — Because of the income gradient of the MUP reform on ethanol intake, we examine whether this carries over onto motor vehicle accidents. Specifically, we examine if there is heterogeneity of the impact of the intervention by local authority income level. We perform two exercises. In the first, we split the sample into local authorities with average income above or below the median national income (presented in Appendix Figures A.26 and A.26). In the second exercise, we repeat the exercise but distinguish local authority districts into income quartiles (shown in Appendix Figures A.28–A.31).⁴⁵ In both exercises across all income groups, we do not detect an effect of the price floor policy on traffic fatalities or drunk driving accidents. This result casts doubt on one of the channels mentioned in Section 4, according to which road collisions are caused primarily by drivers who drink more expensive alcohol whose price is not affected by the reform.

⁴⁵In the case of the estimates based on the synthetic control method, we re-estimate the control group every time for each outcome and each of the income categories.

6. Conclusion

This paper presents the first evidence of whether the alcohol price floor introduced in Scotland in May 2018 affects fatal road crashes and drunk driving accidents. We use administrative data on the universe of motor vehicle accidents recorded in the UK between November 2009 and December 2019 and apply a quasi-experimental design based on the fact that the price floor was implemented in Scotland but not in other parts of the UK to identify the impact. We find no evidence that the minimum unit pricing on alcohol affects traffic fatalities or drunk driving collisions. These results are robust to several assessments of the reliability of inferential methods, checks on model specifications and definitions of the outcome variables. There is also no evidence of cross-border effects or heterogeneity by income and other predictors of heavy drinking, such as age, gender, and weekends. Finally, in spite of potential statistical power issues, we show that the reduction in alcohol consumption as a result of the price floor is likely to be large enough for us to detect a significant impact on motor vehicle accidents.

Despite these null results, we do not interpret them as providing clear-cut evidence to advocate a reversal of the reform. We emphasize three observations on this point. First, road traffic collisions are an important short-run externality of alcohol misuse. But there might be other short-run alcohol-related harm, such as crime, which needs further inquiry. There may also be longer term externalities on health, e.g., hospitalizations, liver cirrhosis and cancer, and on work absenteeism, which could be effectively tackled by the MUP policy. Future research to test these links will be necessary.

Second, the evidence that the Scottish alcohol floor has an effect on alcohol consumption comes from studies that examine off-license purchases. Although three-quarters of purchases are currently off-trade, it is possible that the most serious road accidents are caused by on-trade consumption, which is essentially unaffected by the current MUP reform. New research therefore will have to establish whether the intervention impacts on-trade intake and, through this channel, traffic fatalities.

Third, given the multiple life domains associated with alcohol-related road accidents, policy coordination is likely to play a central role. As suggested by Allcott et al. (2019a), optimal alcohol control policies should be targeted to reduce consumption more among those individuals who are likely to generate the largest externalities and internalities. If the incidence of road accidents is largest among young people — perhaps through limited self-control or habit persistence — then providing information and education targeted to students in secondary school and university may help to raise awareness about alcohol-induced driving impairment. Alcohol-induced traffic collisions may be also highly related to law enforcement. A significant effect of the MUP on traffic crash deaths and drunk driving may be seen in conjunction with an efficient deployment of police resources, such as “hot-spot” policing (Banarjee et al., 2019). This point may be viewed in conjunction with the existing evidence on the lack of an impact of the 2014 Scottish drink drive

limit reduction, which in part seems to be due to weak enforcement (Francesconi and James, 2021). In addition to new research, these observations call for improvements in high-quality data collection and, in the absence of new public policies, large-scale field experiments.

Our results and the previous observations matter to Scotland because of the sunset clause linked to the MUP legislation. They also matter to all the countries that are planning an implementation of the WHO’s SAFER initiative to effectively reduce harm on the road caused by alcohol misuse.

