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Lockdown drinking: The sobering effect of price controls in a pandemic

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Abstract

Lockdown restrictions reduce the spread of COVID-19 but disrupt livelihoods and lifestyles that can induce harmful behavior changes, including problematic lockdown drinking fueled by cheap alcohol. Exploiting differences amongst the four constituent countries of the United Kingdom, we use triple difference analysis on alcohol retail sales to examine the efficacy of minimum unit pricing as a price control device to help curb excessive consumption in a pandemic setting. We find the policy is remarkably effective and well-targeted in reducing demand for cheap alcohol, with minimal spillover effects, and consumers overall buying and spending less.

KEYWORDS

alcohol, excessive consumption, lockdown, minimum unit pricing, pandemic, price control

JEL CLASSIFICATION

C54, D04, H23, I12, I18, L81

1 | INTRODUCTION

Price controls can have good intentions but often poor outcomes in competitive markets (The Economist, 2020). Mandating minimum prices for alcohol may have the good intention of improving public health and reducing alcohol-related crime by reducing the affordability of cheap alcohol to curb high consumption. However, even if applying only to the prices of the very cheapest products in the market, which may be disproportionately linked with harmful consumption, the risk is that the whole market will be affected by price inflation, as higher prices cascade through to higher quality products associated with moderate or light consumption. The inflated prices could then be to the detriment of all consumers, not just those drinking cheap alcohol excessively, where softened competition boosts

Abbreviations: ABV, Alcohol By Volume; AIDS, Almost Ideal Demand System; AR(#), Autoregressive process; COVID-19, CoronaVirus Disease of 2019; DD, Difference-in-Differences; DDD, Difference-in-Difference-in-Differences; EPOS, Electronic-Point-Of-Sale; F-GLS, Feasible Generalized Least Squares; FMCG, Fast-Moving Consumer Goods; ILLS, Iterated Linear Least-Squares; MUP, Minimum Unit Price; PPU, Price Per Unit; SKU, Stock Keeping Unit; SUTVA, Stable Unit Treatment Value Assumption; VAT, Value-Added Tax; WHO, World Health Organization; U.K., United Kingdom.

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industry revenue but lowers all consumer surplus if alcohol demand is stubbornly inelastic (The Economist, 2018). Such an outcome would defeat the key argument for minimum prices, over and above taxation and sales restrictions, that they are highly targeted in only affecting cheap alcohol consumption.

This paper examines this prospect with novel analysis and findings on the extent to which minimum prices are targeted with minimal spillover effects. The context is at a time of heightened concerns about the affordability of cheap alcohol fueling problematic lockdown drinking during the COVID-19 pandemic (Anderson et al., 2020; Chalfin et al., 2021; Daly & Robinson, 2020; Finlay & Gilmore, 2020). Our focus is on minimum unit pricing (MUP), as a flat-rate form of minimum prices applying to all alcohol products based on their alcohol content, and we exploit policy differences amongst the four constituent countries of the United Kingdom, where two of the four countries have introduced MUP, to evaluate the efficacy of the policy. Centering on the introduction of MUP in Wales in March 2020, and with the need to disentangle the policy impact from surging demand for take-home alcohol during the pandemic lockdown period, we find that MUP is highly effective in reducing demand for cheap alcohol. We also find little demand spillover toward more expensive products, and consumers overall buying and spending less as desired, indicating that the policy is very well-targeted even in times of heightened demand.

Our approach is to focus primarily on comparing Wales with England (where MUP has been considered but not been implemented) for which we have weekly customer transaction data from the national leading retailer on alcohol purchases for 18 months before and 6 months after the introduction of MUP in Wales. We have two potential controls for a classic difference-in-differences (DD) study design, either contrasting treated products (originally priced below the price floor) with untreated products (priced above the price floor) or the treated country (Wales) with a control country (initially England) for matched products. However, both have complications because demand spillovers in the former would violate the stable unit treatment value assumption (SUTVA), while country lockdown demand differences in the latter would break parallel trends. The latter is a real prospect since the lockdowns closed the entire on-trade for consumption at licensed premises (accounting for 30% of total alcohol consumption), so the scale of the take-home sales uplift could substantially magnify different population responses to the lockdowns (which each country separately controls under devolved powers, with restrictions gradually eased in the last 2 months of our study period).

For the former, treatment is not binary as the price floor can alter two types of prices—regular prices and promotional prices—suggesting a three-way classification based on the amount of time the products were previously priced below the price floor: never below (untreated), intermittently below (mildly treated), or consistently below (severely treated). Crucially, we find very limited demand spillover to the untreated product group. Specifically, we find that demand spillover from bottom-tier to mid-tier may arise, but MUP curtails the retailer's ability to offer deep discounts (below the price floor) as a promotional tool for mid-tier products, and this seems to curb existing purchases rather than induce consumers to upscale to the top-tier. With this key feature, we use triple difference (difference-in-difference-in-differences (DDD)) analysis with temporal/country/group comparisons to isolate the price floor impact from differential country lockdown effects, while using other cross-country comparisons for validity testing.

We find overall that the effective price change due to the MUP regulation in Wales is an increase of 14.9% while the quantity decreased by between 17.7% (DD estimate) and 21.4% (DDD estimate). These effects are stronger than those found in the prior studies for Scotland, such as Griffith et al. (2022) finding that MUP increased prices by 5% while reducing alcohol volume by 11% on average. We also find stronger effects than those reported for Wales by Anderson et al. (2021) in their short-period study with household panel data covering just the first six months of 2020, finding a price increase of 8.2% and a quantity reduction of 8.6% due to the introduction of MUP.

Our contribution is threefold. First, we explicitly estimate the demand and substitution patterns between three groups of alcohol which are best viewed as vertically differentiated. To our knowledge, this has not been done in the context of imposing price floors for low-quality alcohol products. The substitution patterns help us better understand if increasing the price of the cheapest and lowest quality alcohol will have a knock-on effect on the price of others in equilibrium (e.g., as in Nash-Bertrand pricing models) or not. Thus, we first estimate own and cross-price elasticities using the non-linear Almost Ideal Demand System' (AIDS) demand model via instrumental variables. This exercise reveals that demand is very elastic for the cheapest group and that cross-elasticity at the group level is very low with the other groups. In fact, our estimates show that cross-price elasticity is zero between the highest and lowest quality alcoholic products. In turn, this helps us make reasonable assumptions about the lack of potential spillover effects and lends credibility to identification in our main analysis which uses DD and triple differences (DDD) methods.

Our second main contribution is replication. Earlier studies have shown minimum unit price was effective in curtailing demand for the lowest quality alcohol in Scotland. Our replication of the effect of the policy is for a different country, different dataset and data type, different methodology, and different context. The context differs in two

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important ways: (i) when the minimum unit price was implemented in Scotland, there was already in place a law that prohibited multi-buys like "two for one" or other similar multiple-unit price promotions and it is important to gauge the effectiveness of the policy without the other law; (ii) the minimum unit price was introduced during COVID-19 lockdowns when alcohol consumption at home started surging, and it is not clear ex-ante if the policy of minimum unit price will counter the surge in demand under such conditions. We find that the results of our replication are consistent with the earlier studies and lend support to the success of such a policy for curbing alcohol use.

Finally, our third (and perhaps relatively minor) contribution is methodological. difference-in-differences is a workhorse method for policy evaluations and is typically employed when there is a single shock to one of the units/groups. In our case, there were two simultaneous shocks. The introduction of the policy and the fact that there was a COVID-19 lockdown-induced surge in the demand for alcohol that could differ by product group and country. This is a complication for the analysis rather than a useful feature. Nonetheless, using data from multiple countries and multiple product groups, our work provides an example of precisely what type of assumptions are necessary to be able to identify the effects of a policy when one is introduced which coincides with other shocks that may be present at the same time.

The paper proceeds as follows. The next section sets out the policy context and our modeling approach as distinct from the extant literature. Section 3 details the data, methodology and econometric specification. Section 4 reports results of the DD and DDD analyses. Section 5 discusses the findings and the wider policy context as to whether there are any emerging indications that reduced alcohol consumption due to MUP as identified in our study may be linked to public health benefits or reduced alcohol-related crime. Online appendices provide additional analysis and testing details.

