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Alcohol Prohibition and Pricing at the Pump*

Kai Fischer†

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Abstract

Firms often sell a transparent base product and a valuable add-on product. If only some consumers are aware of the latter, the add-on's effect on the base product's price will be ambiguous. Cross-subsidization between products to bait uninformed consumers might lower, intrinsic utility from the add-on for informed consumers might raise the price. We study this trade-off in the gasoline market by exploiting an alcohol sales prohibition at stations as an exogenous shifter of add-on availability. Gasoline margins drop by 5% during the prohibition. The effect is mediated by shop variety and local competition. Implications for gasoline market definition arise.

Keywords: Off-Premise Alcohol Prohibition, Gasoline Market, Multi-Product Firms

JEL Codes: L11, L91, R41

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1 Motivation

The literature on gasoline markets is broad and has examined many features typical of gasoline competition, such as price dispersion, asymmetries in input cost pass-through and Edgeworth cycles. Most approaches to these topics assume that competition occurs only among gasoline stations, which are usually treated as single-product firms solely selling homogenous gasoline. Only a few papers have dealt with the relation of gasoline prices to stations' attached services such as shops, supermarkets or carwashes (Doyle et al. 2010, Haucap et al. 2017*a*,*b*, Wang 2015, Zimmerman 2012). However, potential interactions of pricing at the pump and the provision of add-on services have relevant implications for market definition and unveil distributional consequences for heterogenously informed consumers. If add-ons distort the signal, that low prices imply the best deal in a homogenous product market like the gasoline market, matching of consumers, who are uninformed about add-ons, to suitable stations could deteriorate.

Whether the provision of add-on quality will raise or lower gasoline prices - relative to a world without add-ons - if only some consumers are aware of the add-on, is unclear from an ex-ante perspective. On the one hand, better services or a wider product assortment increase intrinsic utility of some consumers' shopping. Also, consumers will face opportunity costs of travelling if they are not one-stop shoppers but consume gasoline and the add-on not from just one station. On the other hand, gasoline stations might use low gasoline prices as a quasi-loss leader to bait uninformed consumers. Cross-subsidization could arise (Armstrong & Vickers 2012, Gabaix & Laibson 2006, Heidhues et al. 2017, Lal & Matutes 1994). Less transparently priced add-on services (or products) might then be purchased by consumers. Therefore, the overall price effect of add-on availability is ambiguous and a question for empirical research. Similar trade-offs can be found in most markets. In this work, we go into this matter. We causally identify the role of add-on variety for firm behavior. We exploit a unique setting in the gasoline market, where the availability of an add-on is exogenously determined by public policy. In particular, we examine a quasi-experient, the lifting of a local nightly alcohol ban at gasoline stations in a federal state of Germany, as a shifter of add-on availability. The prohibition restricted the shop assortment of stations as it mandated alcohol sales to be forbidden from 10pm to 5am each day. The policy was implemented in 2010 and lifted in December 2017. It aimed at the reduction of binge alcohol consumption among youths at night. As 60% of all profits of German gasoline stations are linked to the shop, 20% to carwashes and only 20% to gasoline sales (FAZ 2015, Ivanov 2019, Nicolai 2021, NTV 2015), the alcohol sales ban reflects a relevant revenue shock.

We use real-time data of all gasoline prices in the German gasoline market on the station level. By means of a difference-in-differences setup, we take advantage of the low menu costs and within-day variation of prices and compare gasoline prices during and after the prohibition as well as between affected and unaffected stations. This allows us to unveil the overall price effect of add-on availability on the base product's price. Building on precise information about stations' competitive environment and brand affiliation, we further are able to investigate heterogeneity across firms.

Our findings and contributions to the literature are threefold. Firstly, we investigate the effect direction of add-on quality on gasoline prices. We find nightly prices of stations affected by the prohibition to drop by 0.6 Eurocent/l - or 5% of the net margin - during the prohibition. Hence, especially consumers who did not buy alcohol profited from the policy. Stations with smaller product variety, where alcohol's relative importance for shop revenues is higher, reveal even higher price effects. Similarly, stations with few competitors nearby increase prices more strongly. Opportunity costs of buying alcohol at another station increase. Thus, a potential cross-subsidization mechanism is overall outweighed by the intrinsic value of additional services. We, therefore, add to the literature on the role of station amenities for stations' pricing behavior. Other papers have shown that stations' choice to operate convenience stores (Doyle et al. 2010, Ning & Haining 2003, Haucap et al. 2017*a*) and the proximity to hypermarkets nearby (Zimmerman 2012) indeed shape pricing behavior. Though, they mainly rely on the endogenous self-selection of stations into low- or high-quality segments while we exploit an exogenous shifter of service and add-on availability. Our results also address the delineation of gasoline markets as price effects vary with the exposure to alcohol sales. Alcohol revenues are also determined by local supermarkets or pubs. This indicates that gasoline stations might not only compete with other stations.

Secondly, while our results address discussions on multi-product competition across most markets, note that the setting studied in this paper is unique. It mainly differs from other markets with two price components in three ways: At first, add-on services often are valueless to the consumer and are only jointly bought with the base good such as overdraft fees for financial services (Armstrong & Vickers 2012, Gabaix & Laibson 2006). In our setting, consumers are free to opt out of buying alcohol but can still buy other shop products. Beyond that, purchasing alcohol gives positive utility to some consumers. Second, firms often endogenously set the prevailing level of consumer information about prices in the market for the base product by, for example, advertising prices. We consider a price transparency environment which exogenously dictates prices to be equally transparent across firms. By law, gasoline prices of all German stations are published in real time for consumers. Lastly, we do not just vary revenues from add-ons but study the role of add-ons' existence at the extensive margin. Hence, our results represent an upper bound for fluctuations of add-on revenues in our setting and is helpful in forming benchmarks for other industries.

Thirdly, we analyze how active stations are in response to the prohibition. 10% more stations adjust prices during night hours after the lifting of the policy. While this observation could purely represent changes in the Edgeworth cycles, we show that prominent characteristics of price cycles are unaffected by the policy lifting. Therefore, we believe these findings to express changes in opening hours.

The remainder of the paper is as follows: We start with an explanation of the institutional background and a theoretical motivation in Section 2 and 3 before presenting our data and empirical strategy in Section 4. We then proceed with our analysis in Section 5 before providing robustness checks and a conclusion in Sections 6 and 7.