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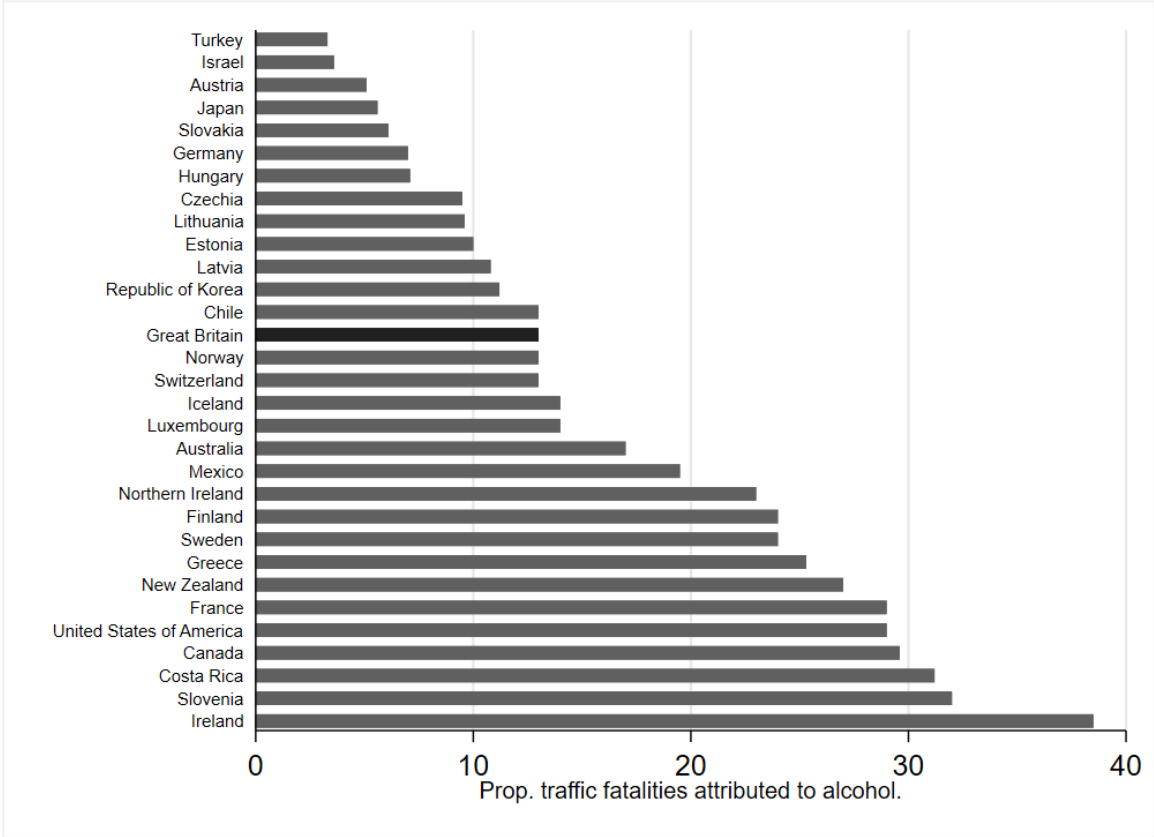
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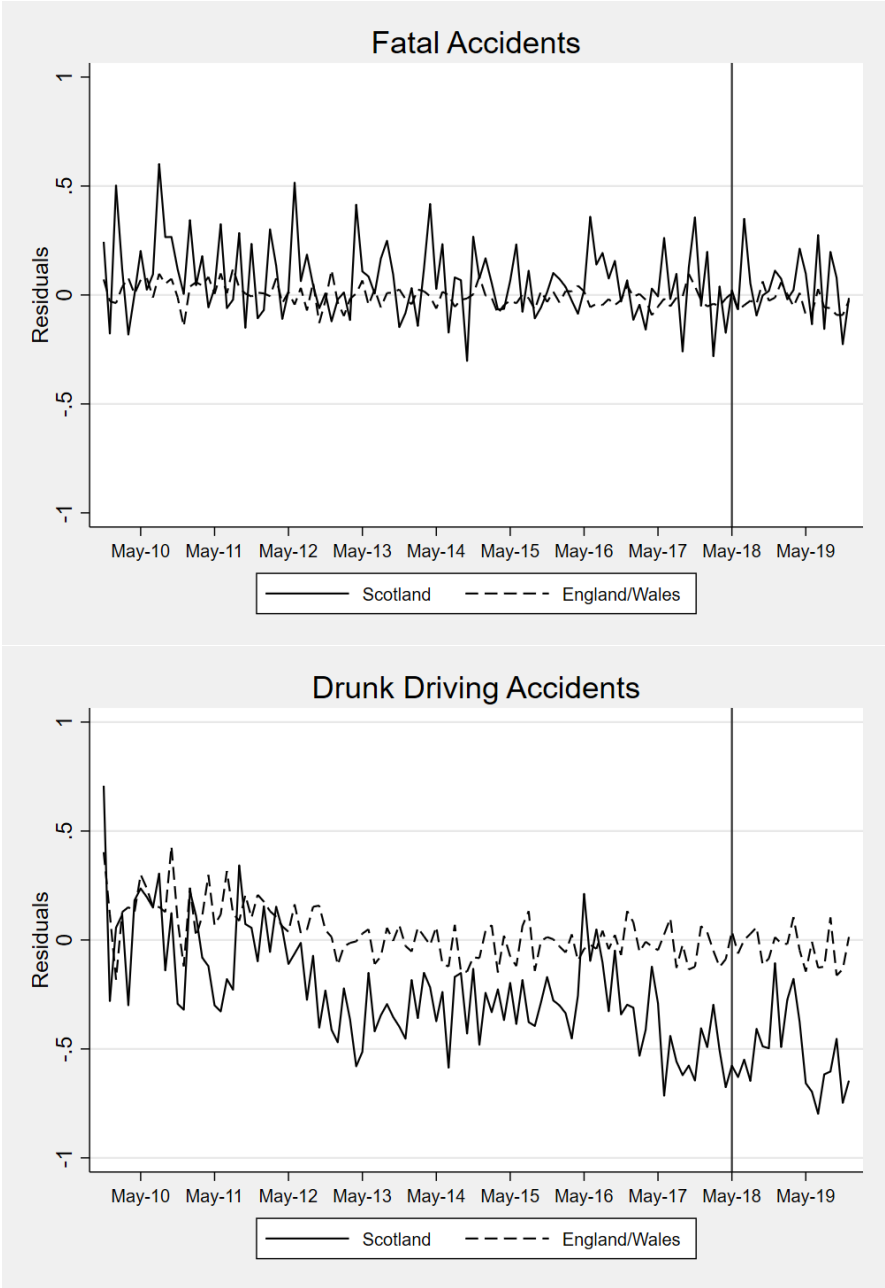
Figures and Tables

