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RESEARCH REPORT

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The effect of a minimum price per unit of alcohol in Scotland on alcohol-related ambulance call-outs: A controlled interrupted time–series analysis

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Abstract

Background and aims: On 1 May 2018, Scotland introduced a minimum unit price (MUP) of £0.50 for alcohol, with one UK unit of alcohol being 10 ml of pure ethanol. This study measured the association between MUP and changes in the volume of alcohol-related ambulance call-outs in the overall population and in call-outs subsets (night-time call-outs and subpopulations with higher incidence of alcohol-related harm).

Design: An interrupted time–series (ITS) was used to measure variations in the daily volume of alcohol-related call-outs. We performed uncontrolled ITS on both the intervention and control group and a controlled ITS built on the difference between the two series. Data were from electronic patient clinical records from the Scottish Ambulance Service.

Setting and cases: Alcohol-related ambulance call-outs (intervention group) and total ambulance call-outs for people aged under 13 years (control group) in Scotland, from December 2017 to March 2020.

Measurements: Call-outs were deemed alcohol-related if ambulance clinicians indicated that alcohol was a 'contributing factor' in the call-out and/or a validated Scottish Ambulance Service algorithm determined that the call-out was alcohol-related.

Findings: No statistically significant association in the volume of call-outs was found in both the uncontrolled series [step change = 0.062, 95% confidence interval (CI) = −0.012, 0.0135 $P = 0.091$; slope change = −0.001, 95% CI = −0.001, 0.1×10^{-3} $P = 0.139$] and controlled series (step change = −0.01, 95% CI = −0.317, 0.298 $P = 0.951$; slope change = −0.003, 95% CI = −0.008, 0.002 $P = 0.257$). Similarly, no significant changes were found for the night-time series or for any population subgroups.

Conclusions: There appears to be no statistically significant association between the introduction of minimum unit pricing for alcohol in Scotland and the volume of alcohol-related ambulance call-outs. This was observed overall, across subpopulations and at night-time.

Francesco Manca is the first author.

KEYWORDS
Alcohol, ambulance, interrupted time−series, minimum unit price, natural experiment, Scotland

INTRODUCTION

In 2018, it was estimated that alcohol was responsible for 5.1% of the global burden of disease and injury [1], causing both acute and long-term conditions. Alcohol-related harm, which includes injuries, violence and other accidents related to binge drinking, also places a significant strain on health-care and emergency services. Alcohol consumption in western Europe is among the highest in the world [2], and 24% of adults in the United Kingdom regularly exceed drinking the Chief Medical Officer's low-risk guidelines [3, 4]. In Scotland there are substantially higher levels of alcohol sales and alcohol-related harm than the rest of the United Kingdom [5, 6].

In 2018, to reduce consumption and alcohol-related harm in the population, Scotland implemented a minimum unit pricing policy (MUP) for alcohol of £0.50, meaning that one UK unit of alcohol (10 ml or 8 g of ethanol) cannot be sold below this threshold. As the incidence of alcohol-related harm is higher in the most socio-economically deprived areas the policy was also expected to have a greater impact upon people living in such areas, with a consequent decrease in the significant health inequalities that exist in Scotland. After 1 year of the policy, off-trade alcohol sales were observed to have fallen by 3.5% [7], an effect that was largely sustained at 3-year follow-up [8]. As overall consumption decreased, it is reasonable to expect that some alcohol-related harms would also reduce. Studies to date have found inconclusive effects of MUP on several acute measures of alcohol harm, such as no changes in alcohol-related crime [9] or in alcohol-related harms attendance within emergency departments [10]. Further, studies showing associations between MUP and decreases in deaths and hospitalizations attributable to alcohol did not find evidence of changes in hospitalizations for acute causes [11]. The legislation that introduced MUP contains a 'sunset clause', meaning that it will end after 5 years of its implementation unless the Scottish Parliament votes for it to continue. This decision will be informed by a large body of evaluation evidence, to which this study will add an additional perspective.