2 Institutional Background

Particularly, we examine a nightly off-premise alcohol prohibition in Baden-Wuerttemberg, a German federal state with a population of eleven million. This policy primarily affected gasoline stations as main nightly off-premise places to go for alcohol (Marcus & Siedler 2015)[1]. From 2010 onwards, Baden-Wuerttemberg prohibited nightly alcohol sales from 10pm to 5am via the 'Alkoholverkaufsverbotsgesetz' (Alcohol Sales Prohibition Law). As most people do not prestore alcohol, the prohibition was, to a large extent, binding (Marcus & Siedler 2015). This specific legislation ran out on December 08, 2017, as local authorities from then on should have selected specific 'hotspots' (e.g., city centres) for bans only.

Its main intentions were the reduction of binge drinking among youths and of indirect spillovers on crime (Baumann et al. 2019, Marcus & Siedler 2015). The policy was clearly effective in several ways indicating a real shock in the volume of alcohol consumed. Up to now, Marcus & Siedler (2015) and Baumann et al. (2019) discussed direct effects on health costs and crime for this specific case study. Both find that the policy had a significant and economically relevant effect. The number and the length of hospital stays among youth binge drinkers and late-night assaults fell due to the policy.

As the legislation ran out ahead of time - it was expected that the legislation would not change before 2018 (Mayer 2017) - and because the law was ineffective just a few days after the public announcement of the abolition, anticipatory effects are unlikely.

We expect such a regulation to have a sizeable impact on the German gasoline market. In Europe, German gasoline stations have one of the lowest net margins on fuels (Scope Ratings 2019). Therefore, shop sales make up a relevant share of stations' overall profits. In particular, alcohol and beverage sales account for more than 10% of all in-shop sales (Scope Ratings 2019). Moreover, consumers coming for alcohol buy other products on the way. Recent years have shown that especially big brands such as ARAL extended their shops by for example integrating shops of supermarket chains. In contrast to other countries, German gasoline stations mostly did not introduce paying at the pump by card, as this would stop consumers from entering the store. Hence, most stations are occupied in person all day long, so that shop sales are possible. Moreover, German gasoline

[1] During the prohibition, only stations which also ran a diner with an official catering license to sell on-premise alcohol were still allowed to sell alcohol at night (§3a Abs. 1 LadÖG). This mainly concerned highway stations with rest houses. which at the same time were not allowed to sell alcohol due to a highway-specific alcohol prohibition.

stations often act as "shopping location of last resort" during night times as then German groceries rarely open. Thus, a nightly prohibition impedes a relevant business time.

Alcohol revenues may be relevant for gasoline prices. In response, cross-subsidization could plausibly be an optimal pricing strategy next to quality-related price inclines. To show this, we perform a simple back-of-the-envelope calculation based on some assumptions. Following the Statistical Office Baden-Wuerttemberg (2019), overall annual gasoline consumption was approximately 7 million tonnes or 8.5 billion litres in 2017. Admittedly gasoline demand is low at night. But the Federal Cartel Office (2019) documents that still around 5% of the daily gasoline trips are at night (10pm to 5am). This implies that around 425 million litres p.a. are sold in Baden-Wuerttemberg at night. Uniformly distributing this over approximately 800 gasoline stations which operate at this daytime, this is slightly more than 0.5 million litres per station and year. If a station followed a cross-subsidization strategy which lowers margin by, for example, half a Eurocent/l, it would lose 2,500 Euro p.a.. This needs to be compensated by additional alcohol sales triggered through lower prices at the pump. Following Scope Ratings (2018), German gasoline stations, on average, earn almost one million Euro p.a. of revenue in their shops, of which alcohol products account for approximately a tenth. As alcohol is sold in evening and night hours for the most part, profits from alcohol sales due to additional attracted consumers could exceed the cost of using gasoline as a bait. In the setting studied in this paper, consumers' alcohol demand response to lower gasoline prices is changed from zero to potentially non-zero after lifting the prohibition.

3 Theoretical Sketch

To get a better understanding of the ex-ante ambiguity of the policy's effect on gasoline prices, consider a fictive gasoline station's profit function before (π^b) and after the prohibition lifting (π^a)

$$\pi^b = p_G^b[\lambda D_\lambda(p_G^b, \gamma_A = 0) + (1-\lambda)D_{1-\lambda}(p_G^b)]$$

$$\pi^a = (p_G^a + p_A)[\lambda D_\lambda(p_G^a, \gamma_A = 1) + (1-\lambda)D_{1-\lambda}(p_G^a)]$$

where p_G^b, p_G^a are the prices for gasoline before and after the policy lifting and p_A is the price of the add-on. $\gamma_A \in \{0, 1\}$ gives whether an add-on is available ($\gamma_A = 1$) or not ($\gamma_A = 0$). λ is the share of informed consumers who are aware of alcohol add-ons when choosing a gasoline station. $1 - \lambda$ consumers do not consider the existence of alcohol as an add-on product. Demand of informed and uninformed consumers is given by $D_\lambda(p_G^t, \gamma_A)$ and $D_{1-\lambda}(p_G^t)$ with $t \in \{a, b\}$ respectively. Alcohol can only be sold after the lifting. For simplicity, we assume (marginal) costs of zero and that every consumer either buys no product or both, gasoline and alcohol. We also ignore the add-on's price p_A in $D_\lambda(p_G^t, \gamma_A)$ which does not affect the trade-off discussed as long as the intrinsic utility from the add-on is sufficiently high.

Maximizing the profit functions then yields the first-order conditions with respect to p_G^t:

$$p_G^b = -\frac{\lambda D_\lambda(p_G^b, \gamma_A = 0) + (1-\lambda) D_{1-\lambda}(p_G^b)}{\lambda D'_\lambda(p_G^b, \gamma_A = 0) + (1-\lambda) D'_{1-\lambda}(p_G^b)} \qquad p_G^a = -\frac{\lambda D_\lambda(p_G^a, \gamma_A = 1) + (1-\lambda) D_{1-\lambda}(p_G^a)}{\lambda D'_\lambda(p_G^a, \gamma_A = 1) + (1-\lambda) D'_{1-\lambda}(p_G^a)} - p_A(p_G^a, \lambda)$$

From the conditions, one can identify the two channels which mainly drive price differences for gasoline before and after the policy lifting: Firstly, prices after the lifting are reduced by the alcohol price. This *cross-subsidization channel* characterizes gasoline as a bait for uninformed consumers. Secondly, informed consumers increase demand due to the availability of alcohol products (s. nominator of optimal prices). This *service quality channel* hence might increase prices. The overall effect is ambiguous.