Figure 1: Proportion of Road Traffic Fatalities Attributed to Alcohol, Selected Countries (2017)



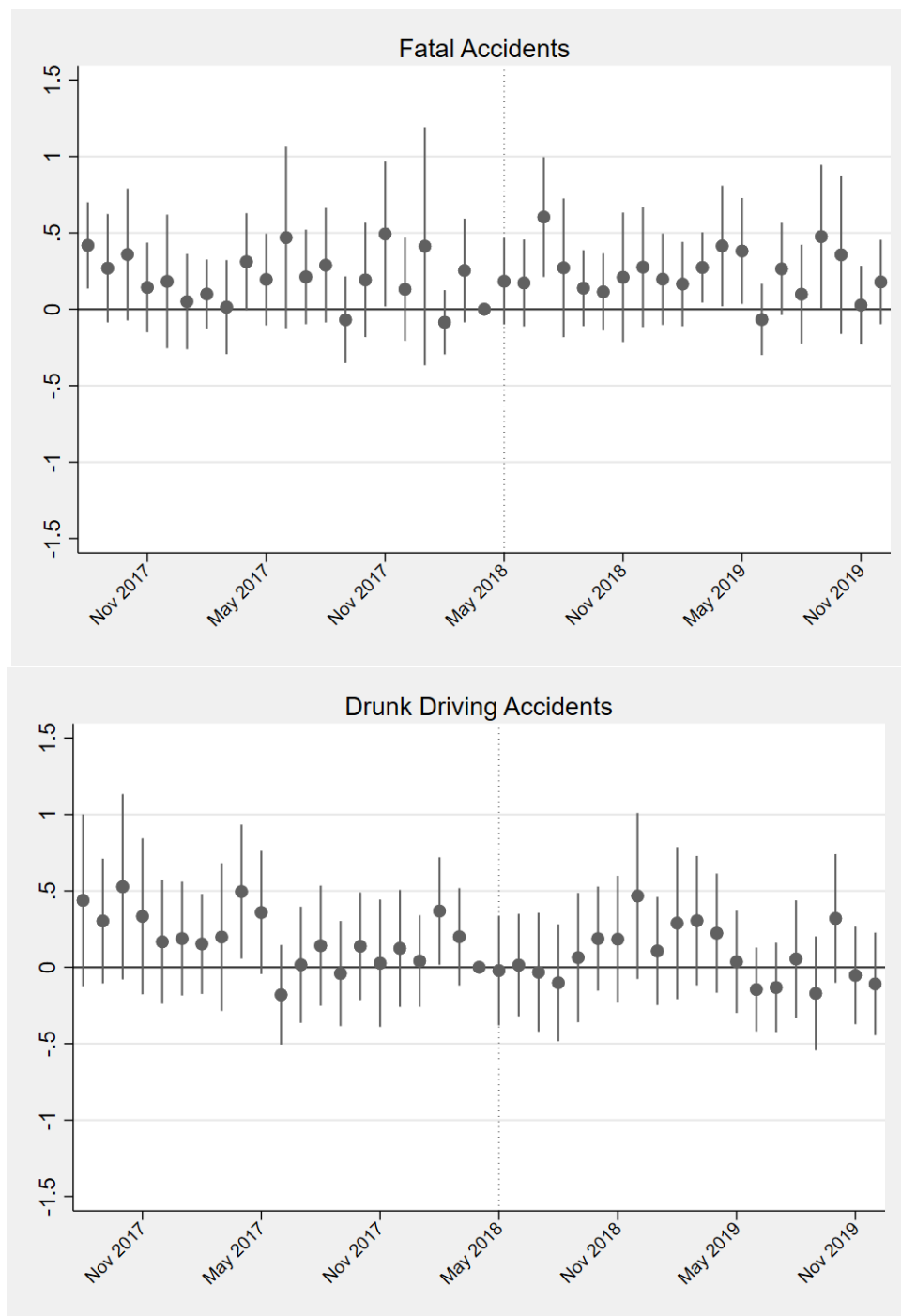
Source: World Health Organization (WHO), the Global Health Observatory.
<https://www.who.int/data/gho/indicator-metadata-registry/imr-details/208>

Figure 2: Deseasonalized Trends in Fatal and Drunk Driving Road Accident Rates: Scotland versus the Rest of Britain