Many aspects of acute alcohol-related harm are typically under-reported [12,13]. Ambulance services are often the first and only health-care providers in contact with some patients who are treated in the community and who may therefore be missing from emergency departments and admissions data. This is reflected in Scottish data where, in 2019, the alcohol-related attendance to emergency departments was 8% [10], while the proportion of alcohol-related ambulance call-outs was 16% [14]. For these reasons, ambulance call-outs may be considered a 'more sensitive' thermometer of the effect of a public health policy than hospital data, especially reflecting the impact of the policy on acute alcohol-related harm. The only other international study analysing the effect of similar MUP policy on ambulance call-outs was an interrupted time−series study from Australia's Northern Territory [15]. It reported a significant negative step change but not a significant slope change in the rate of ambulance attendance post-MUP in the region. To date, no published peer-reviewed study has examined the impact of MUP or any other increase in alcohol prices on ambulance call-outs.

This study aimed to identify whether the introduction of MUP in Scotland was associated with changes in the overall volume of alcohol-related ambulance call-outs and whether there were variations across time of the day, sex, age of the patient or level of socio-economic deprivation of the call-out location.

METHODS

Study design

We used a controlled, interrupted time−series design to evaluate the impact of the introduction of MUP on alcohol-related ambulance call-outs after 20 months. We used ambulance call-outs to people aged under 13 years for any cause as a characteristic-based control, as this outcome was not expected to be impacted by the policy. However, it covered the same geographical area and was assumed to be affected by the same environmental and other unmeasured confounding factors [16].

Data set

Our data provider was the Scottish Ambulance Service (SAS), which supplied a nation-wide data set containing selected anonymized fields of all electronic patient record forms (ePRFs) of ambulance call-outs for all Scotland from 1 May 2015 to 31 October 2021, covering 3 years prior to and 2.5 years after MUP implementation. Every call-out contained information on patients' demographic characteristics, as well as deprivation deciles of the call-out location, assessed using the Scottish Index of Multiple Deprivation (SIMD) (a relative measure of deprivation ranking Scottish areas based on income, employment, access to services, health, crime, housing and education) [17]. The data set also included two markers of whether or not the call was alcohol-related: first, a marker made by ambulance clinicians at time of filling in the ePRF by selecting an on/off field to indicate whether alcohol was a 'contributing factor' in the call-out and secondly, a yes/no marker generated from an algorithm embedded in the SAS system, which analyses the free-text report in every ePRF and detecting whether the call-out was alcohol-related. Each individual record was deemed to be alcohol-related for this study if either of these markers were positive for alcohol involvement. This indicator, combining the yes/no field with the algorithm field, was developed and validated previously and found to have a sensitivity of 94%. A detailed description of the algorithm development and performance is given elsewhere [14]. Based on the algorithm, the definition of 'alcohol-related call-outs' comprises any call-out recording alcohol on the ePRF as a primary cause for care (i.e. alcohol intoxication) or in those calls where the consumption of alcohol was recorded in association with the presenting condition/injury (i.e. mental health crises, falls or assaults).

While the full data set contained records from 1 May 2015 to 31 October 2021, SAS changed their software system for recording call-outs at the end of 2017, with implementation phased in throughout different Scottish regions over several months. In this latest version of the recording system, clinicians could indicate that alcohol (or other substances) was a contributing factor in a given call-out in multiple sections of the ePRF, and as a result the implementation of the new system created a gradual increase in the volume of call-outs identified as alcohol-related, lasting over the implementation period (a couple of months), followed by a more stable level of alcohol-related call-outs from December 2017 (only 5 months before the MUP introduction). Such variation in data collection systems could generate structural breaks in the time−series, and for this reason, whenever data collection processes are inconsistent over time, it is recommended to truncate the analysis period for interrupted time−series analyses [18]. To avoid potential bias in our analysis, we chose to perform the main analysis only on the period when the new system was fully adopted (after 15 December 2017). The analytical data set was further truncated in March 2020 to avoid additional bias as a result of the COVID-19 pandemic and lockdown. The lockdown period affected not only alcohol-related ambulance call-outs but potentially also consumption patterns [19], with probably long-lasting effects. Consequently, for the main analysis, the final data set was from 15 December 2017 to 15 March 2020.