4 Data and Empirical Strategy

Gasoline Price Data. We make use of E5 gasoline prices from all German gasoline stations. The data is collected by the Market Transparency Unit for Fuels (MTU) at the German Federal Cartel Office. The data is gathered in real time which allows us to exploit within-day price variation as needed in our setup. We use a full year of price data (mid-September 2017 to mid-September 2018). We construct the time-weighted average daytime (05am to 10pm) and nighttime (10pm to 05am) price per week and station.

Station Characteristics. Further, the MTU provides exact information on station characteristics such as their brand affiliation and geographical location. From this source, we derive whether stations open all day long (24/7) and operate at night. Our final sample only consists of such 24/7 stations. We use the location data to match stations to counties.

To study heterogeneity across stations, we construct a set of variables containing stations' degree of upstream integration and station's brand value as in Haucap et al. (2017*a*). By that, we are able to proxy market power and heterogeneity in shop assortments. We also construct competition measures such as the distance to the nearest competitor or the number of stations in a certain radius around a station.

Finally, we manually identify around 380 highway stations from our sample as those are typically assigned to a separate market (Federal Cartel Office 2011)[2]. They also face §15 Abs. 4 Bundesfernstrassengesetz (FStrG), which prohibits selling alcohol at highway stations from 12pm to 7am, independently of the discussed prohibition. Hence, the lifting of the treatment should not have been binding as they are still not allowed to sell alcohol.

Overall, we end up with a panel of more than half a million observations for over 6,000 24/7 stations of which approximately 13% are located in Baden-Wuerttemberg.

Empirical Approach. Using this data, we apply a triple difference-in-differences (TDID) estimator, which studies the effect of abolishing the prohibition across federal states and over daytimes[3]. We prefer a TDID estimator over a DID estimator with just nightly prices before and after the lifting because prices are correlated within the day at the station level due to intra-day Edgeworth cycles. So, we avoid missing treatment effects pushed out of the nighttime period (e.g. anticipatory alcohol purchases right before 10pm might affect prices). Nevertheless, we provide supporting simple DID results on daytime and nighttime prices separately later on as well. The regression setup is as follows:

$$P^{E5}_{swn} = \alpha_s + \lambda_w + \lambda_w \times Night_n + \beta_1(BW_s \times Night_n) + \beta_2(BW_s \times Post_w) + \beta_3(BW_s \times Night_n \times Post_w) + \epsilon_{swn}$$

[2] For details on German highway stations see Haucap et al. (2017*a*) and Korff (2021).

[3] As the MTU has been launched after the prohibition's introduction in 2010, we study its lifting.

In particular, P^{E5}_{swn} is the E5 gasoline price at station s in week w at daytime $n \in \{Day, Night\}$. α_s and λ_w are station and week fixed effects. $\lambda_w \times Night_n$ are week-times-daytime fixed effects which control for underlying daytime and week trends[4]. $BW_s \times Night_n$ and $BW_s \times Post_w$ control for daytime and real-time price differences between control and treatment group where BW_s, $Night_n$ and $Post_w$ are dummies for (i) the treated federal state, (ii) night hours, and (iii) weeks after the date of the prohibition lifting, 08th of December 2017. $BW_s \times Night_n \times Post_w$ is the treatment indicator, so that β_3 gives the treatment effect of the policy lifting. We later on show that our results are robust to other specifications of the TDID setup.

Identification. We observe an exogenous policy treatment on the state level. Interpreting our estimates as causal is valid under the assumptions that (i) treated and untreated stations would have been on the same trend in the absence of the treatment and that (ii) treatment and firm behavior of one station does not affect the treatment and outcomes of other stations (corresponding to the stable unit treatment value assumption). To investigate the parallel trend assumption, we will provide dynamic TDID regressions where to split up the treatment effect into its time-specific components. Flat pre-trends will be indicative of the parallel trend assumption's validity (s. e.g., Olden & Moen (2020)). The setup is as follows:

$$P^{E5}_{swn} = \alpha_s + \lambda_w + \lambda_w \times Night_n + \beta_1(BW_s \times Night_n) + \beta_2(BW_s \times Post_w) + \sum_{t=\underline{\tau}, t\neq -1,-2}^{\bar{\tau}} \gamma_t 1[BW_s \times Night_n \times Lifting_{w-t}] + \epsilon_{swn}$$

where $\sum_{t=\underline{\tau}, t\neq -1,-2}^{\bar{\tau}} \gamma_t 1[BW_s \times Night_n \times Lifting_{w-t}]$ gives the sum of all leads and lags of the treatment effect - in two-week bins - except for the omitted reference category before the shock. Regarding the second assumption, spillovers between treated and untreated stations are unlikely due to the exogenously fixed treatment, strict geographical separation of treated and untreated stations and narrow local markets. There are only interactions of treated and untreated stations at the state border, which we will investigate later on.

[4]Due to changes in the Edgeworth cycles over time, daytime price effects differ strongly in real time, so that we control for this variation by interacting the daytime dummy with week fixed effects.

Besides that, one concern in our setting is that the composition of treatment and control group changes due to the treatment. For example, fewer revenues due to the prohibition may lead to market exit during nighttime. Note that this would only downward bias our effect due to softening competition and higher prices during the prohibition. If our treatment effect is positive, we therefore do not face problems to interpret results with regard to which channel outweighs in the discussed trade-off.

Descriptive Statistics. As treatment effects might be a function of, for example, station characteristics or local competition, Table 1 offers insights on structural differences between the treatment and control group before the treatment. While the price level prior to the prohibition lifting has not been statistically different across both groups, the likelihood to operate at night in terms of changing prices were more extensive outside of Baden-Wuerttemberg. Competitive environments, on average, are similar and stations mostly seem to differ in the likelihood of being affiliated with an oligopolistic brand. These stations are meant to have high market power in, for example, steering the Edgeworth cycles (Federal Cartel Office 2011). Lastly, treated stations are more likely to be located in wealthier counties with more vehicles per person. As station differences, hence, primarily lie in mostly time-invariant dimensions such as the brand affiliation or county-specific demand conditions, we are able to address this heterogeneity by, for example, using station fixed effects.

5 Results

Baseline Results. In particular, we present our baseline results in Table 2. A positive treatment effect would imply prices to increase after a prohibition lifting. In this case, the direct quality-price complementarity would outperform the cross-subsidization channel. Our baseline results in model (1) show that, generally, prices rise in Baden-Wuerttemberg after the prohibition lifting during night hours. The effect size is 0.56 Eurocents/l which corresponds to an increase by more than 5% of the average station's profit margin per litre (Scope Ratings 2019).