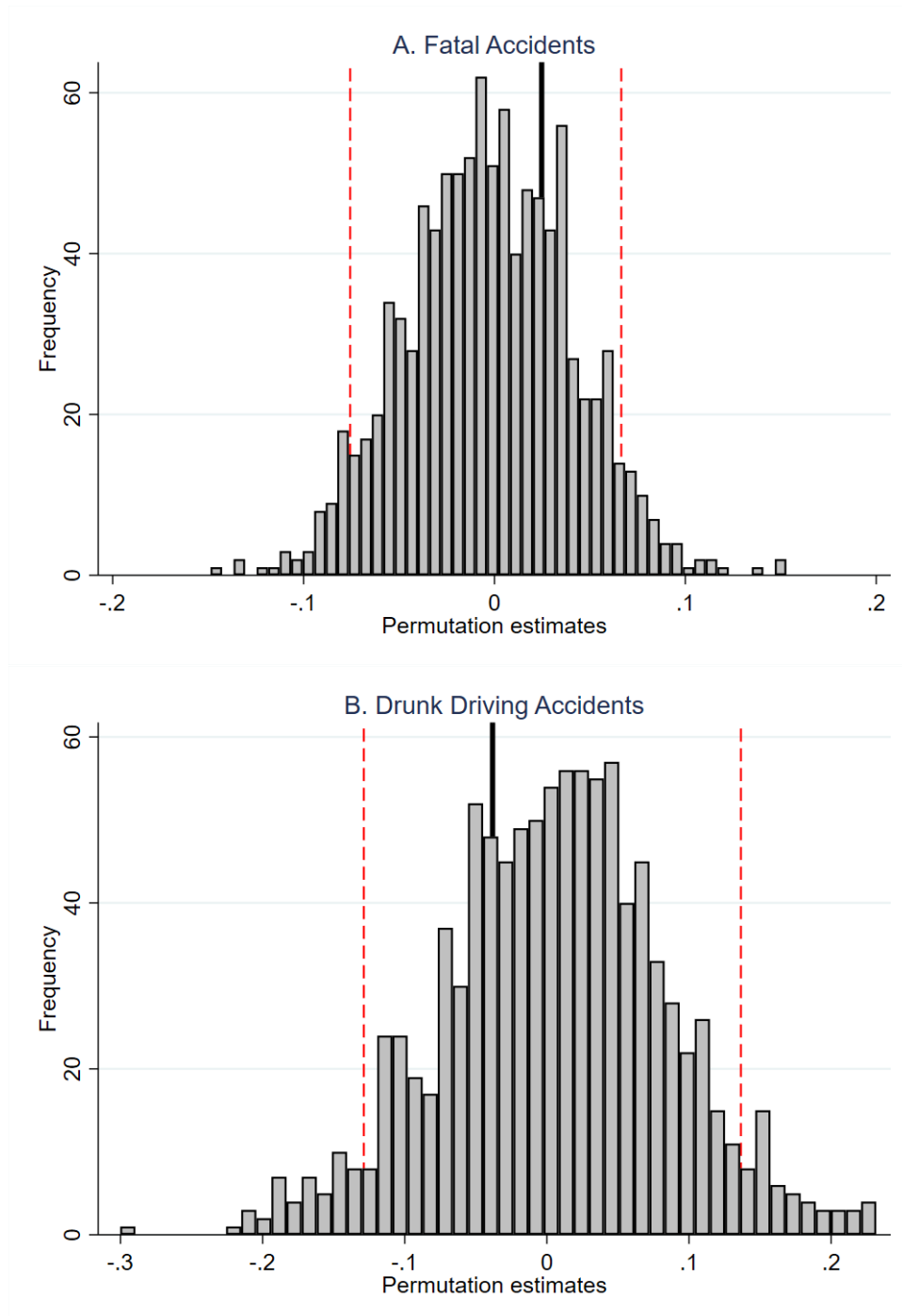
Sources: Road Accidents Data, Department for Transport, STATS19. Vehicle Licensing Statistics, Department for Transport.

Figure 3: Event study estimates for Fatal and Drunk Driving Accidents



Note: The dots display the β_m estimates from equation (3) and the 95% confidence interval around them in the months before and after the introduction of the MUP. Prior to the enactment of the policy. The sample is 20 months before and 20 months after the reform.

Figure 4: Permutation Based Inference: Fatal and Drunk Driving Accidents



Notes: The figure shows local authority district effects estimated from 1,000 permutation tests in Table 2 (column b) as explained in the text. The dashed vertical lines are 5th and 95th percentile values (other than Scotland). The solid vertical line is the Scottish estimate reported in Table 2.

Figure 5: Maps of Great Britain: Scotland versus Synthetic Scotland (Fatal and Drunk Driving Accidents)