To calculate the overall effect of MUP on the burden of alcohol to the ambulance service, we would ideally analyse the number of individual patients treated by ambulance crews. However, SAS classifies its incidents in terms of call-outs, and the data do not record the actual number of patients involved in every call-out. Therefore, the unit of measurement in this analysis is 'call-out'. When we sub-grouped analysis by age, sex and deprivation (see below), whenever multiple ePRFs were recorded within the same incident the average of age and sexes was considered. A minor proportion of accidents had the same number of female and male records; when this happened, we reported it as 'male' in the main analysis. We conducted a counter-analysis changing this to 'female', and our results were insensitive to this choice.

As the characteristic-based control was determined by age (under 13-year-olds), all call-outs having missing age were removed. A small number of call-outs in this control group were identified by the algorithm as being alcohol-related (and therefore fell into both the control and intervention categories); these records were removed from the analysis. As these overlapping observations were uniformly distributed pre- and post-MUP introduction, this was unlikely to have any significant impact upon our results (Table 1).

TABLE 1 Number of alcohol-related and control (aged under 13 years) call-outs by demographics, 15 December 2017–15 March 2020.

No. of call-outs	Alcohol (%) 190 177		Under 13 (%) 58 919	
Socio-economic deprivation quintiles				
1 (most deprived)	66 399	(35.2%)	17 360	(30.0%)
2	47 436	(25.1%)	12 866	(22.3%)
3	35 108	(18.6%)	10 814	(18.7%)
4	24 337	(12.9%)	9268	(16.0%)
5 (least deprived)	15 577	(8.2%)	7504	(13.0%)
Missing	1320		1107	
Sex				
Female	69 788	(37.9%)	22 010	(42.5%)
Male	11 4318	(62.1%)	29 776	(57.5%)
Missing	6071		7133	
Age (years)				
13–25	28 966	(15.7%)	–	
26–45	52 831	(28.6%)	–	
46–65	59 010	(32.0%)	–	
> 65	43 771	(23.7%)	–	
Missing	5599	(3.0%)	–	
Sample size after removing call-outs with missing age	180 355		58 919	
Sample size after removing aged under 13 classified as alcohol	174 756		53 320	

Statistical analysis

We fitted a Seasonal Autoregressive Moving Average (SARIMA) model, able to account for autocorrelation, seasonality and underlying temporal trend. Our main model analysed the potential for a linear change in both level and slope at the point of intervention. We based this upon a potential hypothesis of a gradual effect (in case there was one), which was then tested with information criteria [Akaike information criterion (AIC) and Bayesian information criterion (BIC) statistics] of separate models. We used daily units of time. While a daily series may present challenges (e.g. double seasonality, both weekly and yearly), aggregation to weekly or monthly data points would have significantly reduced the statistical power of the analysis, due to the restricted size of the original data set following truncation for changes in reporting and the impact of the pandemic.

Daily data allowed us to control for potential time-varying confounders such as weather, which is a factor likely to have a role in alcohol consumption [20]. We had only rainfall available, and as we were assessing the overall weather for Scotland, which has sensitive differences within its territory to have a single national figure, we averaged the daily mm of rainfall in different districts with data from the Scottish Environment Protection Agency (www.sepa.org.uk). We used this as a proxy of national weather conditions. Other variables included in the model were bank holidays, months, New Year's Eve, other ambulance call-outs (any call-out different than alcohol-related or aged under 13 years) and Old Firm football matches. Old Firm matches are games between the two main football teams in Glasgow (Celtic and Rangers); such events have been associated with reports of domestic abuse potentially related to alcohol consumption and misuse [21]. We included this as a covariate, as the metro area population of Glasgow is almost a third of the Scottish population.