2 | POLICY CONTEXT AND MODELING APPROACH

Alcohol policy to counter harmful consumption generally relies on taxation along with restrictions on availability and marketing, but with varying success (Carpenter & Dobkin, 2017; Griffith et al., 2019; Hinnosaar, 2016; Kueng & Yakovlev, 2021; Marcus & Siedler, 2015). Such measures can help dampen general demand for alcohol and regulate consumption at licensed premises but may be less effective in restricting consumption at home when consumers can liberally buy heavily discounted cheap alcohol from supermarkets and other stores.[1] This policy shortcoming is magnified in a pandemic when lockdowns entail the suspension of more expensive on-premises consumption, resulting in more home consumption and heightened demand for cheap alcohol.

The concern about cheap alcohol is that the concentration of harm from excessive consumption, and so the bulk of negative externalities, rest with a small number of heavy drinkers who mostly buy cheaper and stronger varieties of alcoholic beverages than lighter drinkers (Griffith et al., 2019). Increasing excise taxes may seem the obvious way to ensure higher prices of such products, but vendors are under no obligation to pass on taxes fully or evenly, with evidence of tax under-shifting in highly competitive markets and for cheap alcohol (Ally et al., 2014; Hindriks & Serse, 2019; Wilson et al., 2021). Instead, mandating a price floor with a set minimum price provides an assured way to reduce the affordability of cheap alcohol, with the benefit of having limited impact on light drinkers buying more expensive alcohol above the price floor (Griffith et al., 2022). Moreover, if expressed in the form of minimum unit pricing then the price floor can be easy to apply with a simple and universal pricing formula based on alcohol content (Calcott, 2019).[2]

Scotland and Wales were amongst the first countries in the world to implement a blanket flat-rate minimum unit price (MUP) policy in 2018 and 2020, respectively.[3] Both countries used their devolved powers to introduce a common minimum unit price (MUP) at 50 pence per unit (50ppu), that is, £0.50 (roughly \$0.65 or €0.58) per alcohol unit (10 mL pure ethanol), applicable to all beverages. In contrast, England and Northern Ireland, as the other two constituent of the United Kingdom, have so far not adopted such a policy.[4] Apart this policy difference, there is a great deal of similarity in respect of the alcohol markets for these four countries where they all operate with the same U.K. taxation rates (both for excise duties and Value-Added Tax), with similar proportions of on-trade and off-trade sales and the preponderance of sales through the same set of nationally operating retail chains.[5]

These common market features, but with a difference in policy implementation across the countries, lend themselves to being exploited as a quasi-natural experiment for evaluating policy impact. Several studies have taken this approach in evaluating the impact of MUP, but mostly in respect of Scotland. These studies tend to fall into two types. The first and simplest type focuses on the overall impact on alcohol consumption, whether in aggregate or by broad product type (e.g., beer, wine, spirits, and cider), typically using electronic-point-of-sale (EPOS) scanner data from retailers. For example, Xhurxhi (2020) uses a DD approach based on annual data to find that MUP led to an average price increase of 4.4% and alcohol consumption decrease of 4.5%, with a greater impact on cider and beer sales than wine and spirits. Similarly, Robinson et al. (2021), using controlled interrupted time series analysis with weekly sales data, find that MUP led to a 3.5% reduction in off-trade sales per adult.

The second and more insightful study type focuses on household purchases using self-reported household panel data allowing for analysis of the policy impact on different types of households, categorized by their alcohol consumption (e.g., heavy, moderate, or light drinkers) and income levels. For example, both O'Donnell et al. (2019) and Anderson et al. (2021) use controlled interrupted time series analysis and obtain similar findings on the impact of MUP for Scotland, resulting in prices increasing by 7.9% and 7.6%, respectively, and in alcohol consumption reducing by 7.6% and 7.7%, respectively, and being well-targeted with high-consumption households reducing purchases the most. Griffith et al. (2022) use a more sophisticated DD approach and find that MUP increased prices by 5% while reducing alcohol volume by 11% on average, but with a 15% reduction for the highest consumption households while leaving the bottom 70% of drinkers unaffected.

Our approach and analysis differ from these studies in several notable respects. First, we focus on the impact of MUP in Wales, which has undertaken fewer prior alcohol policy interventions than in Scotland which might conflate policy impacts. For instance, alongside MUP, Scotland has a ban on multiple-unit discounts (multibuys) and in-store display restrictions on alcohol (Bokhari et al., 2023). It is conceivable that these policies could work in tandem to affect take-home alcohol sales and so isolating the effect of MUP is less straightforward.

Second, we study a more rapidly changing demand period than the stable demand period considered in evaluating the impact of MUP when introduced in Scotland in May 2018. Instead, Wales launched its MUP in March 2020, in the very month the World Health Organization declared COVID-19 a global pandemic and all four U.K. countries simultaneously commenced lockdowns. The timing was a coincidence for Wales but presents an opportunity to compare the effects from the introduction of MUP at the pandemic outset when problems with excessive consumption of cheap alcohol can be more acute, but with the modeling challenge of applying an empirical method that deals with the dynamic situation of surging demand.

Third, rather than examining the effects on product categories in aggregate or by different household consumption types, we examine the impact on product categories by their price tiers to look specifically at the extent of any spillover effects, where MUP has a direct impact on the prices and demand for cheap alcohol but potentially an indirect effect on the prices and demand for more expensive alcohol. Our approach thus provides further insights on how well targeted MUP is beyond the impact on different households.

Fourth, to facilitate this analysis on price tiers, we use a large matched-products sample as a balanced panel by utilizing customer transaction data on take-home alcohol sales from the market leading retailer in the U.K., as a different data source to existing studies based on EPOS or self-reported household purchase data.[6] Our data cover weekly retail sales over 2 years at individual item level from each of the four constituent countries. In total, we have a perfect match on over 2500 alcohol products sold in all four countries, allowing us to analyze in detail the impact of MUP and lockdown drinking on beers, ciders, spirits, wines, and other product types.

3 | DATA AND METHODS

We begin by outlining the data and variables, descriptive statistics, methodology and identifying assumptions, and the econometric specification.[7]

3.1 | Data and variables

Our data are drawn from records of alcohol sales from the market leading retail chain Tesco which provides information at the stock keeping unit (SKU) level by country and week. An SKU example would be "COORS LIGHT 6 × 330 ML". For each SKU, we obtained total expenditures and quantity sold (number of pack/bottles, etc.). The data series spans 104 weeks with 78 weeks before MUP implementation and the remaining 26 weeks starting the week of MUP implementation (March/2/2020). We supplemented this with information on alcohol by volume (ABV), measured as the percentage of ethanol for each SKU using Internet searches. This allowed us to compute weekly quantities, measured in standard "units" of alcohol (per 10 mL ethanol). Similarly, we obtained price per unit of alcohol for each SKU.

The MUP regulation specifies that no product should be sold at a price below 50ppu. However, prices change from week to week, and so a given product may be sold above the MUP threshold in one week, and below it in another week,

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but chiefly distinguished by regular prices as opposed to occasional or promotional prices below 50ppu. Accordingly, we classified all products into three groups based on cutoffs in the frequency of weeks each item was sold below the MUP price in Wales in the initial 78 weeks. These are:

- MUP1: never below 50ppu (0% weeks)—1576 items (share 28.6%);
- MUP2: intermittently below 50ppu (1%–79% weeks)—703 items (share 37.6%);
- MUP3: consistently below 50ppu (≥80% weeks)—276 items (share 33.8%).[8]

For the above classification, we used all 78 weeks rather than a smaller initial period since there is no reason to believe that retailers would start changing prices in anticipation before MUP implementation, and indeed our data confirm this. Importantly, we kept the same classification of the products in other U.K. constituent countries. To be clear, the classification of a product is based on its price in Wales and not in other countries, and hence if "COORS LIGHT 6 × 330 ML" is classified as MUP1 based on Welsh prices and frequencies, it retains the same classification in other countries. This allows us to be able to compare the before/after changes in prices and quantities for the same products. Our final sample consists of 2555 SKUs (see Appendix A for details on data cleaning). While there are relatively fewer items classified as MUP2 and MUP3, which are the direct target of the price floor, they in fact represent 71.4% of alcohol purchase by quantity.