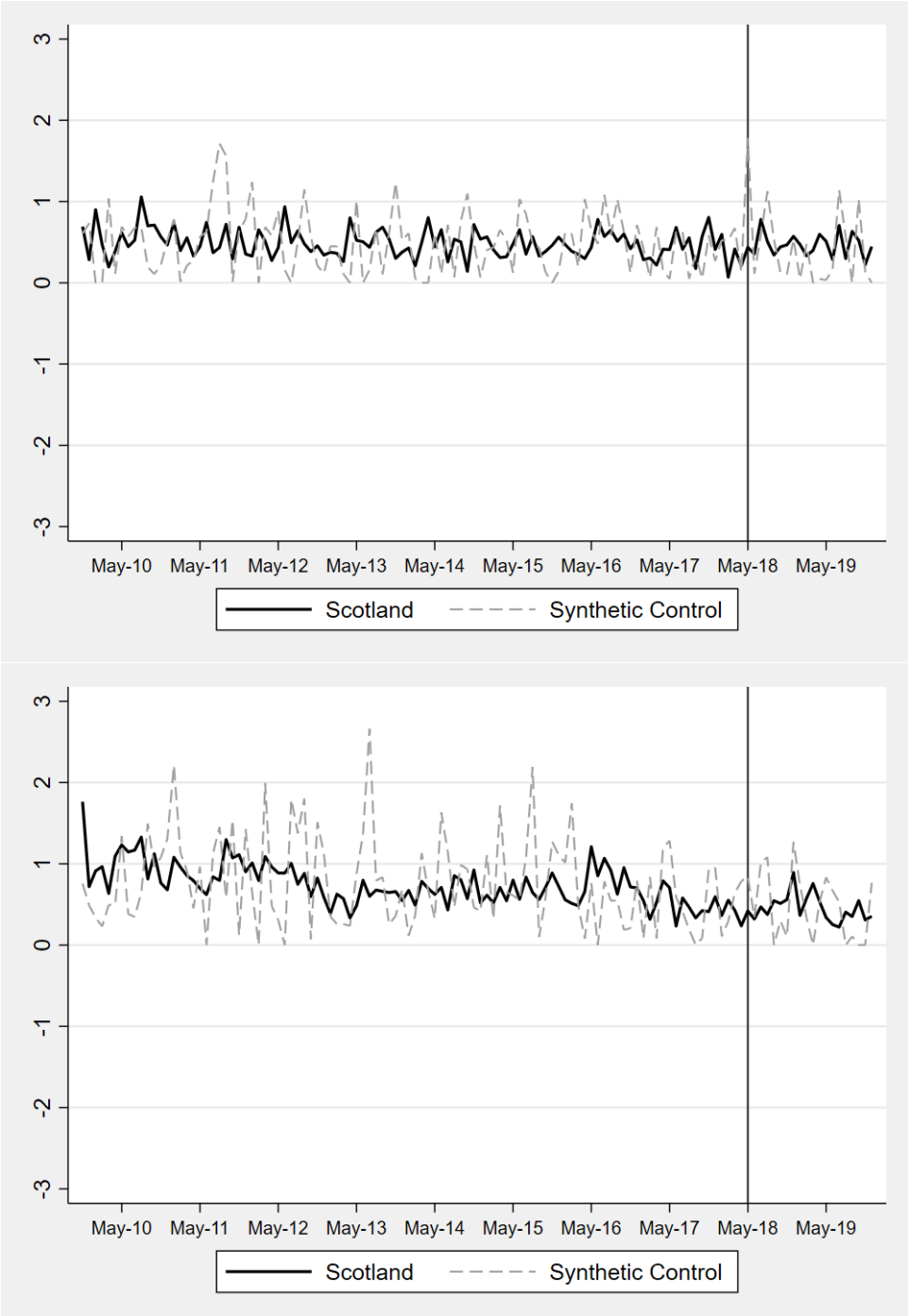
Notes: Local authority districts in dark grey identify Scotland. Local authority districts in light grey make up synthetic Scotland.

These are as follows (weight in parentheses):

Fatal Crashes: Havering (0.013), Eden (0.022), Oldham (0.055), Boston (0.006), Castle Point (0.02), Gwynedd (0.412), Wrexham (0.351)

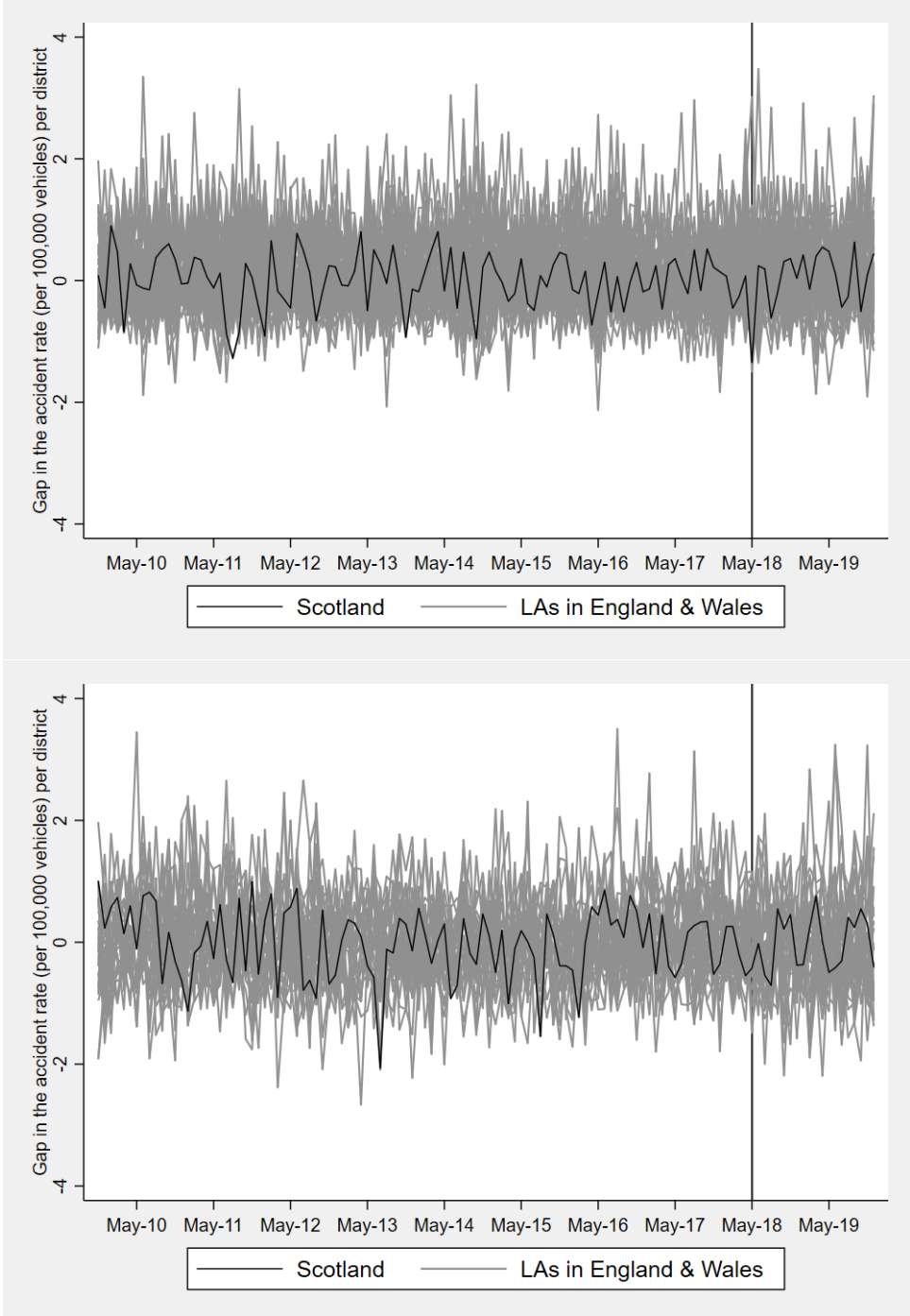
Drunk Driving Accidents: Knowsley 0.071, Trafford (0.002), Solihull (0.188), Castle Point (0.119), Gwynedd (0.420), Wrexham (0.200).

