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THE JOURNAL OF INDUSTRIAL ECONOMICS 0022-1821
Volume LXXII March 2024 No. 1

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Firms often sell a transparent base product and a valuable add-on. 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 competition. Using traffic data, we unveil sizeable consumer-side reactions.

I. 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 and secondary products such as shops, supermarkets, or carwashes (Doyle *et al.* [2010]; Haucap *et al.* [2017a,b]; Wang [2015]; Zimmerman [2012]). However, potential interactions of pricing at the pump and the provision of complementary products have relevant implications for market definition and unveil distributional consequences for heterogeneously informed consumers. If such complementarities distort the signal, that low prices imply the best deal in a homogenous product market like the gasoline market, the matching of consumers, who are uninformed about the availability of complementary products, to suitable stations

*I am thankful for helpful comments by the editor, Ryan McDevitt, two anonymous referees, Katharina Erhardt, Justus Haucap, Paul Heidhues, Ulrich Heimeshoff, Andreas Lichter, Simon Martin, José Luis Moraga-González, Andrea Pozzi, Frank Verboven and Biliana Yontcheva. This article also profited from participants' comments at the DICE PhD Workshop 2021, the Oligo Workshop 2021, the Hohenheimer Oberseminar 2021, and the DICE Empirical IO Group 2022. I gratefully acknowledge funding from the German Research Foundation (DFG) – 235577387/GRK1974. All remaining errors are my own. Open Access funding enabled and organized by Projekt DEAL.

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could deteriorate. Also, common price transparency regulations in gasoline markets, that increase the prominence of stations with cheap gasoline prices, might be misleading then.

Whether the existence of a complementary product raises or lowers gasoline prices—relative to a world without the complement—if only some consumers are aware of the complement, is unclear from an ex-ante perspective. On the one hand, better services or a wider product assortment increase the intrinsic utility of some consumers' shopping. This can cause an outward shift in gasoline demand. Also, consumers will face opportunity costs of traveling if they are not one-stop shoppers but consume gasoline and the complement from different stations. This would explain price increases for gasoline. On the other hand, gasoline stations might use low and transparent gasoline prices as a quasi-loss leader to bait uninformed consumers, who ex-ante do not intend or expect to, in the end, buy additional products in the store. Cross-subsidization could arise (Armstrong and Vickers [2012]; Gabaix and Laibson [2006]; Heidhues *et al.* [2017]; Lal and Matutes [1994]). Less transparently priced complementary products such as add-on services or shop products might then be purchased by consumers at relatively high prices. Therefore, the overall price effect of complementary products on gasoline prices 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 and answer the question of how the introduction of a complementary product affects a firm's price setting for other products. We provide causal evidence by exploiting a unique setting in the gasoline market, where the availability of a complement is exogenously determined by public policy. In particular, we examine a quasi-experiment, the lifting of a local nightly alcohol ban at gasoline stations in a federal state of Germany, as a shifter of complement availability. The prohibition restricted the shop assortment of stations as it mandated sales of alcohol, an important add-on product for gasoline stations, to be forbidden from 10 pm to 5 am. 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.

To analyze the effect of the available add-on on the price of the complementary base product gasoline, we use real-time data of all gasoline prices in the German gasoline market at 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 and, hence, the complementarity on the base product's price. Building on precise information about stations' competitive environment and brand affiliation, we further can investigate heterogeneity across firms.

Our findings and contributions to the literature are threefold. First, we investigate the effect direction of add-on quality on gasoline prices. We find nightly prices of stations affected by the prohibition to increase by 0.6 Eurocent/l—or 5% of the gross margin—after the lifting of the prohibition. Hence, especially consumers who did not buy alcohol profited from the policy when it was in place. Stations with smaller product variety, where alcohol's relative importance for shop revenues is higher, reveal even stronger price effects. Similarly, stations with few competitors nearby increase prices more strongly. Opportunity costs of buying alcohol at another station increase with decreasing competition intensity. Thus, a potential cross-subsidization mechanism is overall outweighed by the intrinsic value of additional services. Using detailed, geo-coded traffic counter data, we provide supporting evidence that traffic increases only in the direct vicinity of gasoline stations after the reintroduction of alcohol sales.

Our findings 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 and Haining [2003]; Haucap *et al.* [2017a]) 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.