To reduce the skewness of our original data, we log-transformed our dependent variable. An advantage of the log transformation is that coefficients in the regression model can be interpreted as a percentage variation of the series. Our seasonal component was weekly, considering the excess in alcohol-related call-outs during weekends (almost 40% occurred on Saturday or Sunday [14]); for monthly adjustments, we inserted a categorical variable for months into our model, excess in specific days. Details of the SARIMA equation and model are reported in the Supporting information.

We performed the same analysis for our characteristic-based control outcome. We then tested for common parallel assumption by regressing the difference between series over time in the pre-intervention period [22]. After verifying the parallel trend, we ran a separate analysis on the difference between control and intervention series which, by incorporating the control into the same model as the intervention analysis, can be interpreted as a difference in difference estimator. Finally, we performed the same analysis for night-time call-outs (8 p.m. to 6 a.m.), as it was the time of the day with the highest concentration of alcohol-related call-outs [14]. All analyses were conducted using Stata version 17 [23]. This analysis was not pre-registered, and results should be considered exploratory.

Subgroup and sensitivity analyses

Subgroup analysis on different socio-economic deprivation quintiles [17], age groups (13–25, 26–45, 46–65 and > 65 years) and sex was performed on the intervention series.

Alternative modelling strategies using panel data and regressions with Newey–West standard errors based on Scottish districts were also employed, using district level covariates (e.g. rainfall levels). However, several areas with zero events over multiple dates and the consequent potential floor effects made SARIMA models on overall Scotland preferred for baseline analysis. We also ran models with weekly data; however, due to limited data points in the pre-intervention period and potential power issues, we used these as sensitivity analyses. In a further sensitivity analysis, to use all the available information prior to lockdown restrictions (May 2015–March 2020), allowing for a longer pre-intervention period, we employed a cubic spline model [24] to mitigate the fluctuation in call-outs given by changes in the recording system. Falsification tests 6 and 12 months after the intervention date were performed.

RESULTS

Alcohol-related call-outs follow a seasonal pattern with peaks at weekends and large peaks on New Year's Eve (Figure 1). In contrast, call-outs for under 13-year-olds have little variation during the week but follow more of a monthly seasonality, with an increase from September to December followed by a gradual decrease over the year (Figure 1). The overall distribution of sex and socio-economic deprivation was similar in the two groups (Table 1). Of the alcohol-related call-outs, 2.9% were also identified as aged under 13; these records were removed from the analysis (Table 1).

While the mean number of daily alcohol-related call-outs before MUP implemented was 194.3 [standard deviation (SD) = 51.0] and after implementation was 216.3 (SD = 52.4), a relative increase of 11.3%, the inferential analysis, considering seasonal and temporal trends, found that the implementation of MUP was not associated with a significant change in daily alcohol-related call-outs [step change, interpretable as an instant change in correspondence to the intervention = 0.062, 95% confidence interval (CI) = −0.012, 0.0135; slope change, the daily gradual change after intervention = −0.001, 95% CI = −0.001, 0.0001] (Table 2). For the control group, the mean number before MUP implementation was 68.4 (SD = 10.8) and 72.3 after (SD = 16.2), a 5.7% growth, with the inferential analysis also finding no significant step change.

Similarly, the difference between the intervention and control group did not show a significant result (step change = −0.010, 95% CI = −0.317, 0.298, slope change = −0.003, 95% CI = −0.008, 0.0001). There were no significant results when the analysis was restricted to alcohol-related call-outs at night-time only. In all the analyses on total and night-time alcohol-related call-outs, the slope change tended to be of the same size but in the opposite direction of the overall trend in the model, meaning that the volume of call-outs remained stable.