We aggregated SKU level data to MUP/country/week level and created three different outcomes of interest in log form. These are ln(qnty), which is log of quantity in units (per 10 mL ethanol), ln(exp) which is the log of total expenditures on alcohol normalized by the total number of customers visiting the store, and ln(price) which is the log of price per unit of alcohol. The expenditure 'per customer' refers to the total number of customers visiting the retailer during the given week and country and not the unique number of persons purchasing alcohol in that category as the latter information is lost in aggregation (since the same person may be purchasing multiple items).

3.2 | Descriptive statistics

Table 1 gives descriptive statistics by country, product groups, and before/after periods for the main outcome measures (for an equivalent table in levels, see Table A-2 in the appendix). In percentage terms, mean alcohol quantity for MUP3 products decreased in Wales by 34.9% (=100(exp(14.19 − 14.62) − 1)), while it increased for both MUP2 and MUP1 products by 18.5% and 19.7%, respectively. By comparison, the mean quantity increased for all three product groups in England. Expenditures (normalized by total customers visiting the store) increased for all three MUP categories in both countries, albeit least for MUP3 in Wales. This increase in expenditures is driven primarily by the decline in the absolute number of customers for the retailer in all countries during the pandemic, reflecting less shopping around and more one-stop shopping (see Figure A-3). The largest and most obvious change was in the price of MUP3 products in Wales where the mean increased by 30.3%, followed by a smaller increase in the prices of MUP2 products (up 7.8%) with very little change in the prices of MUP1 products. There was also a small increase in prices in England but not as large as observed in Wales (e.g., up 2.9% for MUP3 products). To the best of our knowledge, the regulation does not allow the final price to be below 50 ppu even due to occasional promotions.[9] In our aggregate data series, the observed minimum is 50 ppu in Wales post the MUP regulation for MUP3 products. See Table A-3 for all the minimum prices.

To highlight these changes, we plot the values of log quantity and log price overtime in Figure 1 (and a similar comparison for England and Scotland is shown in Figure A-4 in Appendix A). The left panel shows the trends of log quantity for each of the MUP categories, where they have been demeaned over their own pre-ban averages.[10] Until March/2/2020, the quantity lines were remarkably parallel. Importantly, in the top left panel, we can see a deviation in the paths following MUP implementation in Wales, particularly for MUP3 products (consistently sold below 50ppu). There is also a small decline in quantity in England a little later on after the lockdowns. This may have been due to the easing of initial lockdowns and reopening of the on-trade.

The right hand side panel in Figure 1 shows trends in log prices. The sharp rise in the prices of MUP3 products in Wales is in contrast to the stable trend of MUP3 products observed for England (top right panel). Similarly, there is a clear deviation in time paths for the prices of MUP2 products after MUP implementation in Wales. In contrast, the prices of MUP1 products follow similar paths in the two countries even after MUP implementation. Moreover, it appears that MUP1 products in both prices and quantities do not seem to be affected much by the MUP implementation, lending some credence to the notion that the spillover effect to MUP1 products is either not present or is very small. Finally, note that

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TABLE 1 Statistics by country, minimum unit price (MUP) group and before/after implementation.

	MUP3 group				MUP2 group				MUP1 group			
	Before		After		Before		After		Before		After	
	Mean	Std	Mean	Std	Mean	Std	Mean	Std	Mean	Std	Mean	Std
Wales												
ln(qnty)	14.62	0.182	14.19	0.091	14.68	0.315	14.85	0.097	14.38	0.390	14.56	0.163
ln(exp)	0.916	0.083	0.994	0.132	1.145	0.183	1.528	0.150	1.205	0.268	1.542	0.159
ln(price)	−0.954	0.037	−0.689	0.002	−0.672	0.044	−0.597	0.009	−0.288	0.028	−0.281	0.014
England												
ln(qnty	16.98	0.155	17.10	0.099	17.19	0.296	17.31	0.131	16.94	0.372	17.07	0.154
ln(exp)	0.817	0.069	1.063	0.119	1.111	0.172	1.379	0.146	1.211	0.260	1.486	0.146
ln(price)	−0.925	0.028	−0.897	0.012	−0.668	0.042	−0.654	0.027	−0.279	0.028	−0.273	0.021

Note: Means and standard deviations are over the first 78 weeks (before) and following 26 weeks (after). Table A-2 in Appendix A-2 provides the numbers in levels rather than in logs.

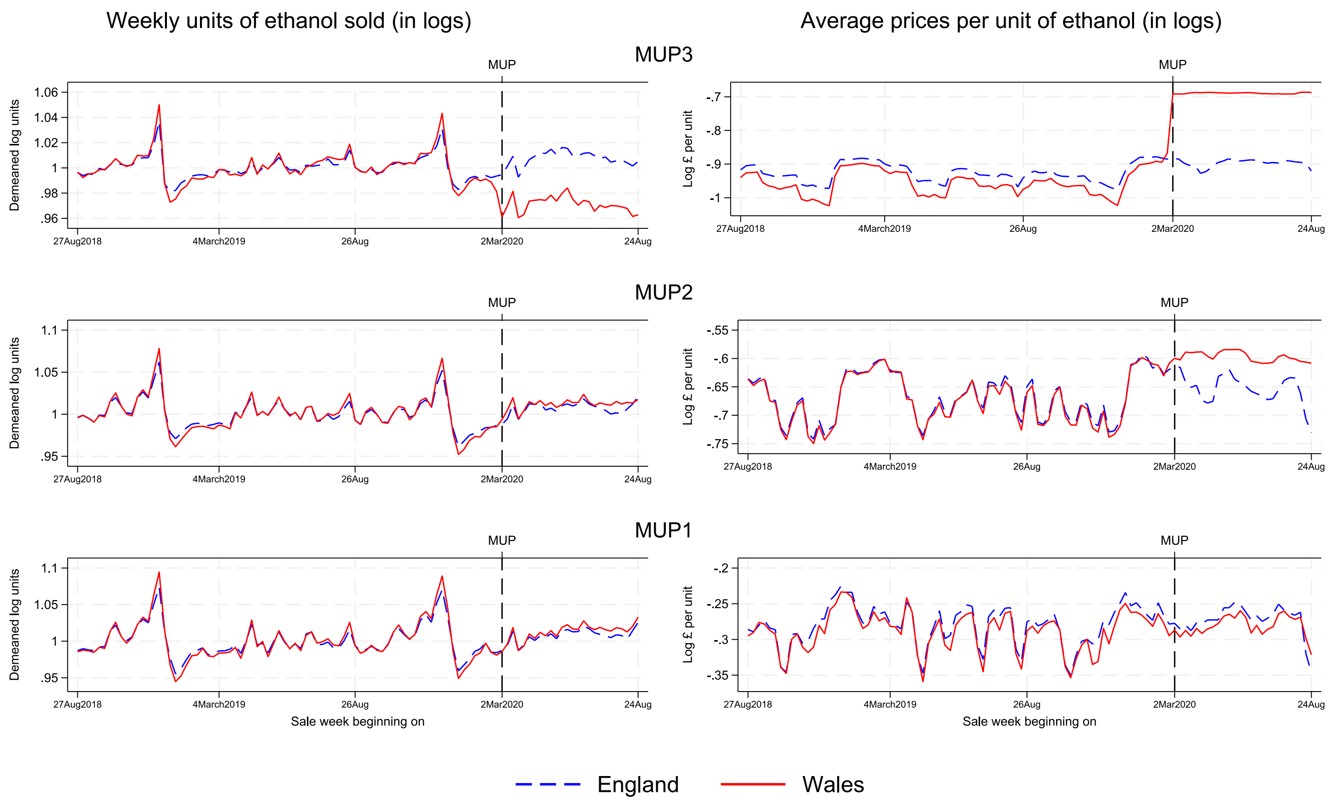

FIGURE 1 Average quantity and price per week by MUP group and country. MUP, minimum unit price.

quantity paths changed for products sold in England too, presumably due to the lockdown, and especially the price of MUP2 products decreased significantly in England during the lockdown but in keeping with the seasonal pattern.

3.3 | Methodology (identifying assumptions)

A standard method of identifying the impact of policy change is to use a DD design. Providing a 'parallel trend' assumption holds in the pre-policy period, we can then compare change on the outcome of the products affected by the

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MUP policy with the before/after change observed in a control group not affected by the policy (and we test for this in Section 4.4). Two types of control groups are available: same products in another country, or an unaffected set of products in the same country.

For either of the two types of controls, the treatment groups are MUP3 (affecting regular prices) and MUP2 (affecting occasional/promotional prices), while the regulation has no direct impact on MUP1 products.