Table 1: Descriptive Statistics

Statistic	Units	Control (Pre-Lifting) (1)	BW (2)	Δ (p-value) (3)
Outcomes				
E5 Gasoline Price (Day)	Euro/l	1.373	1.370	0.39
E5 Gasoline Price (Night)	Euro/l	1.439	1.433	0.11
1[Active between 10pm and 5 am]	yes/no	0.863	0.836	0.03**
1[Active between 11pm and 4 am]	yes/no	0.515	0.472	0.01***
Competition				
# Competitors 0.5km Radius (Day)	#	0.472	0.449	0.56
# Competitors 0.5km Radius (Night)	#	0.257	0.230	0.32
# Competitors 1km Radius (Day)	#	1.081	1.070	0.90
# Competitors 1km Radius (Night)	#	0.546	0.535	0.85
Station Characteristics				
Income (County Level)	Euro	22341.1	24450.0	0.00***
Commuter Share (County Level)		0.388	0.348	0.11
Car Density (County Level)		0.567	0.572	0.01**
Truck Density (County Level)		0.038	0.033	0.00***
Share of Youths (18-25 y.o., County Level)		0.086	0.096	0.00***
Premium Station	yes/no	0.437	0.411	0.32
Oligopolistic Station	yes/no	0.372	0.273	0.00***
Highway Station	yes/no	0.051	0.046	0.65

Note: This table compares descriptive statistics of untreated stations with treated stations (both pre-treatment). The p-values come from linear regressions of the respective outcome on an intercept and a dummy for Baden-Wuerttemberg where we implement heteroskedasticity-robust standard errors clustered on the county level.

To show that we do not take up unrelated variation, which does not correspond to the daytime-specific treatment, we check whether night prices purely drive the effect in models (2) and (3). The respective simple DID regressions show that only night prices increase significantly while day prices are unaffected by the prohibition lifting. This is in line with our intuition. In model (4), we use gross margins (price minus taxes) as outcome, which are subject to subtracting input costs to arrive at net margins.

A positive effect is indicative of alcohol assortments improving the quality of gasoline stations

Table 2: (Triple) Difference-in-Differences Regression

	Gasoline Price in Euro/l			ln(Gross Margin)
	(1)	(2)	(3)	(4)
BW×Night×Post	0.0056**			0.0085**
	(0.0022)			(0.0037)
BW×Post		0.0080***	0.0025	
		(0.0024)	(0.0019)	
Approach	TDID	DID	DID	TDID
Sample	Baseline	Only Night	Only Day	Baseline
Observations	593,193	296,598	296,595	593,193
Adjusted R^2	0.889	0.876	0.953	0.885

Note: *p<0.1; **p<0.05; ***p<0.01. All results are based on OLS regressions with heteroskedasticity-robust standard errors clustered on the county level. The regression setup follows the regression equation from the 'Data and Empirical Strategy' section. Simple DIDs in models (2) and (3) include station and week fixed effects as well as the reported interaction term.

for the consumers. Consumers are willing to pay more at the pump as they, for example, get additional services. If consumers enter the station to purchase alcohol, gasoline might be sold as a by-product. Interestingly, we do not find any evidence for lower gasoline prices after the prohibition which fits a story of gasoline being a bait product for stations. This would have been in line with cross-subsidization if consumers had not been aware of buying alcohol when approaching a station to fuel (Gabaix & Laibson 2006, Lal & Matutes 1994). Similarly, Haucap et al. (2017*b*) discussed that carwashes or supermarkets typically offer fuel cheaply. Hence, the mechanism underlying our observations here is likely to be reverse. Consumers approach stations with the purpose of buying alcohol and then are willing to fuel at a higher price as they otherwise would face non-negligible opportunity costs of an additional trip.

The effect is remarkable, especially when considering that alcohol sales only make up 10% of an average station's shop revenues. Extrapolating this to the overall importance of the shop for price setting, gasoline competition is highly related to shop revenues. Strategic interactions of shop assortment and gasoline prices also indicate that gasoline stations act like multi-product firms.

Note that we cannot fully exclude that our reduced-form effect is a sum of a cross-subsidization effect (which reduces gasoline prices) and the discussed quality improvement (which increases prices). We can only ensure that the quality and intrinsic utility channel dominates. We later check whether cross-subsidization may play out more strongly for bigger shops, so that the treatment effect might vary across stations' types of shops.

Dynamic Estimates. To verify that the observed effects really originate from the legalization's lifting and hence can be interpreted as causal, we provide two types of dynamic approaches: Firstly, we apply a dynamic TDID setup in which the treatment effect is split up into several smaller time intervals before and after the treatment.

Figure 1 gives the dynamic estimates from the baseline regression above. As evident, we observe the significant price drop to just arise after lifting the prohibition. While there is a slight delay until the treatment effect evolves, the effect size remains constant after some weeks until the end of the time window[5]. The slight delay is in line with the unexpected timing of the policy lifting. Pre-trends are flat which gives us certainty that the effect is a consequence of the policy change.

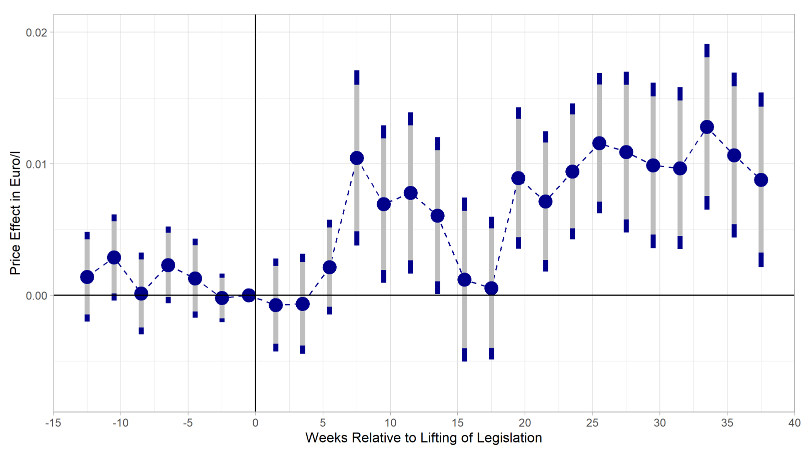

Figure 1: Dynamic Difference-in-Differences Estimates
This plot gives dynamic estimates of γ_t from the dynamic DID strategy discussed in the 'Data and Empirical Strategy' section. The exact timing of the ending of the prohibition is indicated by the black vertical line. We provide 90 and 95% confidence intervals. Standard errors are heteroskedasticity-robust and clustered on the county level.