Figure 6: Trends in Fatal and Drunk Driving Road Accident Rates: Scotland versus Synthetic Scotland



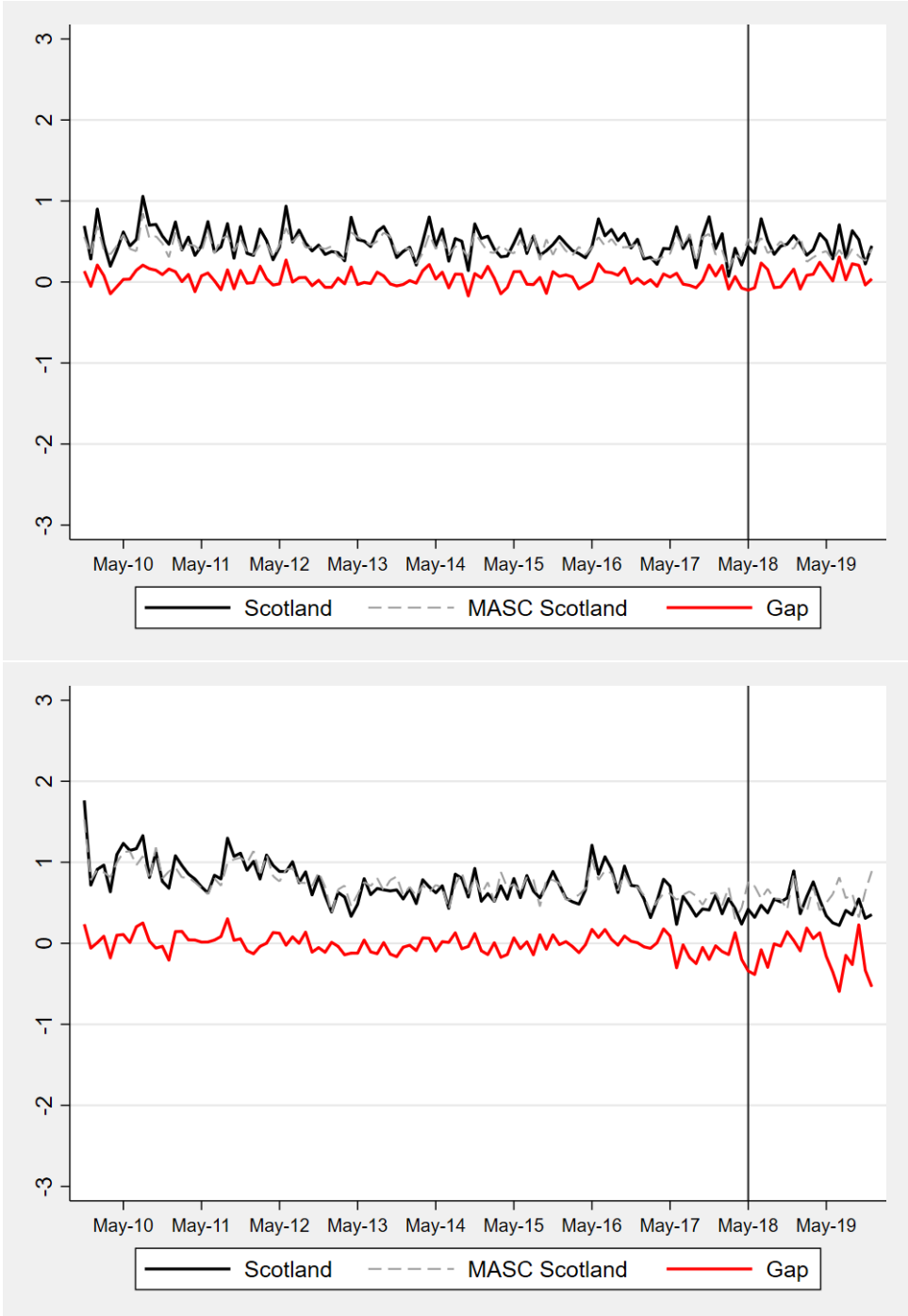
Notes: The top panel shows the estimates for fatal road accidents, the bottom panel shows those for drunk driving.