For DD comparisons within Wales, we allow MUP1 products to form the control group. However, spillover effects are an evident possibility, as MUP1 products are potential substitutes for MUP2 and MUP3 products. For instance, with price competition and upward sloping best-response curves (strategic complementarity) amongst vertically (quality) differentiated products, the raised prices for treated products (previously below the price floor) could prompt an increase in the price of the control group prices as well, and that in turn would affect their demand. Even if prices of non-treated products do not change, the relative change in price for one set of products can affect the demand for products in the control group, unless the cross-price elasticity is almost zero. Thus, we could violate SUTVA.

Alternatively, we can obtain DD estimates by comparing before/after sales of products directly affected by the price floor in Wales with the before/after sales of the same products in England. Since England (neighboring Wales) did not apply MUP, the same products from England may provide a better control as they would not violate the above-mentioned SUTVA. The condition holds as long as there is no (strong) linkage in retail prices across the two countries and there is no significant cross-border purchases of alcohol, that is, if the markets are segmented at national level. A discussion on the relevance of extensive cross-border shopping activities in our context is given in Appendix D. Based on previous findings and our analysis on administrative traffic data, we do not think cross-border shopping would have a significant impact on our estimates over our window of observation.

Barring any cross-border purchase issues, the second DD estimator sounds more plausible. However, a potential problem is that the U.K.-wide lockdown was initiated almost immediately following MUP implementation in Wales. So while we expect consumption to decrease due to the price floor introduction, this is countered by evidence of a strong surge in retail store alcohol purchases during the lockdown (Anderson et al., 2020). Furthermore, we do not know if the lockdown-effect outcomes were the same across the countries. The cross-country DD estimator would not be valid if the effects of lockdown-induced sales on the treated products differ by countries, as it would imply that the sales in the two countries would follow different trajectories even if there were no policy change in minimum unit price. This could happen, for instance, if the lockdown induces a 30% increase in sales of MUP3 products in Wales but a 50% increase in England. In fact, even the first DD estimator above (i.e., a within-Wales cross-product DD estimator) would not be valid even if there were no spillover effects if the lockdown induced different trajectories for control (MUP1) and treated products (MUP2 and MUP3).

To account for the possibility of differential country lockdown effects, we use a triple difference (DDD) estimator to identify the effect of MUP implementation on the MUP2 and MUP3 product groups. Essentially, from the cross-country DD estimator for MUP2 and MUP3, we can subtract the cross-country DD estimator for MUP1, which would give us a DDD estimate. This DDD approach is valid under the alternative assumption that the effect of the lockdown on MUP1 versus the treated products is the same across countries. Under this setup, the identifying assumption is as follows. Suppose the lockdown effect on sales in Wales on MUP1 and MUP3 products is 10% and 30% respectively, so that the differential effect in Wales is 20%. Suppose further that the lockdown effect in England on MUP1 and MUP3 products is 30% and 50% respectively. Thus, the differential effect of lockdown between the treated and untreated products is again 20%, that is, it is the same across the two countries (and we thank an anonymous referee for this example). If this assumption holds, we can isolate the effect on outcomes of the price floor regulation from the effect induced by the country lockdowns. In a later section, we provide some corroborating evidence for such an assumption.

The intuition behind the DDD estimator is as follows. If there are no country differentials arising from the lockdowns and only within-country spillover effects, then the DD estimators by product groups across countries are sufficient for identification. If, though, there is no spillover effect on to MUP1 products, but there is a differential country lockdown effect that is the same the same across product groups (as given in the example above), then subtracting the DD estimator for MUP1 products from the DD estimators for MUP2 and MUP3 products would identify the net impact of the policy on affected products. Moreover, under these assumptions, the DD estimate for MUP1 captures the country lockdown differential between Wales and England for the control group. Appendix A.5 illustrates the relationship between the DD and the DDD estimators using only mean values for total quantity before/after by country and MUP groups.

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3.4 | Experical specification

Both the DD and the DDD estimators can be obtained from a single reduced form regression that controls for seasonality and time trends. Let Y be the outcome variable of interest. Then we can estimate

$$\begin{aligned} Y_{it} = &\alpha_1 + \alpha_2 X_{it} + \beta_1 W_{it} + \beta_2 P_{it} + \beta_3 M2_{it} + \beta_4 M3_{it} \\ &+ \beta_5(W_{it}P_{it}) + \beta_6(W_{it}M2_{it}) + \beta_7(W_{it}M3_{it}) + \beta_8(P_{it}M2_{it}) + \beta_9(P_{it}M3_{it}) \\ &+ \beta_{10}(W_{it}P_{it}M2_{it}) + \beta_{11}(W_{it}P_{it}M3_{it}) + u_{it}. \end{aligned} \tag{1}$$

In the equation above, P, W, $M2$ and $M3$ are 1/0 indicator variables equal to 1 if the observation refers to a post-MUP legislation period ($t \geq 79$), from Wales, for MUP2, or MUP3 category products, respectively. The variable X consists of time trends and seasonal dummies, a polynomial in time up to power four and dummies for each calendar month. As a robustness check, we also allow the polynomial and monthly fixed effects to be country-specific by including additional interactions terms (see Section 4.4). The DDD estimates of the effect of legislation for MUP2 and MUP3 products are β_{10} and β_{11} respectively, and if by assumption there is no spillover effect on MUP1, then the effect of the MUP enforcement is zero on these products (as mentioned above, the identifying assumption requires that the differential effect of the lockdown across the treated and control groups is the same across countries). The coefficient β_5 in this case represents the differential country lockdown effect on MUP1 products. Under the alternative (stronger) assumption that the lockdown effect is the same for a given product group across countries, but allowing for policy spillover effects on MUP1 products, we can obtain the DD estimates as β_5, $\beta_{10} + \beta_5$ and $\beta_{11} + \beta_5$ for MUP1, MUP2 and MUP3 products, respectively.

For any given outcome variable Y, Equation (1) is essentially a comparison of six time series, consisting of three MUP categories in two countries each. Equivalently, it is a long panel of 104 weeks with six cross-sectional units, and hence the usual panel methods that rely on a large number of cross sections for asymptotic inference are not appropriate here. Given the scale difference across the MUP categories and countries, our imposed error structure allows for country-MUP-group level heteroskedasticity. We also allow for errors across the six series to be contemporaneously correlated and hence we specify a heteroskedastic error structure with cross-sectional correlation. Inspection of residuals from initial pooled ordinary least squares estimates indicates that error terms are correlated over time, and each time series exhibits an autoregressive process of order one ($u_{it} = \rho_i u_{i,t-1} + \epsilon_{it}$) (see Figure B-1 in Appendix B). Consequently, we estimate the models via Feasible-GLS allowing additionally for separate AR(1) process for each of the time series (see Greene, 2003, p. 320).

3.5 | Demand specification

We can check if the SUTVA condition described earlier is violated by estimating cross-price elasticities between the three product groups. In turn that gives us an estimate of the extent of any spillovers across the product groups that are not the direct target of this policy. Importantly, they also tell us if our DD or DDD estimates are likely to be informative about the causal effects of the policy or not. The additional bonus is that we also obtain own-price elasticities for each group of products and can see the extent of vertical differentiation between them.

Since we aggregate the data from individual items to the three groups, a natural starting point is to estimate demand using Deaton and Muellbauer's (1980) AIDS while recognizing that price is likely to be endogenous and account for it in the estimation. Thus, we estimate the revenue based share equations s_{jt} of the jth MUP-group class in period t as

$$s_{jt} = \alpha_j + \sum_{k}^{3} \gamma_{jk} \ln p_{kt} + \beta_j \ln(y/P)_t + \tau_j X'_{jt} + \mu_{jt} \tag{2}$$

where p_{kt} is the price of the kth class in that week, y is the total expenditure on all three classes of alcohol, P is the overall alcohol price index and X'_{jt} is a vector of other controls that may or may not be specific to the MUP group j. In our case these include country dummies, month dummies, and a polynomial of degree three in time (where time is measured in weeks). The μ_{jt} is the MUP specific error term. The remaining Greek symbols are the parameters of the model to be estimated and where elasticities are computed using estimates of β_j and γ_{jk} at some selected values of the shares (typically mean values or mean of all values). The price index is also a function of the same parameters and is given by

$$\ln P_t = \alpha_0 + \sum_{k}^{3} \alpha_k \ln p_{kt} + \frac{1}{2} \sum_{i} \sum_{k} \gamma_{ki} \ln p_{kt} \ln p_{it}. \tag{3}$$

The model is estimated as a system of share equations and the price index but can be either linear, if the price index is first approximated with a linear version, such as the Stone price index, or is estimated non-linearly. We use the iterated linear least-squares estimator due to Blundell and Robin (1999) which is a conditionally linear estimator. See also Lecocq and Robin (2015). As is typical in such models, we also impose parameter restrictions to make the estimated equations consistent with consumer theory (i.e., shares should add to one, the Slutsky symmetry, homogeneity of degree zero). Thus, we impose $\sum_{j=1}^{J} \alpha_j = 1, \sum_{j=1}^{J} \gamma_{jk} = 0, \sum_{j=1}^{J} \beta_j = 0, \sum_{k} \gamma_{jk} = 0$ and $\gamma_{jk} = \gamma_{kj}$.