Besides showing that the effect only arises after the legislation, we provide evidence beyond mod-

[5] After about 15 weeks, there is a short-term drop in the effect size. The timing corresponds to the Easter holidays and hence might reflect a short-term heterogeneous exposure to demand for alcohol at gasoline stations across federal stations in Germany.

els (2) and (3) in Table 2 that the treatment effect is purely bounded to night hours. This is done in Figure 2, where we run the simple DID regression of whether prices changed in Baden-Wuerttemberg after the treatment for hourly average prices on the week level separately. Indeed, the results closely represent the hypothesis that there is no treatment effect over daytime while a treatment effect arises at night. The effect does not appear immediately after 10 pm, which is likely related to limited demand effects for alcohol. Some supermarkets are still open until midnight, and most restaurants have not closed yet. Admittedly, there is a significant effect remaining between 5 and 6am. This is likely related to the given timing of the Germany-wide intra-day Edgeworth cycles, where most stations changed prices after 6am (Federal Cartel Office 2018).

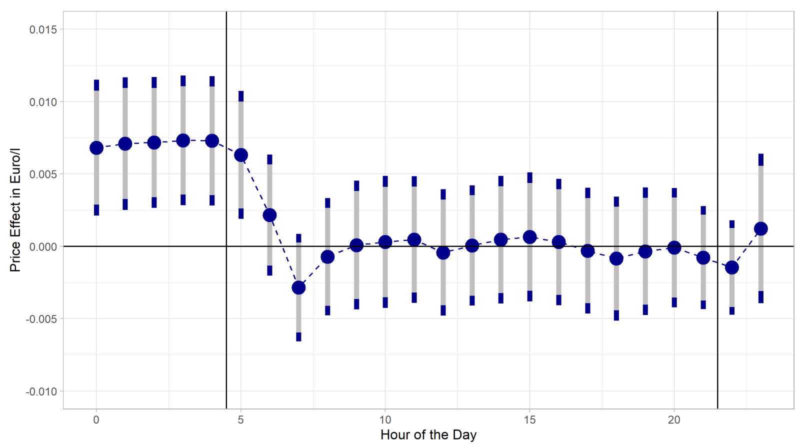

Figure 2: Dynamic Effects by Hour of Day
This plot gives dynamic estimates of the interaction term $BW_s \times Post_w$ of a simple DID model where one regression is run for each hour separately. The exact timing of the beginning and ending of the prohibition is indicated by the black vertical line. Standard errors are heteroskedasticity-robust and clustered on the county level. We provide 90 and 95% confidence intervals for all coefficients.

Heterogeneity Analyses. To understand which stations are more prone to react to the prohibition, we study effect heterogeneity across station characteristics such as competition at the pump, variety in the product assortment or brand affiliation.

Firstly, we study competition effects. As described above, our price effect likely originates from the mechanism that alcohol-demanding consumers visit gasoline stations and only consume gasoline on the side. Then, the price effect would arise from opportunity costs of travelling to a different gasoline outlet. This effect should be larger if alternative stations are far away. Similarly, if consumers only have one station nearby, they more likely are informed about the add-on which reduces the cross-subsidization incentive. Hence, lower gasoline competition should foster the effect. We study this by splitting the sample at the median number of nightly competitors in a 1km radius[6]. Figure 3 reports our results on heterogeneity analyses. Indeed, we find that lower competition is related to a higher nightly price increase after the prohibition lifting. Simultaneously, higher competition is correlated with stations lying in densely populated areas, so that stations in cities do not drive our effect[7]. In cities, alcohol consumers may be motorized less often which does not incentivize changes in gasoline prices.

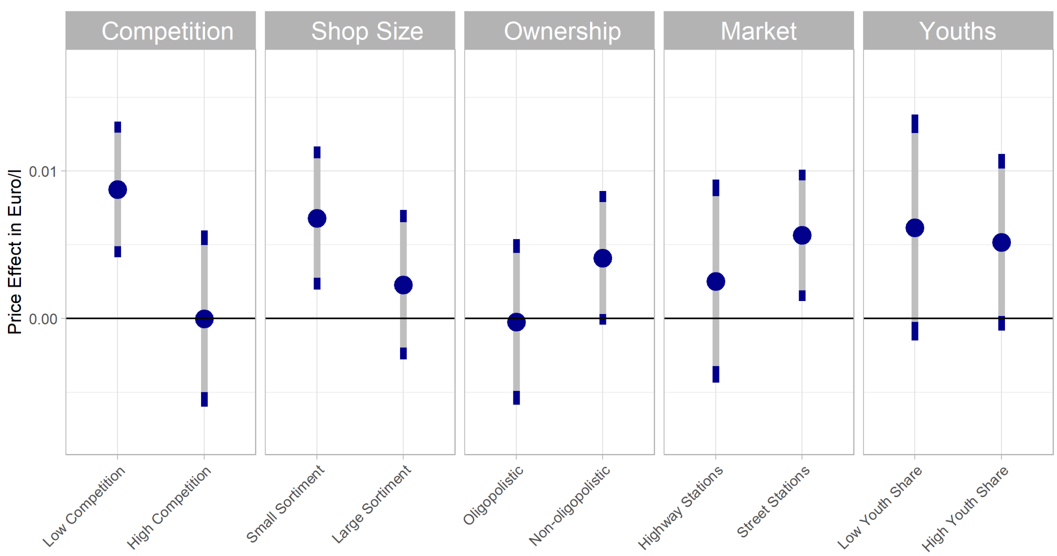

Figure 3: Heterogeneity Analysis: Intensive Margin
This plot gives the treatment effect β_3 from the baseline regression for subsamples along firm characteristics. The y-axis documents the effect size in Eurocent/l, the x-axis gives the respective subsample. 90% and 95% confidence bands are reported. Standard errors are heteroskedasticity-robust and clustered on the county level.

Secondly, we study how ex-ante shop assortments impact the price effect's size. To sort stations into different shop categories, we follow the definition by the Federal Cartel Office (2011). Stations are sorted into premium and small assortment stations based on their brand affiliation. Premium

[6] Our results also hold for different radii and sample splits not at the median.
[7] This also holds when studying the effect heterogeneity across county differences in the population density.

stations are known for a wider assortment of products. Alcohol is a very simple product offered by any station, so that the marginal return and relative importance of alcohol revenues is typically higher in smaller shops. We find premium stations with large product variety to not react significantly, while the price effect is especially evident for low assortment stations. Consumers who buy alcohol at gasoline stations may be likely to buy other shop products there as well, so that bigger shops do not experience a comparable shock to shops with smaller product varieties. In contrast, the premium station may face consumers who buy more after the prohibition but have visited the station before as well. This then does not lead to more gasoline sold at premium stations. Also, the null effect for premium stations might be a result of stronger cross-subsidization since gasoline purchasers might buy more products beyond alcohol when entering the store.