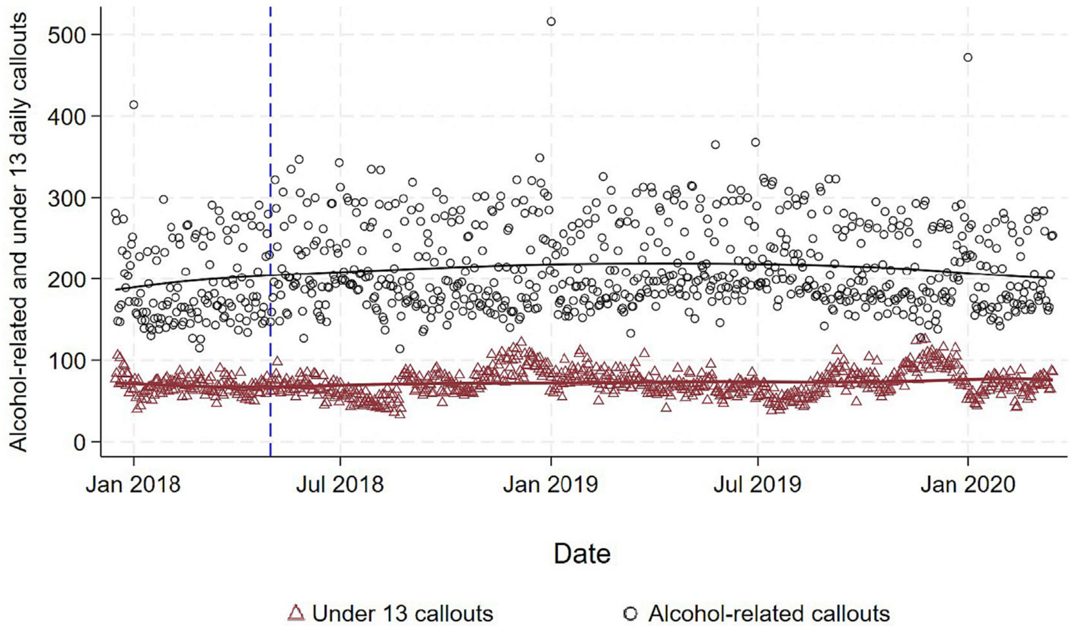

FIGURE 1 Time–series of daily alcohol-related and age under 13 years call-outs. The dashed blue line is in correspondence of the minimum unit pricing implementation date. Solid lines are local linear smooth plots of alcohol-related (black) and age under 13 years (maroon) call-outs.

Subgroup and sensitivity analyses

We found no evidence of a significant decrease in alcohol-related call-outs associated with MUP for any of the subgroups examined (different age groups, sex or call-outs to locations with different levels of deprivation). These findings are presented in the Supporting information. Where there were statistically significant changes, these happened only for measures at mid ranks of a category (e.g. variations in third and fourth socio-economic deprived quintiles, but not in the least or most deprived groups), we believe these probably arise from daily spurious variations. Similarly, significant step changes (in female patients, call-out locations in the most deprived quintile and those aged 46–65 years) on the day of the implementation of MUP are more likely to represent spurious noise in the data rather than attributable to an instant effect on the first day of MUP.

Falsification tests for total alcohol-related call-outs produced significant change in slope and trend when the intervention point was set at 6 months after the introduction of MUP and a significant change in slope, trend and overall underlying trend when set at 12 months after (Table 2). In contrast, falsification tests for the control and the difference between series were not significant. Regarding the night-time analysis, both 6- and 12-month falsification tests in the uncontrolled intervention series had significant results for overall trend and slope change. In the difference between series, only the falsification test postponing the intervention by 6 months had significant coefficients for step change (−0.143, 95% CI = −0.2635, −0.0223), slope change (−0.001, 95% CI = −0.001, 0.0001) and overall trend (0.001, 95% CI = 0.0002, 0.0012) with an expected gradual effect at the end of follow-up equal to a 35% decrease. Given the different sign on the change in slope coefficient in the 12-month falsification test, a probable explanation could be that this is a spurious effect rather than a lagged gradual effect of the policy.