Second, while our results address discussions on multi-product competition across most markets, note that the setting studied in this article 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 and Vickers [2012]; Gabaix and 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 that 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 add-on revenues but study the add-on existence at the extensive margin. Hence, our results represent an upper bound for fluctuations of add-on revenues in our setting and are helpful in forming benchmarks for other industries.

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

The remainder of the article is as follows: We start with an explanation of the institutional background and a theoretical motivation in Sections II and III before presenting our data and empirical strategy in Section IV. We then proceed with our analysis in Section V before providing robustness checks and a conclusion in Sections VI and VII.

II. 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 the main nightly off-premise places to go for alcohol (Marcus and Siedler [2015]; Baueml *et al.* [2023]).[1] From 2010 onwards, Baden-Wuerttemberg prohibited nightly alcohol sales from 10 pm to 5 am via the "Alkoholverkaufsverbotsgesetz" (Alcohol Sales Prohibition Law). As most people do not prestore alcohol, the prohibition was binding (Marcus and 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. In the three years after the lifting of the policy, there, though, were only rare occasions, when a municipality implemented an alcohol consumption prohibition–mainly during festivals (Landtag von Baden-Wuerttemberg [2020]).

Its main intentions were the reduction of binge drinking among youths and of indirect spillovers on crime (Baumann *et al.* [2019]; Baueml *et al.* [2023]; Marcus and Siedler [2015]). The policy was effective in several ways indicating a real shock in the volume of alcohol consumed. Up to now, Baueml *et al.* [2023], Marcus and Siedler [2015] and Baumann *et al.* [2019] discussed direct effects on health costs (hospital admissions, doctor visits) and crime for this specific case study. All three papers find that the policy had an economically relevant effect. The number and the length of hospital stays among youth binge drinkers and late-night assaults fell due to the policy. The effect is strongest on young adults since they are more price-sensitive, can hardly pre-store alcohol in their parents' home and are more likely to conduct off-premise pre-drinking (Baueml *et al.* [2023]).

[1] During the prohibition, only stations that 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.

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 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 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, hypothetical back-of-the-envelope calculation based on some assumptions. Following the Statistisches Landesamt Baden-Württemberg [2022], overall annual gasoline and diesel consumption was approximately 7 million tonnes or 9 billion liters in 2017. Admittedly gasoline demand is low at night. But the Federal Cartel Office [2019] documents that still around 5% of all car drivers preferably fuel at night (10 pm to 5 am), which gives a lower bound of the actual demand. This implies that at least around 425 million liters 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 liters per station and year. If a station followed a cross-subsidization strategy that lowers margins by, for example, only half a Eurocent/l, it would lose around 2500 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 shop revenues p.a., of which alcohol products account for approximately a tenth. As alcohol is sold in the 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 bait. In the setting studied in this article, consumers' alcohol demand response to lower gasoline prices is changed from zero to potentially nonzero after lifting the prohibition.

III. THEORETICAL SKETCH

To get a better understanding of the ex-ante ambiguity of the policy's effect on gasoline prices, we consider the differences between a gasoline station's optimization problem before and after the policy lifting. Before the lifting, the station can only sell gasoline. After the lifting, alcohol can be sold in addition.

We model a market in which consumers have heterogeneous preferences for alcohol and differ in whether they anticipate buying alcohol at a gasoline station or not. The model is set up in the following way: on the consumer side, a share of α consumers only want to buy gasoline and no alcohol. λ is the share of informed consumers—among those who do potentially buy alcohol—who are aware of the availability of alcohol products when choosing a gasoline station. $(1-\alpha)(1-\lambda)$ consumers do not consider the existence of alcohol at all. The demand of only-gasoline consumers is given by $D_\alpha\left(p_G^t\right)$ with p_G^t being the gasoline price before $(t=b)$ and after $(t=a)$ the lifting. For consumers who potentially buy alcohol, informed and uninformed consumers' demand is given by $D_{1-\alpha,\lambda}(p_G^t,\gamma_A)$ and $D_{1-\alpha,1-\lambda}(p_G^t)$ with $t\in\{a,b\}$ respectively. $\gamma_A\in\{0,1\}$ indicates whether alcohol is available $(\gamma_A=1)$ or not $(\gamma_A=0)$. Alcohol can only be sold after the lifting. If consumers gain utility from alcohol, then $D_{1-\alpha,\lambda}(\hat{p},1)>D_{1-\alpha,\lambda}(\hat{p},0)\ \forall\,\hat{p}$, that is, alcohol availability causes an outward shift in gasoline demand. For simplicity, we assume (marginal) costs of zero.[2]