Thirdly, we study the role of market power. In the German gasoline market, market power is associated with vertical integration to oil refinery firms as these also supply competitors and have been determining the daily Edgeworth cycles for years (Federal Cartel Office 2011, Siekmann 2017). Vertical integrated, so-called 'oligopolistic' brands are, for example, Shell, Aral (BP) or Total. We study whether the effect differs across oligopolistic and non-oligopolistic brands. We find that especially non-oligopolistic brands increase nightly prices after the prohibition lifting. Our results show that oligopolistic stations' price level was not lower before the prohibition lifting, so that a price drop during the prohibition did not occur at stations with market power.

Fourthly, we study a sample of only highway stations. Highway stations have been subject to an alcohol prohibition throughout night hours, independent of the discussed alcohol prohibition. Hence, as these stations were still not subject to the opportunity to sell alcohol from December 08, 2017, onwards, we expect to observe a zero treatment effect. That is why this analysis might be interpreted as a 'quasi-placebo' test. Indeed, at highway stations, no price effect is found.

Lastly, we study heterogeneity in the local share of youths (18-25 year-olds). This traces back to Marcus & Siedler (2015), who find that the discussed alcohol prohibition especially reduced alcohol binge consumption among young adults. We investigate whether a higher share of youths proxies a demand shock for gasoline as well. Our estimates do not reveal treatment effect heterogeneity across stations from counties with different shares of youths. Youths are less likely to be motorists, which does not incentivize gasoline stations to change prices at the pump.

Station Activity. As we find that gasoline prices at stations in Baden-Wuerttemberg during the prohibition have been lower, there likely is an unambiguous effect on overall revenues of stations: Alcohol revenues vanish and gasoline prices drop. Hence, it is a natural question whether some stations change how actively they participate in the market.

To study stations' activity, we use the real-time price data to elicit whether a gasoline station in a certain week changed prices at night or not. If stations change prices, this will be indicative of whether they open at night. Due to data availability, we cannot fully exclude that effects on price changes are shaped by Edgeworth cycle adaptions due to the policy instead of operating times. Though, we provide some evidence in Table A.1 on whether gasoline stations in Baden-Wuerttemberg show different Edgeworth cycle characteristics after the policy lifting. The number of price changes over the day as well as cycling frequency and asymmetry remain unaffected.

We determine whether a station has changed its price between 10pm and 5am and, for a second measure, whether there have been changes between 11pm and 4am. We apply a standard dynamic DID estimator in a two-way fixed effects model to study stations' propensity to operate at night. Again, flat pre-trends will be indicative of whether the parallel trend assumption holds:

$$1[Active\ at\ Night]_{sw} = \alpha_s + \lambda_w + \sum_{t=\underline{\tau}, t\neq -1,-2}^{\bar{\tau}} \gamma_t 1[BW_s \times Lifting_{w-t}] + \epsilon_{sw} \tag{1}$$

$1[Active\ at\ Night]_{sw}$ is a dummy which will turn one if a station s has operated at night in week w. We apply two definitions for this outcome: Firstly, the variable will turn one if a station is active/changes the price at least once in a week between 10pm to 5am. Secondly, the variable will turn one if a station is active/changes the price between 11pm and 4am at least once a week.

Figure 4 gives the dynamic estimates for both outcomes. It appears that the share of stations being active at night increases substantially after the lifting of the prohibition. In fact, stations in Baden-Wuerttemberg are 8.7 percentage points (or 10% respectively) more likely to operate/change prices

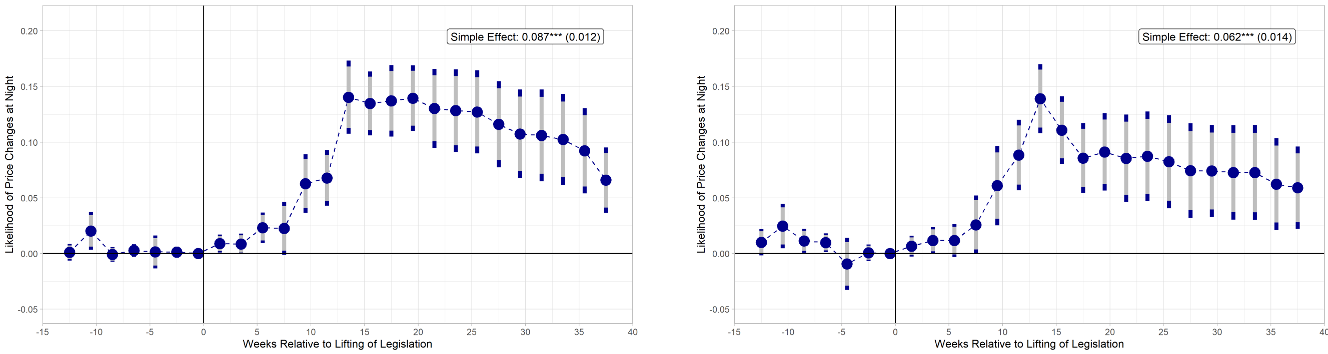

Figure 4: Dynamic Effects on Likelihood to be Active at Night
This plot gives dynamic estimates of the leads and lags from equation (1). The left plot defines $1[Active\ at\ Night]_{sw}$ with changing prices between 10pm and 5am, the right plot takes a more restricting defintion of price changes between 11pm and 4am. Standard errors are heteroskedasticity-robust and clustered on the county level. The exact timing of the beginning and ending of the prohibition is indicated by the black vertical line. We provide 90 and 95% confidence intervals for all coefficients from a linear probability model.

at some point between 10pm and 5am than when the prohibition was active. In contrast to the price effect, which arises after 5-7 weeks, the reaction in night activity takes about twice as long until reaching a constant treatment effect level. This is very much in line with lower menu costs for price level changes than structural changes in a station's activity at night.

When investigating heterogeneous responses across stations with small or large assortment, we find heterogeneity, which corresponds to the price effects found above. Stations with a small assortment typically sell less products, so that a restriction of alcohol might hit them more strongly. Indeed, we find that such stations react more pronouncedly in activity during prohibition hours (s. Figure 5). We also checked again, whether highway stations do not react to the policy in means of nightly activity and, indeed, that is observed.