Alternative models such as Newey–West with heteroskedastic and autocorrelated errors did not show significant results in both uncontrolled ITS and in the difference between the series (Supporting information). Similarly, the analysis on weekly data provided similar results to the main analysis, showing null effect of the policy across all uncontrolled and controlled series. All these analyses had non-significant coefficients of overall and post-intervention trends, but of the same extent and opposite sign, mirroring the baseline models (Supporting information). The sensitivity analysis using cubic spline on weekly data, but starting from May 2015 showed no significant results; however, it highlighted how the excessive increase in the outcome after the change in the recording system (December 2017) is greater than expected, and would have potentially added bias into our estimates (for the visual inspection of the spline and the visual effect of the change in system in the series, see Supporting information).

DISCUSSION

We did not find associations between MUP implementation and variations in the daily volume of alcohol-related call-outs. There was also no evidence of significant variations in subpopulations and, in particular, among different socio-economic groups (relevant subanalyses to assess the MUP effects on health inequality).

There could have been several reasons explaining our null findings; here, we discuss three reasons. First, many alcohol-related ambulance call-outs are generated by alcohol consumption during

TABLE 2 Regression outputs of logarithm of alcohol-related call-outs (aged under 13 years of age call-outs and difference between the two series).

	Main analysis				Falsification test 6 months				Falsification test 12 months			
Coefficient		*P*-value	95% CI		Coefficient	*P*-value	95% CI		Coefficient	*P*-value	95% CI	
Alcohol, overall												
Step change	0.062	0.091	−0.012	0.1354	0.011	0.694	−0.042	0.063	−0.067	0.009	−0.116	−0.017
Slope change	−0.001	0.138	−0.001	0.1×10^{-5}	−0.001	0.000	0.2×10^{-3}	0.001	-0.4×10^{-3}	0.005	−0.001	-0.1×10^{-3}
Overall trend	0.001	0.136	-0.1×10^{-5}	0.001	0.4×10^{-3}	0.000	0.2×10^{-3}	0.001	0.3×10^{-3}	<0.001	0.2×10^{-3}	0.4×10^{-3}
U13, overall												
Step change	0.006	0.949	−0.192	0.205	0.027	0.754	−0.145	0.200	−0.029	0.724	−0.192	0.133
Slope change	−0.001	0.513	−0.003	0.002	-0.3×10^{-3}	0.430	−0.001	0.001	-0.3×10^{-3}	0.533	−0.001	0.001
Overall trend	0.001	0.515	−0.001	0.003	0.2×10^{-3}	0.551	−0.001	0.001	0.2×10^{-3}	0.261	-0.1×10^{-3}	0.001
Difference, overall												
Step change	−0.01	0.951	−0.317	0.298	−0.074	0.584	−0.340	0.191	−0.084	0.208	−0.214	0.047
Slope change	−0.003	0.257	−0.008	0.002	−0.001	0.504	−0.004	0.002	−0.001	0.516	−0.004	0.002
Overall trend	0.003	0.252	−0.002	0.007	0.001	0.430	−0.001	0.003	0.001	0.421	-0.1×10^{-4}	0.002
Alcohol, night-time												
Step change	0.056	0.338	−0.059	0.171	−0.039	0.472	−0.145	0.067	0.004	0.917	0.077	0.086
Slope change	−0.001	0.054	−0.002	0.1×10^{-5}	−0.001	0.000	−0.001	-0.4×10^{-3}	−0.001	0.001	−0.001	-0.3×10^{-3}
Overall trend	0.001	0.074	-0.1×10^{-5}	0.002	0.001	0.001	0.3×10^{-3}	0.001	0.3×10^{-3}	0.006	-0.9×10^{-4}	0.001
U13 night-time												
Step change	0.022	0.846	−0.205	0.247	0.108	0.180	−0.05	0.267	−0.017	0.848	−0.187	0.154
Slope change	-0.4×10^{-3}	0.719	−0.003	0.002	-0.5×10^{-4}	0.855	−0.001	0.001	-0.4×10^{-3}	0.395	−0.001	0.4×10^{-3}
Overall trend	0.001	0.688	−0.002	0.003	-0.1×10^{-4}	0.909	−0.001	0.001	0.2×10^{-3}	0.118	-0.6×10^{-3}	0.5×10^{-3}
Difference, night-time												
Step change	0.123	0.138	−0.040	0.285	−0.143	0.020	−0.264	−0.022	−0.107	0.113	−0.240	0.025
Slope change	0.0004	0.638	−0.001	0.002	−0.001	0.009	−0.001	-0.2×10^{-3}	0.12×10^{-3}	0.422	-0.4×10^{-3}	0.001
Overall trend	−0.001	0.523	−0.002	0.001	0.001	0.010	0.2×10^{-3}	0.001	0.1×10^{-3}	0.687	-0.1×10^{-3}	0.3×10^{-3}