We then construct the gasoline station's profit function before (π^b)

$$(1)\qquad \pi^b=p_G^b\left[\underbrace{\alpha D_\alpha\left(p_G^b\right)}_{(1)}+\underbrace{(1-\alpha)\Big(\lambda D_{1-\alpha,\lambda}\left(p_G^b,0\right)}_{(2)}+\underbrace{(1-\lambda)D_{1-\alpha,1-\lambda}\left(p_G^b\right)\Big)}_{(3)}\right]$$

and after the prohibition lifting (π^a)

$$(2)\qquad \pi^a=\left(p_G^a+p_A\right)\left[(1-\alpha)\Big(\lambda D_{1-\alpha,\lambda}\left(p_G^a,1\right)+(1-\lambda)D_{1-\alpha,1-\lambda}\left(p_G^a\right)\Big)\right]+p_G^a\alpha D_\alpha\left(p_G^a\right).$$

Before the prohibition lifting, the station earns the price p_G^b from (1) those consumers who are only willing to buy gasoline and from (2) informed and (3) uninformed consumers who would also buy alcohol if available. After the prohibition lifting, the gasoline station is paid p_G^a by the same three groups. In addition, they earn alcohol revenues from those informed and uninformed willing to buy it. Also, the station faces an outward shift in the demand for gasoline from informed consumers due to the add-on availability.

Maximizing profits and rearranging the first-order conditions yields the policy's price effect.

[2] We also ignore the add-on's price p_A in $D_\lambda(p_G^t,\gamma_A)$ which does not affect the sign of the prohibition's price effect as long as the intrinsic utility from the add-on is sufficiently high.

Result 1. The price effect of the policy, Δp_G, is implicitly given by the expression

$$\Delta p_G = p_G^a - p_G^b = \Delta_{CS} + \Delta_{SQ} \tag{3}$$

$$\text{where } \Delta_{CS} = -\frac{p_A \left[(1-\alpha)\left(\lambda \frac{\partial D_{1-\alpha,\lambda}(p_G^a,1)}{\partial p_G^a} + (1-\lambda)\frac{\partial D_{1-\alpha,1-\lambda}(p_G^a)}{\partial p_G^a}\right)\right]}{\alpha \frac{\partial D_\alpha(p_G^a)}{\partial p_G^a} + (1-\alpha)\left(\lambda \frac{\partial D_{1-\alpha,\lambda}(p_G^a,1)}{\partial p_G^a} + (1-\lambda)\frac{\partial D_{1-\alpha,1-\lambda}(p_G^a)}{\partial p_G^a}\right)},$$

$$\text{and } \Delta_{SQ} = \frac{\alpha D_\alpha(p_G^b) + (1-\alpha)\left(\lambda D_{1-\alpha,\lambda}\left(p_G^b,0\right) + (1-\lambda)D_{1-\alpha,1-\lambda}\left(p_G^b\right)\right)}{\alpha \frac{\partial D_\alpha(p_G^b)}{\partial p_G^a} + (1-\alpha)\left(\lambda \frac{\partial D_{1-\alpha,\lambda}(p_G^b,0)}{\partial p_G^b} + (1-\lambda)\frac{\partial D_{1-\alpha,1-\lambda}(p_G^b)}{\partial p_G^b}\right)}$$
$$- \frac{\alpha D_\alpha(p_G^a) + (1-\alpha)\left(\lambda D_{1-\alpha,\lambda}(p_G^a,1) + (1-\lambda)D_{1-\alpha,1-\lambda}(p_G^a)\right)}{\alpha \frac{\partial D_\alpha(p_G^a)}{\partial p_G^a} + (1-\alpha)\left(\lambda \frac{\partial D_{1-\alpha,\lambda}(p_G^a,1)}{\partial p_G^a} + (1-\lambda)\frac{\partial D_{1-\alpha,1-\lambda}(p_G^a)}{\partial p_G^a}\right)}.$$