6 Robustness Checks

The price effect of the legislation lifting may be especially high if consumers are aware of alcohol again being available at gasoline stations. This would cause a stronger demand shock. Hence, consumer awareness might be essential. Even though consumers might be implicitly steered throughout shops, some consumers actively decide to visit gasoline stations to buy products in the shop. We investigate consumer awareness by studying search queries in *Google Trends*, which documents

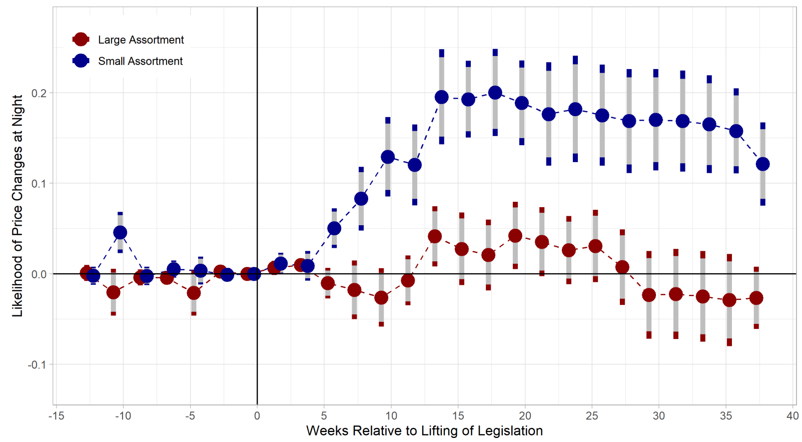

Figure 5: Dynamic Effects on Station Activity: Heterogeneity Along Assortment Variety
This plot gives dynamic estimates of the leads and lags from equation (1) for two subsamples of station with heterogenous store assortment. The outcomes $1[Active\ at\ Night]_{sw}$ is defined as the weekly share on which prices have been changed between 10pm and 5am. Standard errors are heteroskedasticity-robust and clustered on the county level. The exact timing of the beginning and ending of the prohibition is indicated by the black vertical line. We provide 90 and 95% confidence intervals for all coefficients from a linear probability model.

standardized search frequencies for keyword in the search engine. Google searches have been used in previous literature to study policy awareness or agents' behavior as well (Garthwaite et al. 2014, Isphording et al. 2021, Lichter & Schiprowski 2021). Google documents weekly search frequencies for given phrases on the state level. We use this data and estimate a dynamic DID setup with the standardized search frequency across states and over time. We include searches related to the policy, for example, 'Alcohol Selling Prohibition Baden-Wuerttemberg' (in German: Alkoholverkaufsverbot Baden-Württemberg), 'Gasoline Station' (Tankstelle), 'Alcohol Gasoline Station' (Alkohol Tankstelle), 'Gasoline Station Opening Hours' (Tankstelle Öffnungszeiten), 'Baden-Wuerttemberg Alcohol' (Baden-Württemberg Alkohol) and others.

Figure 6 gives the effect of the lifting's announcement - around one week before the actual lifting - on the search frequencies for related keywords in Baden Wuerttemberg relative to the other German states. As can be seen, in Baden Wuerttemberg, the policy receives attention right after the policy announcement and the policy lifting. No anticipatory awareness is evident. The additional search frequency of up to a standard deviation only holds for a few weeks when the search intensity drops to the former level again. This is in line with attention at the time of the shock. This supports consumer awareness of the policy change.

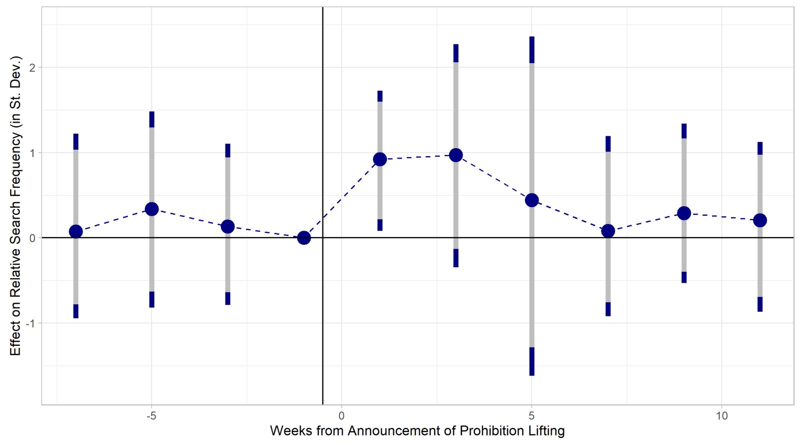

Figure 6: Dynamic Effects on Policy Awareness

This plot gives dynamic estimates of the leads and lags for the discussed DID model. The outcome is the standardized search frequency. Standard errors are clustered on the state level ($n = 16$). We apply the wild-bootstrap inference with 499 repetitions to account for the small number of clusters. The exact timing of the beginning and ending of the prohibition is indicated by the black vertical line. We provide 90 and 95% confidence intervals for all coefficients. $N = 7,360$ observations across 20 weeks, 16 federal states and 23 keywords. We include keyword-times-week and state fixed effects and bin the weekly observations to months.

Beyond studying policy awareness, we implemented further robustness checks. We tested our empirical setup's robustness to including other fixed effects combinations or additional state-level time trends and prices effects near state borders in Tables A.2 and A.3. Our results do not change in the presence of the trends and the different fixed effects allocation. Our border analysis further reveals no price effect at the state border. This fits the ex-ante hypothesis that competition and strategic complementarity between treated and untreated stations lead to a null effect when both types of stations are in near vicinity. We also checked heterogeneity in the treatment effect depending on which region (federal state) of Germany is chosen as a comparison group. Figure A.1 gives the respective estimates and shows that the effect is positive and statistically significant for most of the states as control group. Only, Bavaria and Lower Saxony reveal negative estimates. Hence, the effect is barely sensitive to specific regions of Germany. Moreover, we provide additional evidence on other inference methods for our baseline estimate in Table A.4. In fact, clustering on the county level is a conservative approach as markets are often defined on the granular municipality level (Pennerstorfer et al. 2020) or studies even cluster on the station level (Assad et al. 2021).

7 Conclusion

This paper examines (unintended) spillover effects of a nightly off-premise prohibition for alcohol sales in Baden-Wuerttemberg, Germany. Applying difference-in-differences setups, we find that gasoline prices in Baden-Wuerttemberg increased by around 0.6 Eurocent/l after the lifting of the prohibition ($\approx$ 5% of the net margin). We argue that gasoline stations exploit being 'stores of last resort' for alcohol at night. As opportunity costs of fuelling at a different station from where alcohol is purchased are high, alcohol consumers create a demand shock for stations. The effect size increases in the absence of many competitors and is especially high at stations with small shop assortments.