Abbreviation: CI = confidence interval.

weekends and night-times [14], much of which takes place in licensed premises (bars, clubs), and MUP does not affect the price of alcohol sold in such premises. We found a potential lagged effect of MUP in a decrease in alcohol-related call-outs at night-time after 6 months, but we believe that this was most probably a spurious finding, as it was not supported by any theory of change *ex-ante* (see below).

Secondly, the types of drinking or individuals that generate alcohol-related ambulance call-outs may not be as price-elastic as other alcohol consumption. This may be caused by the extent of the policy (£0.50) that could have been too low to affect acute outcomes, which constitute a relevant part of ambulance call-outs. This would be in line with other studies showing null or controversial associations between MUP and acute outcomes (e.g. road traffic accidents [25] and hospitalizations [11]). Understanding what percentage of alcohol-related ambulance call-outs are linked to off-trade versus on-trade consumption and to single-occasion versus dependent drinking would help to unpick what happened. Such data are not available in Scotland, although there have been initiatives in emergency departments to identify where people were drinking prior to an alcohol-related visit [26]. Finally, the overall reduction in consumption due to MUP may have been generated by small reductions by a large population of drinkers. This is supported by an overall reduction in consumption (3% after 3 years [8]), but with limited evidence on alcohol-addicted [27] and harmful drinkers [28]. While this would have long-term benefits in terms of reduced alcohol-related disease (e.g. alcoholic liver disease, as already shown in midterm evaluations [11]), which would be expected to reduce pressure on the health service, it would have minor effects on our outcome measure in this study, which is more focused upon acute harm.

The significant increasing trends in alcohol call-outs in a few of the subgroups may have appeared because our analysis started in December and included only a few months (not an entire year) before MUP implementation. Therefore, a short pre-intervention period could limit the analysis trend, which shows higher variability (standard deviations) after MUP implementation for both series. This could explain why the effect of the underlying trend is always offset by the slope change, which describes a flat curve after MUP. We found several step changes in our subgroup analyses as well as in some falsification tests; however, it is worth noting that we used daily data that are subjected to more noise due to the high level of granularity. To avoid misleading conclusions, we focused more upon significant changes in slope (which imply a continuous change over time from the intervention) rather than on step changes. When falsification tests provided significant results in the overall uncontrolled alcohol-related series, we found no evidence of effect in the series of the differences and similar results (but not statistically significant) in the control (aged under 13 years) series. The lack of significance may be due to power issues, as the number of under 13-year-old call-outs was lower than the number of alcohol-related ones. Therefore, we would not force the interpretation of the significance of our falsification tests as a delayed effect of the policy. In contrast, in night-time call-outs we found a significant decrease in falsification tests after 6 months, both in the uncontrolled series and in the series of the difference. While this could be interpreted as a lagged effect of the policy, the 12-month falsification test found a non-significant increase suggesting that such results were probably due to daily noise or the use of a suboptimal control group, rather than lagged effect of the policy.