The expressions Δ_{CS} and Δ_{SQ} represent the two channels that mainly drive price differences for gasoline before and after the policy lifting: First, prices after the lifting are reduced as stations cross-subsidize between alcohol and gasoline revenues. This is expressed in the first addend of (3), Δ_{CS}, which is negative. This term expresses that per-consumer alcohol revenues p_A are negatively correlated with the gasoline price p_G^a. This *cross-subsidization channel* characterizes gasoline as bait for uninformed consumers. Second, informed consumers increase demand due to the availability of alcohol products. This is expressed in the difference between the two addends of Δ_{SQ}, where alcohol availability ($\gamma_A = 1$) increases demand after the policy change (see nominator). This *service quality channel*, hence, increases demand and prices. Thus, the overall effect on Δp_G is ambiguous.

The model further delivers intuitive predictions on how different parameters mitigate the size of the price effect or determine its sign:

Result 2. The treatment effect Δp_G

- increases in the alcohol-induced demand shift of informed consumers $D_\lambda(p_G^a, 1) - D_\lambda(p_G^a, 0)$,
- is (weakly) negative in perfectly uninformed markets ($\lambda = 0$) and
- vanishes in markets with only gasoline buyers: $\lim_{\alpha \to 1} \Delta p_G = 0$.

Result 2 states the following: If more consumers are aware of the utility gain from the availability of alcohol, this strengthens the demand expansion of the *service quality channel*. This is the case when $D_\lambda(p_G^a, 1) - D_\lambda(p_G^a, 0)$ increases.

The channel will be nonexistent if no consumer is aware of alcohol ($\lambda = 0$). However, the *cross-subsidization channel* is fostered by higher per-consumer alcohol revenues p_A in equilibrium, which can, for example, arise from an outward shift in alcohol demand. Both channels will become irrelevant if consumers only buy gasoline ($\alpha = 1$). We use these predictions to guide our empirical analysis of treatment effect heterogeneity across different types of markets and stations later on. This allows us to better understand whether observed prices support the modeled channels.

Nevertheless, the observed price effects of the policy might not be purely related to the channels discussed above. For example, we assumed that the share of informed consumers λ among potential alcohol consumers and the share of only-gasoline consumers α do not change with the policy lifting. Changes in these variables could rationalize positive as well as negative price effects of the policy lifting beyond the channels we discussed above. A higher share of informed consumers, who want to buy alcohol and gasoline, arrive for alcohol and could be less elastic with respect to the gasoline price. A change in the demand elasticity could explain price changes then. We discuss such other potential mechanisms in our empirical analysis later on to clarify the role of the channels modeled above.

IV. 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 and accessed via `tankerkoenig.de`. 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 (05 am to 10 pm) and nighttime (10 pm to 05 am) 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 construct several variables that, later on, guide our heterogeneity analysis. First, we derive whether stations open all day long (24/7) and operate at night which is reported in the MTU data.[3] Our final sample only consists of such 24/7 stations as other stations do not operate all night, which is the prohibition period.

Second, we use the location data to match stations to municipalities and counties. This allows us to match detailed information on municipality- and

[3] We extract this information from the first fully covered opening hours by the MTU being publically available from January 2019, just three months after the end of our sample period.

county-level variables such as population density, degree of urbanity, or the share of youths in the overall population.

Third, based on stations' brand affiliation, we identify stations' degree of upstream integration and station's brand value. We follow Federal Cartel Office [2011] in classifying stations into oligopolistic and nonoligopolistic stations as well as premium and nonpremium stations. Previous research found that oligopolistic and premium stations tend to be expensive Haucap *et al.* [2017a]. Using these classifications, we can proxy market power and heterogeneity in shop assortments.

Fourth, 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. We differentiate between daytime and nighttime competition measures. Daytime competition includes all stations nearby while nighttime competition is restricted to 24/7 stations as competitors.

We discuss most of the named variables in the descriptive statistics later on.

Finally, we manually identify around 380 highway stations from our sample as those are typically assigned to a separate market (Federal Cartel Office [2011]).[4] They also face §15 Abs. 4 Bundesfernstrassengesetz (FStrG), which prohibits selling alcohol at highway stations from 12 pm to 7 am, 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 6000 24/7 stations of which approximately 13% are located in Baden-Wuerttemberg.