Figure 7: Gaps in Road Accident Rates for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs, Fatal and Drunk Driving Accidents



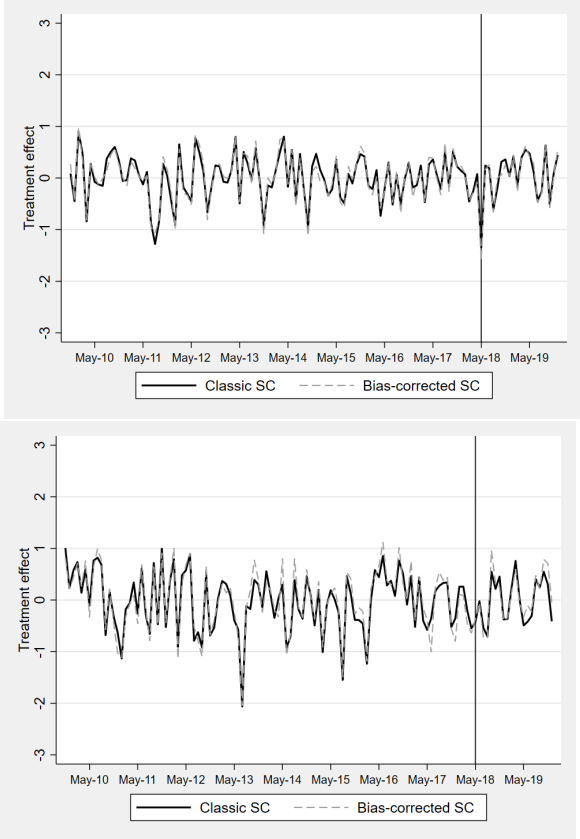
Notes: The top panel shows the estimates for fatal road accidents, the bottom panel shows those for drunk driving. In both panels, placebo districts with pre-reform mean squared prediction error (MSPE) that are more than two times higher than Scotland's are excluded. 'LAs' denotes local authority districts.

Figure 8: Trends in Fatal and Drunk Driving Road Accident Rates: Scotland versus MASC Scotland



Notes: The top panel shows the estimates for fatal road accidents, the bottom panel shows those for drunk driving. ‘MASC Scotland’ calculated using the matching and synthetic control approach proposed by Kellogg et al. (2019).

Figure 9: Treatment effects for the classic synthetic control and for the bias-corrected synthetic control



Notes: The top panel shows the estimates for fatal road accidents, the bottom panel shows those for drunk driving. Bias Corrected Synthetic Control Treatment effects are those proposed by Abadie and L'hour (2021).

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

	All LAs		Synthetic Scotland	
	Scotland	England & Wales	Fatal	Drunk Driving
<u>Accident rates (per 100,000 vehicles)</u>				
Fatal accidents	0.485	0.450	0.484	
		<i>0.567</i>		
Drunk Driving accidents	0.741	0.996		0.764
		<i>0.057</i>		
<u>Controls</u>				
Temperature Range	6.815	7.680	7.026	7.161
		<i>0.000</i>		
Population Density	4.079	12.90	4.250	5.267
		<i>0.000</i>		
Road Length	1873	1014	1646	1582
		<i>0.000</i>		
No Qualifications	26.85	22.47	25.39	25.41
		<i>0.000</i>		
Very bad/bad health	0.053	0.054	0.057	0.058
		<i>0.298</i>		
Mean weekly gross pay	409.5	424.5	382.5	385.6
		<i>0.025</i>		
Mean working hours	35.89	36.79	36.08	36.11
		<i>0.000</i>		
JSA rate	2.719	2.277	2.522	2.534
		<i>0.004</i>		
Total premise licenses	530.0	592.2	671.2	653.5
		<i>0.135</i>		

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. ‘Total premise licenses’ is the yearly number of (on-trade) premises registered in the LA with a legal license to sell alcohol.

Table 2: The Effect of MUP on Fatal and Drunk Driving Road Accident Rates — Difference-in-Difference Estimates

	Mean	(a)	(b)	(c)	(d)
A. Fatal Accidents					
Treatment effect (β)	0.485	0.028 (0.066)	0.025 (0.066)	0.027 (0.066)	0.028 (0.057)
Permutation effects (other LAs)					
5th percentile		-0.074	-0.076	-0.075	
95th percentile		0.067	0.066	0.067	
Two-tailed test p -value		0.821	0.539	0.782	
B. Drunk Driving Accidents					
Treatment effect (β)	0.741	-0.026 (0.068)	-0.038 (0.067)	-0.059 (0.066)	-0.025 (0.077)
Permutation effects (other LAs)					
5th percentile		-0.133	-0.129	-0.126	
95th percentile		0.136	0.136	0.140	
Two-tailed test p -value		0.719	0.573	0.375	
Rejection probability		5.8%	5.4%	4.9%	5.5%
Monthly trend		Y	Y	Y	Y
Monthly trend \times Scotland		Y	Y	Y	Y
Month of year dummies		Y	Y	Y	Y
Month of year dummies \times Scotland		Y	Y	Y	Y
Controls		N	Y	Y	N
Local Authority fixed effects		N	Y	Y	N
Additional trends		N	N	Y	N
Observations		45,704	45,704	45,704	244
Scottish Regions		31	31	31	1
English/Welsh Regions		345	345	345	1