We believe that our study provides a valuable contribution to evaluating the impact of MUP in Scotland by focusing on its impact on alcohol-related ambulance call-outs; that is, on an acute and critical frontline emergency service. The use of a reliable measure of alcohol-related ambulance call-outs throughout the whole of Scotland is a strength, although our study also has several limitations. Most importantly, the power of the study was limited by the short pre-intervention period due to the change in the recording system and the consequent use of daily data that we explained extensively earlier. ITS using daily data can detect quick and instant variations; however, they may have more challenges in detecting persistent long-term effects. Therefore, our results are more robust regarding short-term daily variations, and may be less definitive and affected by more uncertainty for inference of long-term repercussion of the policy. This could be ascribed as the main limitation of our study, as policymakers may be more alert to long-term implications. However, despite the lower power, our analyses on weekly series (more likely to effectively model cyclical patterns) showed similar results. Other potential limitations are related to our control group. We found different seasonal patterns between the intervention and control series [alcohol-related call-outs followed weekly fluctuations, while under 13-year-old call-outs had a yearly seasonality (Figure 1)]. In addition, certain covariates (e.g. weather) affected the two series differently, suggesting that the two populations were intrinsically different. However, we controlled for weather and we believe that this was the best control available to us, as a location-based controls such as that used in similar analyses on MUP in Scotland [7,9] would have been difficult. For instance, there are multiple ambulance trusts in the rest of the United Kingdom, and none have embedded a reliable marker for identifying that call-outs are alcohol-related [29]. In addition, while there could be theoretical concerns, our control satisfied statistical tests for parallel trend assumption in both daily and weekly models. While not ideal, our characteristic-based control accounted for local variations such as weather, specific bank holidays, features within ambulance service such as changes in ambulance service provision and the National Health Service (NHS) funding environment.

CONCLUSIONS

Minimum unit pricing for alcohol set at a rate of £0.50 per UK unit of alcohol and implemented in Scotland in May 2018 was not associated with changes in the volume of alcohol-related ambulance call-outs. Further, no reduction in such call-outs was associated with MUP for any subgroups analysed, including different sexes, ages or the level of deprivation of the call-out location. The limited impact of MUP upon dependent drinkers and on prices in bars/clubs most probably explains these findings.

AUTHOR CONTRIBUTIONS

Francesco Manca: Conceptualization (supporting); formal analysis (lead); methodology (lead); visualization (lead); writing—original draft (lead); writing—review and editing (equal). **Jim Lewsey:** Conceptualization (equal); funding acquisition (equal); methodology (equal); supervision (lead); writing—review and editing (lead). **Daniel Mackay:** Conceptualization (equal); formal analysis (supporting); funding acquisition (equal); methodology (supporting); supervision (supporting); validation (lead); writing—review and editing (equal). **Colin Robert Angus:** Conceptualization (equal); funding acquisition (equal); supervision (equal); visualization (supporting); writing—review and editing (equal). **David Fitzpatrick:** Conceptualization (equal); funding acquisition (equal); writing—review and editing (equal). **Niamh Fitzgerald:** Conceptualization (lead); funding acquisition (lead); supervision (equal); writing—review and editing (lead).

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DECLARATION OF INTERESTS

All authors have nothing to declare with respect to any current or potential interest or conflict in relation to this study.

DATA AVAILABILITY STATEMENT

The data that support the findings of this study were obtained from SAS. Restrictions apply to the availability of these data, which were used under license for this study.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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