Traffic Counter Data. We, moreover, use novel hourly traffic flow information from around 1700 traffic counters in Germany. This data is publically available from the "Bundesanstalt für Straßenwesen" and allows us to study the reaction of traffic flows in response to the policy. In detail, the data reports the number of cars passing by a certain counter within a specific hour for each day. We also know on which type of road the counters are located. Each counter's location is geo-coded and hence we can calculate the distance to the nearest open station or the federal state's border. While this data does not exactly reflect demand data, it can unveil traffic reactions to the policy and, by that, might help to understand the mechanism behind our findings.

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 daytimes.[5] 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 due to intra-day Edgeworth

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

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

cycles. So, we avoid missing treatment effects pushed out of the nighttime period (e.g., anticipatory alcohol purchases right before 10 pm 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:

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

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 that control for underlying daytime and week trends.[6] $BW_s \times Night_n$ 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. The identical regression approach will be used to study traffic flows later on.

Note that we assign the period after the policy lifting as the treatment period. While the prohibition (pre-lifting) period might also be seen as treatment, we understand the treatment to be the regained availability of alcohol sales at gasoline stations.

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 whether the parallel trend assumption is fulfilled in our setting (see e.g., Olden and Moen [2022]). The setup is as follows:

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

[6] 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.

where $\sum_{t=\underline{\tau},t\neq-1,-2}^{\overline{\tau}} \gamma_t 1[BW_s \times Night_n \times Lifting_{w-t}]$ 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 between treated and untreated stations at the state border, which we will investigate later on.

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.[7] If our treatment effect is positive, we, therefore, do not face problems interpreting results about 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 I offers insights into 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 was 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 brand affiliation or county-specific demand conditions, we are able to address this heterogeneity by, for example, using station fixed effects.

Descriptives on traffic flow data are reported in Table A1 in the appendix. There are 132 counters in Baden-Wuerttemberg and 1554 in other federal states. A median traffic counter is around 3km away from the nearest station, which opens 24/7 and counts around 25,000 (2500) cars during daytime (nighttime) per day. This includes traffic on both sides of the road.

V. RESULTS

Baseline Results. We present our baseline results in Table II. A positive treatment effect would imply prices increase after a prohibition lifting. In this case, the direct quality-price complementarity would outperform the

[7] We later on provide a discussion on the size of the potential downward bias when discussing nighttime market entry of stations in response to the policy lifting.

TABLE I
DESCRIPTIVE STATISTICS

Statistic	Units	Control (Pre-lifting) (1)	BW (Pre-lifting) (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
Margin (Day)	Euro/l	0.094	0.092	0.38
Margin (Night)	Euro/l	0.149	0.145	0.11
1[Active between 10 pm and 5 am]	yes/no	0.863	0.836	0.03**
1[Active between 11 pm and 4 am]	yes/no	0.515	0.472	0.01***
Competition				
# Competitors 0.5 km radius (Day)	#	0.472	0.449	0.56
# Competitors 0.5 km radius (Night)	#	0.257	0.230	0.32
# Competitors 1 km radius (Day)	#	1.081	1.070	0.90
# Competitors 1 km radius (Night)	#	0.546	0.535	0.85
Station characteristics				
Share of youths (18–25-year-old, county level)		0.086	0.096	0.00***
Share of youths (18–25-year-old, municipality level)		0.075	0.082	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

Notes: 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 standard errors clustered at the county level.

TABLE II
(TRIPLE) DIFFERENCE-IN-DIFFERENCES REGRESSION

	Gasoline price in Euro/l			ln(Gross margin)
	(1)	(2)	(3)	(4)
BW × Night × Post	0.0056**			0.0481**
	(0.0022)			(0.0191)
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	593,076
Adjusted R^2	0.889	0.876	0.953	0.784

Notes: All results are based on OLS regressions with standard errors clustered at 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. 0.01% of all observations have a negative margin which we drop in the regression of logged margins in column (4).
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

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. To put this into context, we calculate *gross margins* of gasoline stations.[8] We show that gross margins

[8] We calculate stations' gross margins based on average, daily input costs data. For this, we obtain wholesale prices from the Oil Market Report by Argus Media—a source also used in

increase by around 5%. Note that gross margins still include transportation or variable labor costs, so that *net* margins should be affected even more strongly. Net margins mostly do not exceed two Eurocents/l (Scope Ratings [2019]).

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 as an outcome, which are subject to subtracting, for example, labor costs to arrive at net margins.