Notes: Observations are at the local authority district-month-year level. The dependent variable is the number of accidents per 100,000 registered vehicles. ‘Treatment effect’ is the difference-in-difference estimate of the interaction of Scotland and post-MUP, β in equation (2). The sample period goes from November 2009 to December 2019. Standard errors in parentheses are clustered at the local authority district level. The 5th and 95th percentile confidence intervals from permutation tests and two-tailed test p -values are obtained from the Fisher’s randomization inference analysis based on the distribution of permutation effects described in the text. ‘Mean’ refers to the Scottish pre-reform mean of the dependent variable. ‘Controls’ are local authority 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). ‘Additional trends’ are four characteristic-specific linear trends interacted with population density, proportion of residents aged 16 or more with no educational qualification, Job Seekers’ Allowance rate, alcohol licensed premises. In columns (b) and (c), ϕ_2 from (2) is set to 0, because we control for LA fixed effects. In Panel B ‘Monthly trend’ and ‘Monthly trend \times Scotland’ are in quadratic form. ‘Rejection probability’ refers to the value of the inference assessment for a 5%-level test as proposed by Ferman (2021) and discussed in the text.

Table 3: The Effect of MUP on All, Serious- and Slight-Injury Road Accident Rates — Difference-in-Difference Estimates

	Mean	(a)	(b)	(c)	(d)
A. All Accidents					
Treatment effect (β)	24.4	-0.319 (0.655)	-0.408 (0.672)	-0.312 (0.669)	-0.316 (0.639)
Permutation effects (other LAs)					
5th percentile		-1.34	-1.32	-1.34	
95th percentile		1.50	1.46	1.46	
Two-tailed test p -value		0.230	0.563	0.661	
B. Serious-Injury Accidents					
Treatment effect (β)	4.28	0.425 (0.218)	0.405 (0.225)	0.418 (0.220)	0.433 (0.235)
Permutation effects (other LAs)					
5th percentile		-0.525	-0.524	-0.513	
95th percentile		0.534	0.524	0.504	
Two-tailed test p -value		0.146	0.192	0.144	
C. Slight-Injury Accidents					
Treatment effect (β)	19.7	-0.772 (0.628)	-0.838 (0.634)	-0.757 (0.627)	-0.777 (0.639)
Permutation effects (other LAs)					
5th percentile		-1.39	-1.38	-1.40	
95th percentile		1.45	1.43	1.46	
Two-tailed test p -value		0.0959	0.253	0.299	
Rejection probability		5.8%	5.4%	4.9%	5.5%
Monthly trend		Y	Y	Y	Y
Monthly trend \times Scotland		Y	Y	Y	Y
Month of year dummies		Y	Y	Y	Y
Month of year dummies \times Scotland		Y	Y	Y	Y
Controls		N	Y	Y	N
Local Authority fixed effects		N	Y	Y	N
Additional trends		N	N	Y	N
Observations		45,704	45,704	45,704	244
Scottish Regions		31	31	31	1
English/Welsh Regions		345	345	345	1

Notes: For details, see the notes to Table 2.

Table 4: The Effect of MUP on Fatal and Drunk Driving Road Accident Rates — Difference-in-Difference Estimates Found on Accurate Information on Alcohol Involvement

	Mean	(a)	(b)	(c)	(d)
A. Fatal Drunk Driving Accidents					
Treatment effect (β)	0.620	0.065 (0.294)	0.131 (0.316)	0.240 (0.314)	0.065 (0.333)
B. All Drunk Driving Accidents					
Treatment effect (β)	14.2	-0.482 (2.121)	0.856 (2.382)	-1.353 (1.810)	-0.482 (2.131)
Linear annual trend		Y	Y	Y	Y
Linear annual trend \times Scotland		Y	Y	Y	Y
Controls		N	Y	Y	N
Region fixed effects		N	Y	N	N
Additional trends		N	N	Y	N
Observations		110	110	110	20

Notes: Observations are at the region-year level. The sample period goes from 2009 to 2019. Robust standard errors are in parentheses. The regions are: North East, North West, Yorkshire and the Humber, East Midlands, West Midlands, East, South East, South West, Wales, and London (base category). Controls are the same as those used in Table 2 defined regionally at the annual level. The definition of a drunk driving accident is a reported incident on a public road in which someone is killed or injured, where at least one of the motor vehicle drivers or riders involved met one of the following criteria: (i) failed a roadside breath test by registering above $35 \mu\text{g}/100 \text{ ml}$ of breath (England and Wales) or $22 \mu\text{g}/100 \text{ ml}$ (Scotland) after December 2014; (ii) refused to give a breath test specimen when requested by the police, other than when incapable of doing so for medical reasons; (iii) died, within 12 hours of the accident, and was subsequently found to have more than 80 mg of alcohol per 100 ml of blood (England and Wales) or 50 mg (Scotland). For all the other details, see the notes to Table 2.