Prices are likely to be correlated with the error terms and so we use two sets of instruments. The first set is variables that may directly affect retail prices for alcohol and include weekly exchange rates between the pound sterling and the US dollar and the euro. Additionally, we also used the average price of diesel as that too can affect delivery costs and hence retail prices. These types of instruments have been used successfully elsewhere as well (e.g., Griffith et al. (2019) and Bokhari et al. (2023)). We also experimented with using ex-factory prices for ciders and fruit wines and beers but they generally did not perform well in terms of first-stage statistics so we did not use them in the final estimates. The second set of price instruments is due to Hausman et al. (1994) where the price of the same product from another geographic region is an instrument. The identifying assumption is that the prices from different areas are correlated via common cost shocks but there are no common demand side shocks. We implemented a version of these instruments by estimating the weekly mean price of MUP group j from the three other countries of the U.K. for the reference country i. The performance of these instruments is further discussed in Appendix C.1.

4 | RESULTS

We start in Section 4.1 with the results from the demand estimation and focus on own- and cross-price elasticities to understand the likely impact of price changes on own demand for MUP3/MUP2 products and the substitution patterns between the groups of alcohol products. We then discuss the main results from DDD/DD analysis by comparing Wales and England in Section 4.2. Section 4.3 provides tests which i) uses data from England and Northern Ireland as a falsification, and ii) tests the presence of differentiated lockdown effects within MUP1 products only. Next, we describe the results of further robustness checks and parallel trend assumptions in Section 4.4. Following that, Section 4.5 summarizes the main findings from the analysis by product categories (beers, ciders, spirits, and wines), with full details confined to the appendices. Finally, Section 4.6 shows the results of comparisons between England and Scotland, where minimum unit pricing had already been in operation since May 2018.

4.1 | Elasticities

Table 2 provides uncompensated (Marshallian) elasticities computed at the sample mean as well as income/budget elasticities and the mean expenditure shares. The demand parameters, first-stage statistics and compensated (Hicksian) elasticities are given in Appendix C.1 (the first-stage F-stats were all above 20). Two things stand out very clearly from these elasticity estimates. First, MUP3 products have the most elastic demand and hence a policy targeting the price of these products to reduce the quantity is likely to be most effective.[11] While MUP3 products constitute 33.8% by volume (reported earlier), they are 21.0% share of the expenditure on alcohol. On the flip side, the income or budget elasticity of MUP3 products in the lowest at 0.695 of the three groups and that of MUP1 is the largest at 1.204.

Second, the cross-price elasticity between MUP1 and MUP3 is not statistically different from zero, indicating that there is likely very little spillover effect of a price increase in the MUP3 products due to the MUP policy on the quantity consumed of MUP1 products. There is, though, likely to be some spillover effect on to the quantity consumed of MUP2 products, which seem to be a buffer between the very cheap MUP3 alcohol and the higher priced MUP1 alcohol.

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TABLE 2 Marshallian price (η_{jk}) and budget (θ_j) elasticities and shares (s_j).

η_{jk}	p_3	p_2	p_1	θ_j	s_j
q_3	-2.538^a	1.606^a	0.237	0.695^a	0.210
(MUP3)	(0.102)	(0.263)	(0.195)	(0.104)	(0.044)
q_2	0.818^a	-2.296^a	0.526^a	0.951^a	0.386
(MUP2)	(0.065)	(0.166)	(0.123)	(0.009)	(0.032)
q_1	0.016	0.403^b	-1.622^a	1.204^a	0.404
(MUP1)	(0.075)	(0.192)	(0.142)	(0.010)	(0.044)

Note: Uncompensated/Marshallian elasticities given by η_{jk} where j, k entry is the elasticity of product j with respect to the price of product k. Standard errors are in parenthesis. Superscripts a, b, c indicate significance at 1%, 5% and 10%, respectively. Budget/income elasticity given under column θ_j and the last column provides the mean expenditure shares and standard deviations of the shares. Full set of regression coefficients are given in Table C-3.

4.2 | Minimum unit price in Wales

Table 3 provides estimates of selected coefficients β_5, β_{10} and β_{11}, as well as of their sums $\beta_5 + \beta_{10}$ and $\beta_5 + \beta_{11}$ for all outcome measures (See Table C-4 for the full set of regression coefficients). Additionally, for both DDD and DD estimates, the table provides the overall combined effect on MUP2 and MUP3 alcohol products and all alcohol products (MUP1, 2 and 3) combined.

Starting with ln(qnty) (log of total quantity in alcohol units) and MUP3 products (consistently <50ppu), quantity sold in Wales declined by 44.4% due to the price floor (computed as $100(\exp(\beta_j) - 1)$. Further, there was no significant change in quantity of MUP2 products (intermittently <50ppu). This is somewhat surprising given our earlier demand estimation which points to a positive and significant cross-price elasticity between the MUP3 and MUP2 products. Note that these are both DDD estimates, which assumes that there is no spillover effect on MUP1 products (never <50ppu). Therefore, β_5, the coefficient for MUP1 products, measures the lockdown country differential between England and Wales that is not attributable to the MUP regulation, indicating 4.6% higher quantity. The combined effect on MUP2 and MUP3 products is an overall net reduction of 21.4% in quantity sold.[12] Under the alternative assumption of a lockdown country differential present between England and Wales, the β_5 estimate represents the DD estimate with a 4.6% increase in MUP1 products due to the spillover effects. Under this assumption, the effect of MUP implementation on MUP3 and MUP2 products are −41.8% and +5.0% respectively, and the combined effect on all three product groups is a decrease of 17.7%.

In terms of expenditure per customer, the DDD estimates indicate a reduction for MUP3 products of 19.6% (where per customer is the total number of customers for this retailer, and not the total number of customers that purchased alcohol products). This reduction in expenditures is driven by a 25.2% increase in the price of MUP3 products. Similarly, for MUP2 products, the increase in expenditure is also driven by 5.7% increase in price (see column 3) as quantity purchased did not change. The assumption of no spillover effect on MUP1 products is corroborated by no change in the price of MUP1 products ($\beta_{11} = 0.002$ and not significant at conventional levels). Overall, the combined effect of the introduction of MUP in Wales is a 14.9% increase in prices and a 6.3% decline in expenditure per customer.

Under the alternative assumption of spillover effects on MUP1 products, the DD estimator shows that expenditure per customer decreased for MUP3 products ($\beta_5 + \beta_{11} = -0.158$) and increased for MUP2 products ($\beta_5 + \beta_{10} = 0.112$) and the combined effect on these two products is −0.0648. Finally, the overall effect on expenditure on all the products combined is much more modest at −0.4% with an overall increase in price of 14.9%.

Estimates obtained under the alternative error structure assumptions, that is, that errors are independent across the six time series lead to similar conclusions (see Table B-1 in Appendix B).

In the foregoing discussion, the two sets of estimators give bounds to the effect of the price floor under alternative assumptions, and differ by the estimate of β_5, which could be positive or negative. Just to be clear, under the DDD estimation, β_5 measures size of the country lockdown differential, while under the DD estimation it is interpreted as the size of the spillover effect with no country lockdown differentials. Thus if β_5 is significantly different from zero, it is due to (1) a spillover effect on MUP1 products in Wales, and/or (2) a country lockdown differential (or both). We next explore these issues.