Implications for policymakers arise. Our analysis shows that gasoline stations rely on multiple income channels and strategically consider their price interactions. Product variety as means of add-on quality is positively priced in gasoline prices. Stations do not cross-subsidize between a transparently priced product (gasoline) and a less transparently priced product (alcohol). These findings have implications for market definition, which - up to now - mostly is limited to gasoline businesses themselves in the literature. Price relations between gasoline and consumables though indicate that competition on shop products (for example with supermarkets) may show price effects at the pump as well. Further evidence on market delineation and spillovers from shop-related regulation on gasoline prices could give new insights to those questions.

Second, our results hint at distributional effects which will arise if consumers are heterogeneously informed. It may even be that commonly applied price transparency regulations, which make gasoline prices more salient, leverage the mismatch of uninformed consumers and high add-on quality stations.

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A Appendix

Table A.1: Edgeworth Cycle Characteristics

	Median Price Change	ln(# Price Changes)	Price Spread
	(1)	(2)	(3)
BW×Post	0.0004	0.0225	0.0018
	(0.0003)	(0.0152)	(00017)
Approach	DID	DID	DID
Observations	2,155,817	2,156,356	2,118,970
Adjusted R^2	0.189	0.753	0.591

Note: *p<0.1; **p<0.05; ***p<0.01. All results are based on OLS regressions with heteroskedasticity-robust standard errors clustered on the county level. The regression setup follows a simple DID.

Table A.2: Robustness Checks: TDID Setup

	Gasoline Price in Euro/l						
	(1)	(2)	(3)	(Baseline)	(5)	(6)	(7)
BW×Night×Post	0.0055**	0.0055**	0.0055**	0.0056**	0.0056**	0.0056**	0.0056**
	(0.0022)	(0.0022)	(0.0022)	(0.0023)	(0.0023)	(0.0023)	(0.0023)
Approach	TDID	TDID	TDID	TDID	TDID	TDID	TDID
BW dummy	✓	×	×	×	×	×	×
Post dummy	✓	✓	×	×	×	×	×
Night dummy	✓	✓	✓	×	×	×	×
BW×Post	✓	✓	✓	✓	×	×	×
BW×Night	✓	✓	✓	✓	✓	×	×
Post×Night	✓	✓	✓	×	×	×	×
Station FE	×	✓	✓	✓	✓	✓	✓
Week FE	×	×	✓	✓	✓	✓	✓
Night×Week FE	×	×	×	✓	✓	✓	✓
BW×Week FE	×	×	×	×	✓	✓	✓
Night×Station FE	×	×	×	×	×	✓	✓
State Trends	×	×	×	×	×	×	✓
Observations	593,193	593,193	593,193	593,193	593,193	593,193	593,193
Adjusted R^2	0.072	0.529	0.868	0.889	0.890	0.912	0.914

Note: *p<0.1; **p<0.05; ***p<0.01. All results are based on OLS regressions with heteroskedasticity-robust standard errors clustered on the county level. The regression setup follows the regression equation from the 'Data and Empirical Strategy' section. The models provide different specifications of a TDID setup.

Table A.3: Robustness Checks: State Border

	Gasoline Price in Euro/l	
	(1)	(2)
BW×Night×Post	−0.0033	−0.0034
	(0.0215)	(0.0077)
Approach	TDID	TDID
Robustness Check	Border (≤ 1km)	Border (≤ 2.5km)
Observations	1,682	7,310
Adjusted R^2	0.874	0.879

Note: *p<0.1; **p<0.05; ***p<0.01. All results are based on OLS regressions with heteroskedasticity-robust standard errors clustered on the county level. The regression setup follows the regression equation from the 'Data and Empirical Strategy' section. We subsample stations near the policy border.

Table A.4: Inference of Baseline Regression

Coefficient Baseline	0.0056
P-Value	
One-Way Clustering	
Station Level (Baseline)	(0.0014)***
County Level (Baseline)	(0.0022)**
Two-Digit Postcode Level	(0.0029)*
Two-Way Clustering	
Station Level + Week	(0.0005)***
County Level + Week	(0.0009)***
Two-Digit Postcode Level + Week	(0.0010)***
Wild Bootstrap (999 rep.)	
Station Level	(0.0015)***
County Level	(0.0028)**
Two-Digit Postcode Level	(0.0029)*
Cluster Size	
N(Stations)	6,144
N(Counties)	401
N(Postcode Areas)	92
N(Week)	52

Note: *p<0.1; **p<0.05; ***p<0.01

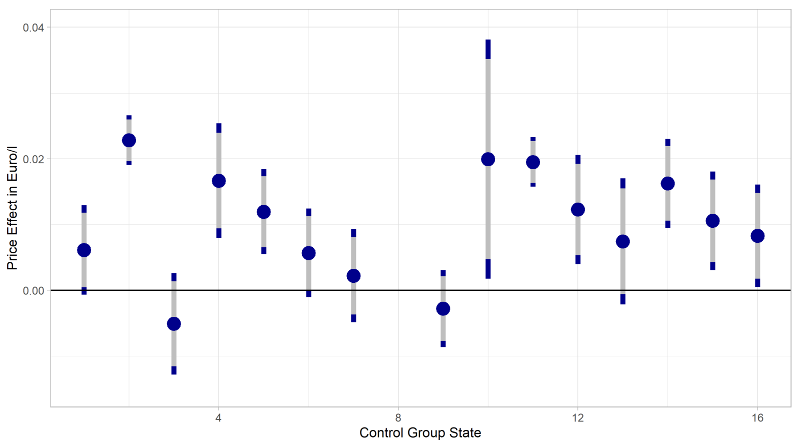

Figure A.1: Treatment Effect for Individual Federal State as Control Group
This plot gives estimates from the triple DiD model presented in the Section 'Data and Empirical Strategy' for different control groups. In particular, each estimate uses a different federal state as control group. Standard errors are heteroskedasticity-robust and clustered on the county level. We provide 90 and 95% confidence intervals for all coefficients. States are as follows: Schleswig-Holstein (1), Hamburg (2), Lower Saxony (3), Hamburg (4), Northrhine-Westphalia (5), Hesse (6), Rhineland-Palatinate (7), Baden Wuerttemberg (8), Bavaria (9), Saarland (10), Berlin (11), Brandenburg (12), Mecklenburg-Hither Pomerania (13), Saxony (14), Saxony-Anhalt (15), Thuringia (16).

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