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TABLE 3 Effect of minimum unit price (MUP) implementation in Wales.

	ln(qnty)	ln(exp)	ln(price)
DDD estimates (no spillovers)			
MUP3: β_{11}	−0.587[a]	−0.218[a]	0.225[a]
	(0.014)	(0.010)	(0.005)
MUP2: β_{10}	0.00381	0.0531[a]	0.0553[a]
	(0.011)	(0.007)	(0.005)
MUP1: β_5	0.0453[a]	0.0592[a]	0.00200
	(0.012)	(0.008)	(0.003)
DD estimates			
MUP3: $\beta_5 + \beta_{11}$	−0.542[a]	−0.158[a]	0.227[a]
	(0.013)	(0.009)	(0.005)
MUP2: $\beta_5 + \beta_{10}$	0.0491[a]	0.112[a]	0.0573[a]
	(0.012)	(0.006)	(0.004)
Combined effects			
Combined (DDD)	−0.241[a]	−0.0648[a]	0.139[a]
(MUP2 and 3)	(0.009)	(0.006)	(0.005)
Combined (DD)	−0.195[a]	−0.00449	0.139[a]
(MUP1,2 and 3)	(0.009)	(0.007)	(0.005)
Observations	624	624	624
chi2	347,661.8	2511.0	34,781.6
df	26	26	26

Note: ln(qnty) is log of quantity (in units of alcohol), ln(exp) is log of expenditure per customer and ln(price) is log of price per unit. F-GLS estimates with separate AR(1) process for each country-MUP group, and correlation across groups (robust S.E. in parenthesis). Superscripts *a*, *b*, *c* indicate significance at 1%, 5% and 10%, respectively. Only selected coefficients shown. Full set of coefficients are given in Table C-4. The #obs, χ^2, and df are reported for the main regressions and not the auxiliary regressions used for computing the combined effects.

4.3 | Falsification and differential lockdown effects tests

We undertake two analyses in this section. First, we re-estimate the model above using data from England and Northern Ireland. Second, we check if the lockdown affected subgroups of the control MUP1 group equally. The rationale for these tests and the results are discussed below.

4.3.1 | England versus Northern Ireland

We use data from England and Northern Ireland—both countries without minimum pricing. This eliminates the possibility of any spillover effects. Thus, if the England/Northern Ireland before/after differential for MUP1 is similar to one observed for Wales/England, it indicates the presence of country lockdown differentials, which, in turn, supports the use of DDD estimator. If, though, this differential does not exist between England and Northern Ireland then it suggests that lockdown effects are similar across countries, and we can use the DD estimates from the initial model. Note also that had there been no lockdown, comparison of England and Northern Ireland data would give coefficients of interest that would not be significantly different from zero, as there is no MUP regulation in either country. Given the lockdown, however, these coefficients can be different from zero, but should be significantly smaller in magnitude compared to those from the England/Wales comparison.

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TABLE 4 Northern Ireland versus England.

	ln(qnty)	ln(exp)	ln(price)
DDD estimates (no spillovers)			
MUP3: β_{11}	-0.0811^{a}	-0.0593^{a}	−0.00976
	(0.027)	(0.018)	(0.006)
MUP2: β_{10}	-0.0882^{a}	-0.0569^{a}	−0.00689
	(0.028)	(0.020)	(0.008)
MUP1: β_5	0.0369^{c}	0.0289^{c}	0.0199^{a}
	(0.022)	(0.016)	(0.005)
DD estimates			
MUP3: $\beta_5 + \beta_{11}$	-0.0442^{c}	-0.0304^{c}	0.0101^{a}
	(0.025)	(0.016)	(0.003)
MUP2: $\beta_5 + \beta_{10}$	-0.0513^{b}	−0.0280	0.0130
	(0.024)	(0.017)	(0.008)
Combined effects			
Combined (DDD)	-0.0906^{a}	-0.0739^{a}	-0.00918^{c}
(MUP2 and 3)	(0.020)	(0.014)	(0.005)
Combined (DD)	-0.0538^{a}	-0.0453^{a}	0.0104^{a}
(MUP1,2 and 3)	(0.015)	(0.013)	(0.003)
Observations	624	624	624
chi2	167,482.1	1830.6	23,229.2
df	26	26	26

Note: ln(qnty) is log of quantity (in units of alcohol), ln(exp) is log of expenditure per customer and ln(price) is log of price per unit. F-GLS estimates with separate AR(1) process for each country-MUP group, and correlation across groups (robust S.E. in parenthesis). Superscripts *a*, *b*, *c* indicate significance at 1%, 5% and 10%, respectively. Only selected coefficients shown. The #obs, χ^2, and df are reported for the main regressions and not the auxiliary regressions used for computing the combined effects.

Table 4 shows selected coefficients in DDD estimation as before but using data from England and Norther Ireland. The DDD coefficient for log quantity for MUP1 is 0.0369, statistically significant, and in fact of similar magnitude as that for Wales versus England (0.0453). The coefficients for MUP2 and MUP3 are also statistically significant but considerably different in magnitude from those obtained in the Wales versus England case: the DDD and DD estimates for MUP3 are now −0.081 and −0.044, and sharply compare to previous estimates of −0.587 and −0.542. These large differences for MUP3 coefficients relative to the previous case provide a falsification test of our comparison between Wales and England: if the estimated effect on MUP3 products was not due to legislation, we would have expected a similar sized differential between England and Northern Ireland. Similarly, the combined effect on MUP2 and MUP3, or of all three combined, is in the range of 5.2% (DD) and 8.7% (DDD), and much smaller than those obtained from the Wales/England case, which were 21.4% (DD) and 17.7% (DDD). These results lend support for the country differential hypothesis, and preference for DDD estimates.

Nonetheless, when ignoring cross-series correlations, the Northern Ireland and England differences are no longer statistically different (see Table B-2 in Appendix B), and provide support for a DD estimate. We therefore consider the DDD and DD estimates as the lower and upper bounds for the net impact of the MUP introduction in Wales under alternative assumptions.

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TABLE 5 MUP1 products only—England versus Wales.

	ln(qnty)	ln(exp)	ln(price)
DDD estimates (no spillovers)			
MUP1a: θ_6	0.0464[a]	0.0555[a]	0.00619
$(\beta_{1A} = \theta_6)$	(0.012)	(0.008)	(0.004)
MUP1b: θ_7	−0.0132	−0.0284[b]	−0.0108
$(\beta_{1B} = \theta_7 + \beta_{1A})$	(0.025)	(0.012)	(0.009)
Observations	416	416	416
chi2	285,783.1	1602.6	4966.0
df	22	22	22

Note: ln(qnty) is log of quantity (in units of alcohol), ln(exp) is log of expenditure per customer and ln(price) is log of price per unit. F-GLS estimates with separate AR(1) process for each country-MUP group, and correlation across groups (robust S.E. in parenthesis). Superscripts *a*, *b*, *c* indicate significance at 1%, 5% and 10%, respectively. Only selected coefficients shown.

4.3.2 | Differential effects of lockdown

Next, we test if our assumption for DDD might hold; that is, the differential effect of the lockdown across the treated and control product groups is the same across Wales and England. We cannot directly check this assumption. Instead, we check if the lockdown affected Wales and England similarly in other dimensions. To this end, we divide MUP1 products into two groups, MUP1A and MUP1B depending on if they are below or above the median price for this group in Wales. Next, for each of these subgroups, we can estimate a DD model comparing England/Wales and before/after. If the DD estimates are β_{1A} and β_{1B}, we can test if these are equal or not. If they are, it shows that the lockdown effect is the same in two subgroups, and lends indirect support to our assumption above. The test is most directly implemented via a three-way interaction term in joint estimation for MUP1A and MUP1B products. Specifically, we estimate

$$\begin{aligned} Y_{it} = {} & \gamma_1 + \gamma_2 X_{it} + \theta_1 W_{it} + \theta_2 P_{it} + \theta_3 M1_{Bit} + \theta_4 (W_{it} M1_{Bit}) + \theta_5 (P_{it} M1_{Bit}) \\ & + \theta_6 (W_{it} P_{it}) + \theta_7 (W_{it} P_{it} M1_{Bit}) + u_{it}, \end{aligned} \quad (4)$$

where $M1_{Bit}$ is a 1/0 dummy variable equal to one if the observation refers to MUP1B product, $\beta_{1A} = \theta_6$ and $\beta_{1B} = \theta_6 + \theta_7$, and the equality is tested by significance of θ_7.

Table 5 provides estimates of the triple interaction term as described above. The first row of the table shows the effect on MUP1A products. Note that these coefficients are very similar to MUP1 coefficients listed as β_5 in Table 3. The second row for θ_7 tests if $\beta_{1A} = \beta_{1B}$. In two of the three cases we do not reject the null which indicates that England/ Wales before/after DD estimate for MUP1A products is same as that for MUP1B products. In turn, this lends support to our identification assumption for DDD estimates provided earlier.

4.4 | Robustness and parallel trends

We already noted earlier that our results in Table 3 and Table 4 hold when we allow for independence of errors across the six time series (which are given in Tables B-1 and B-2 in Appendix B). So far our specifications are parsimonious and account for common time trends (a polynomial in time to the power four) and common monthly fixed effects. Thus, we next check if the forgoing analysis holds up if allow for country-specific time trends and country-specific monthly fixed effects via additional interaction terms included in the main specification. The results are given in Table B-3 and Table B-4 and are qualitatively similar but show slightly larger magnitudes for MUP1 products.

Critical to our identification setup is the 'parallel trends' assumption. While Figure 1 shows that the parallel trends assumption holds for log quantity and log price in the pre-MUP period, we conduct two closely related formal tests for parallel trends, with results reported for all outcome variables in Appendix B.4. The first test uses data from the pre-MUP period ($T \leq 78$) to estimate a variant of Equation (1) where the indicator variable P_{it} for pre-post period is

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TABLE 6 Scotland versus England (difference-in-differences (DD) Estimates).

	ln(qnty)	ln(exp)	ln(price)
MUP1: β_5	−0.0134	−0.0179	−0.0108
	(0.047)	(0.032)	(0.011)
MUP3: $\beta_5 + \beta_{11}$	-0.100^{a}	-0.0896^{a}	−0.00831
	(0.025)	(0.015)	(0.010)
MUP2: $\beta_5 + \beta_{10}$	−0.0533	-0.0380^{c}	−0.00198
	(0.037)	(0.019)	(0.014)
Combined effects			
Combined (DD)	-0.0646^{a}	-0.0676^{a}	−0.00737
(MUP1,2 and 3)	(0.023)	(0.016)	(0.009)

Note: ln(qnty) is log of quantity (in units of alcohol), ln(exp) is log of expenditure per customer and ln(price) is log of price per unit. F-GLS estimates with separate AR(1) process for each country-MUP group, group level heteroscedasticity and correlation across groups (robust se in parenthesis). Superscripts *a*, *b*, *c* indicate significance at 1%, 5% and 10%, respectively. Only selected regression coefficients shown.

replaced by a linear time-trend. We then test for the joint significance of β_5, β_{10} and β_{11} and do not reject the null that these parameters are equal to zero (see Table B-5). This supports the hypothesis of parallel trends in the pre-MUP period. In a second test, we run a placebo test. We again use only the pre-MUP period data but this time keep P_{it} as an indicator variable which is set to one after $T/2$. We then test for the significance levels of key parameters, and in all but one case, they are not statistically significant.[13] The test results are given in Table B-6.

4.5 | Difference-in-difference-in-differences estimates by product categories

We also estimated the impact of the MUP regulation in Wales by segmenting alcohol products by the following types: (i) Beers (includes ales, stouts and lagers); (ii) Ciders (including perries); (iii) Spirits; and (iv) Wines (including all wine-based drinks). Estimation details and results are described in Appendix ?? Briefly, they show a large reduction in quantity sold of MUP3 products for beers and ciders but with considerably less reduction for wines.

4.6 | Lockdown impact for existing minimum unit price regulation

We next compare the effect of lockdown drinking across Scotland and England, where the former already had its price floor in place before the beginning of our time series, while the latter has no price floor. For this analysis, we employed a DD estimator as there are no changes in MUP policy in either country during the study period. We kept the same MUP classification (based on prices observed in England) even though there were no products priced under the MUP floor in Scotland.

The movement over time in (log) quantity and prices is shown in Appendix A.4. Mean (log) quantity increased in England for all three MUP categories after week 79, by 13.7%, 12.7% and 13.2% for MUP1, MUP2 and MUP3 products, respectively. In contrast, the corresponding increases in quantity in Scotland were 15.8%, 7.6% and 3.5%. Prices also increased in England slightly for all three categories (0.7%, 1.5% and 2.9% for MUP1, MUP2 and MUP3, respectively) while they showed no systematic pattern in Scotland (decreasing by 1.1% for MUP1 products and increasing slightly for MUP2 and MUP3 products by 0.3% and 0.5%, respectively).

We estimated a −9.5% difference in MUP3 products' sales in Scotland compared to England, and the effect is statistically significant. See Table 6. The −5.2% difference estimated for MUP2 products is not statistically significant. Similarly, no significant difference in sales of MUP1 products were found between these two countries. The estimated

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net combined reduction for all products sold in Scotland was −6.3% and was statistically significant. Similarly, there is a large unadjusted increase in expenditure per customer across MUP categories in both countries post lockdown commencing. This is because of fewer customers with shoppers reducing store visits (see Figure A-3). However, the DD estimates show that while there was no statistically significant difference in expenditure per customer for MUP1 products and a small effect on MUP2 products, expenditures per customer of MUP3 products declined by 8.6% in Scotland. The combined net reduction on expenditure per customer for Scotland was 6.5%. There was no statistically significant difference between the two countries for prices.

Overall, these results imply that while consumption increased with the lockdowns, the relatively lower increase in total quantity and expenditure per customer in Scotland was mainly driven by the considerably smaller increases in demand for MUP2 and MUP3 products, which already had a 50ppu floor in place.

5 | DISCUSSION AND CONCLUSION

We find that minimum unit pricing as a price control can be highly effective in reducing demand for cheap alcohol with an overall net reduction in take-home alcohol as a measure to counter harmful drinking which lockdown restrictions could exacerbate. There is the risk that this could be only a short-term effect, where the sudden jump in the cheapest alcohol prices initially put off consumers faced with "sticker shock", but who might later adjust their expectations and start buying the products again. However, as Figure 1 clearly shows, this is not the pattern we observe over the full 6 months after the MUP introduction in Wales. Similarly, as evident from the comparison involving Scotland, MUP appears to have a persistent demand-dampening effect on cheap alcohol. It is also unlikely that our results are simply driven by an extensive increase in cross-border trade activity given the "stay in place" orders issued by the authorities during COVID-19 that limited the opportunities to make cross-border shopping trips in our context.

Perhaps the most striking aspect is how well targeted is the intervention with very limited spillover effects on sales or prices for more expensive (untreated) alcohol products. Our intuition is that the sharp price hike for the very cheapest alcohol cut its demand sharply but with some spillover as these consumers switched to buying more from the next quality tier up, whose prices had been hovering around the price floor, but in turn whose existing consumers bought less due to less discounted promotional prices. If so, then these opposing demand effects essentially canceled out each other for this mid-tier, yet with little further spillover to the top tier.

Our results and findings provide some broad welfare insights on the effects on industry revenue and consumer surplus, even if we cannot address equity issues for different household types with our aggregate customer purchase data.

For the industry, we find no windfall revenue gain arising from MUP. The weakness of demand spillover effects across the price tiers means that the sharp reduction in sales for cheap alcohol is not offset by increased sales of more expensive alcohol. The net effect on expenditure is marginally negative, where higher prices (implying higher unit margins) are not enough to compensate for the loss of volume lowering overall revenue. While other papers may have found the same outcome by examining expenditure by different household types (e.g., heavy, moderate, or light drinkers), we provide a different insight by examining the impact on why there is no revenue gain by finding that demand for the cheapest alcohol (MUP3) is surprisingly elastic and that there is no substantial demand gain for the next price tier of products (MUP2). We reason that is because MUP also affected these products partially by curtailing the retailer's ability to offer deep discounts (below 50 ppu) as a promotional tool, so while MUP may have led some consumers to trade-up to the next quality level from the very cheapest alcohol this demand gain was offset by existing customers of this price tier buying less. Meanwhile, the upper price tier for the most expensive alcohol (MUP1) witnessed almost no spillover effect whatsoever.

We have not probed the politics of MUP as a policy choice and industry's position on MUP, but with our finding we can see why industry might have lobbied against the policy's introduction in England (Woodhouse, 2020). However, this goes against the prior view that industry would stand to gain considerably from the introduction of MUP (The Economist, 2018). For instance, back in 2010, the Institute for Fiscal Studies predicted that rolling out MUP across Britain "could transfer £700 million from alcohol consumers to retailers and manufacturers" and "The largest beneficiary in cash terms would be Tesco (which gains around £230 million)" (Griffith & Leicester, 2010). In contrast, our results indicate that Tesco has not gained any revenue from the introduction of MUP.

On the consumer side, we can consider the effect on direct consumers, that is, those who were consuming MUP3 and MUP2 products, as well as those who could have been affected indirectly via any price effects on MUP1 products.

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Our analysis suggests that the policy effect is largely limited to those who were consuming low-quality cheap alcohol with little spillover effects to MUP1 products and hence should not change the consumer surplus for those consuming more expensive alcohol, which by volume is only 28.6% of the share. The significantly raised prices and lower consumption of MUP3 products imply substantially reduced consumer surplus for these purchasers in the traditional sense, that is, area under the demand curve (and leaving aside the own-health benefits from reduced alcohol consumption). While MUP2 purchasers lose out financially through higher average prices given that MUP prevents deep discounting for promotional offers that would otherwise take prices below the MUP (50ppu) price floor.

Beyond the direct economic welfare effects, a natural follow-up question to ask is whether the reduced alcohol consumption arising from MUP identified in our study has led to any broader societal benefits on public health and safety. We do not have any matching micro-data to examine this issue in sufficient detail to complement the study in this paper. However, we have investigated publicly available data on population-level trends. We summarize our findings here and provide the details in Appendix E, documenting the trends on three sets of outcomes: drink driving, alcohol-related deaths and hospitalizations, and domestic violence.

On drink driving, Francesconi and James (2022) find no evidence of a reduction in road crash deaths and drunk driving collisions following the introduction of MUP in Scotland through to December-end 2019. Similarly, our analysis of trends through to 2021 finds no clear indication that the introduction of MUP in Wales led to a discernible change in drink driving occurrences in comparison to the trends observed in England over the same period. Certainly, the COVID-19 lockdowns reduced traffic levels and reduced road traffic collisions, but alcohol impairment continued to feature as a contributory factor in road traffic collisions and the proportion of positive or refused breath tests rose in Wales.

On public health, Wyper et al. (2023) find that the introduction of MUP in Scotland, drawing on comparisons with England through to the December-end 2020, was associated with a significant 13.4% reduction in deaths and a 4.1% reduction in hospitalizations wholly attributable to alcohol consumption, with beneficial effects driven by significant improvements in chronic outcomes, particularly alcoholic liver disease. However, we find no indication that Wales has achieved similar benefits to those in Scotland in reducing the overall alcohol-specific death rate since introducing MUP. The difference might be down to the different trends for Scotland and Wales, especially on deaths linked to chronic conditions, or a delayed impact from the introduction of MUP because of the pandemic. Even so, deaths from accidental poisoning dropped markedly over the pandemic in Wales and Scotland compared to England and Northern Ireland, which is consistent with MUP deterring extreme binge drinking. On hospital admissions, we find the number of alcohol-related hospital admissions declined more in Wales than in England in the two years from March 2020, but so did total hospital admissions, and it is difficult to determine the net effect from the introduction from MUP given the considerable disruption to hospital admissions caused by the pandemic.

Finally, regarding domestic violence, the link between alcohol and domestic violence is well established empirically, and crime surveys from England and Wales show that victims believed offenders were under the influence of alcohol in around one third of domestic abuse incidents. If MUP helps reduce excessive alcohol consumption then that, in turn, might help reduce the incidence of domestic violence. However, we find no information available from public data sources and official statistics that allows us to assess directly whether the introduction of MUP in Wales had a dampening effect on the number of alcohol-involved domestic abuse cases. Nevertheless, based on all domestic abuse incidents recorded by the regional police forces in England and Wales, we find a comparative reduction in domestic abuse cases in Wales during the year when lockdowns occurred, but that such an effect dissipated when lockdowns ceased in the following year, and so any MUP effect may have been short-lived or possibly coincidental and dependent on how incidents were reported during the pandemic.

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DATA AVAILABILITY STATEMENT

Data and code used in this paper are available via the replication package available at https://doi.org/10.3886/E199482V1.

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ENDNOTES

1 In contrast, in highly regulated markets, state control of liquor stores or strict licensing can help ensure high prices (e.g., Avdic & von Hinke, 2021; Conlon & Rao, 2019, 2020; Miravete et al., 2018, 2020).

2 For instance, Scotland and Wales apply the formula M × S × V, where M is the minimum unit price (£0.50 per 10 mL ethanol), S is the percentage strength of the alcohol (ABV expressed as a cardinal number), and V is the volume of the container in liters.

3 The Republic of Ireland introduced MUP in 2022 and other countries continue to evaluate the policy. Other forms of minimum pricing are by liquid volume (e.g., parts of Australia and Canada) and/or product-specific (like high-strength spirits in several former Soviet countries) (WHO, 2020).

4 The U.K. government decided in 2013 not to adopt MUP in England following industry lobbying against the policy (Woodhouse, 2020). The policy is still under consideration in Northern Ireland.

5 Even so, there are some differences in consumption patterns across the four countries, notably with higher consumption in Scotland than the other three constituent countries. However, the volume gap has narrowed over the past 20 years (Giles & Richardson, 2020). There are also some differences in the composition of sales, such as more spirits but less beer consumed per adult in Scotland compared to England and Wales, but both the long-term trends and weekly sales patterns are very similar (Giles et al., 2019).

6 The retailer is Tesco which held a consistent national market share of 27% over the study period for both alcohol and total grocery retail spending (based on Kantar Worldpanel FMCG data). Further details are available upon request.

7 Data and code used in the paper are available from Bokhari et al. (2024).

8 The 80% cutoff represents the clearest break in the distribution of products by the proportion of weeks with prices below 50ppu, see Appendix A. For a robustness check, we also applied an alternative categorization of MUP products based on total sales that fall below the MUP price instead of the frequency of weeks that the price is below 50ppu. The concordance analysis reported in Appendix A shows an almost full overlap of the two categorization approaches.

9 In the raw SKU level data we observed deviations in 0.48% cases where the transaction price was slightly below 50 ppu in Wales in the post-regulation period. This could be an error or some unaccounted bundle purchase.

10 The adult (18+) populations are Wales 2.5 m, England 44.2 m, Scotland 4.5 m, and Northern Ireland 1.5 m. Because of large differences in adult population, in the figures we have demeaned the log quantity over the pre-ban period, that is, from each log quantity at time t, we subtract the mean value for the specific product group and country in the pre-ban period.

11 At least three earlier papers (Bokhari et al., 2023; Griffith et al., 2019, 2022) that used household-level microdata from the UK have reported elasticities by type of household: low, medium, and high alcohol-consuming households or by income groups. Bokhari et al. (2023), (Table 3) report that high-drinking households have the most elastic demand (−1.793) while low-drinking households have the least elastic demand (−1.091). Similarly, the earlier IFS working paper version of Griffith et al. (2022) shows that for both low- and high-consumption groups, the elasticity of alcohol for low-income households is larger in magnitude than that of high-income groups (but not so for the medium-consumption group). To the extent that MUP3 products are more likely to be purchased by low-income and/or high-consumption households, our elasticity measures by product groups are consistent with earlier studies.

12 The combined effect was computed using auxiliary regressions similar to Equation (1), but where MUP2 and MUP3 were combined into a single category. Appendix C, section C.3 provides details and regression outputs with results in Table C-5. An alternative is to compute the combined effect by multiplying coefficients with pre-MUP means for MUP2 and MUP3 to compute the implied change and convert back to percentages. However, such computations rely on first-order Taylor series expansions and the approximation becomes worse when estimated coefficients are far away from zero (above 0.1 in magnitude), as is the case for our estimates of β_{10} and β_{11}.

13 The one exception is for ln(qnty) for MUP3 parameter β_{11} in which case the parameter is significantly different from zero at the 10% level. But even here, the magnitude is only −0.0204 in this placebo test, and compares sharply with the non-placebo variant reported in Table 3 where the same parameter was −0.587 and significant at 1% level.

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