A positive effect is indicative of alcohol assortments improving the quality of gasoline stations 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 and Laibson [2006]; Lal and Matutes [1994]). Similarly, Haucap *et al.* [2017b] discussed that carwashes or supermarkets typically offer fuel cheaply. Hence, the mechanism underlying our observations here is likely to be reversed. 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 between 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: First, we apply a dynamic TDID setup in which the treatment effect is split up into several smaller time intervals before and after the treatment.

Assad *et al.* [2023] and Haucap *et al.* [2017a]. Cost data already includes the energy tax. Margins then are given by the VAT-deducted price minus the input costs. The average margin in the sample is 10.6 Eurocent/l which fits survey evidence (Scope Ratings [2018]).

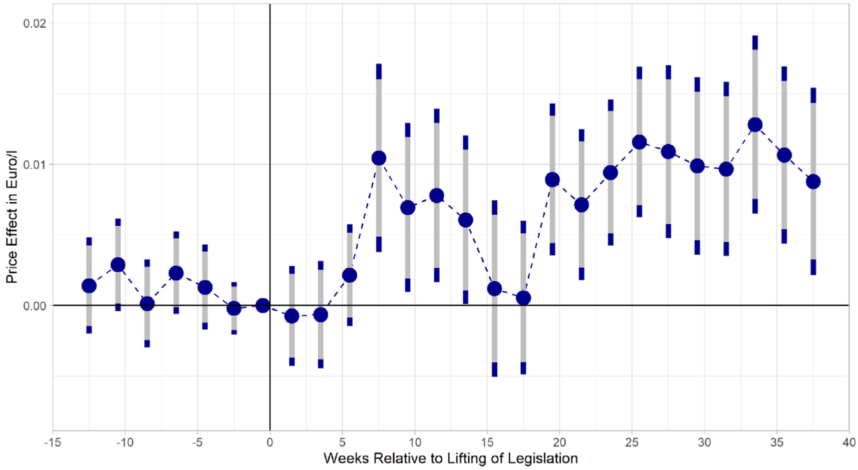

Figure 1
Dynamic Difference-in-Differences Estimates

Notes: 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 end of the prohibition is indicated by the black vertical line. We provide 90 and 95% confidence intervals. Standard errors are clustered at the county level [Colour figure can be viewed at *wileyonlinelibrary.com*]

Figure 1 gives the dynamic estimates from the baseline regression above. As evident, we observe that the significant price drop arises just 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.[9] 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.

Besides showing that the effect only arises after the legislation, we provide evidence beyond models (2) and (3) in Table II 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

[9] 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.

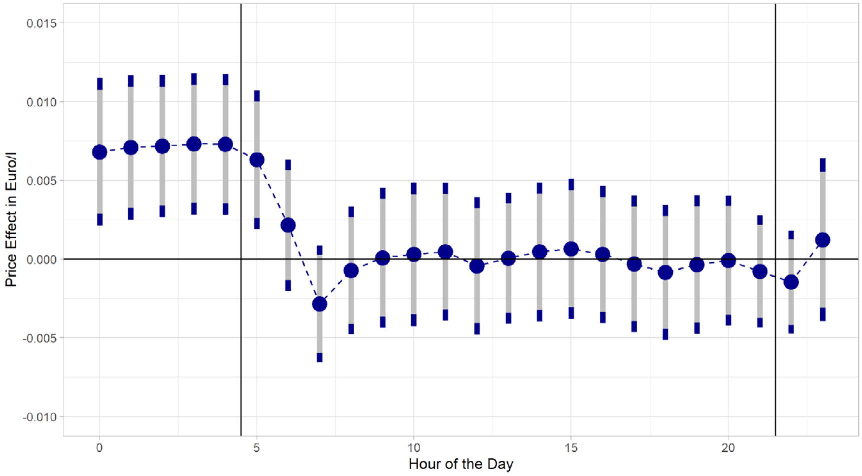

Figure 2
Dynamic Effects by Hour of Day

Notes: 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 end of the prohibition is indicated by the black vertical line. Standard errors are clustered at the county level. We provide 90 and 95% confidence intervals for all coefficients [Colour figure can be viewed at *wileyonlinelibrary.com*]

remaining between 5 am and 6 am. This is likely related to the given timing of the Germany-wide intra-day Edgeworth cycles, where most stations changed prices after 6 am (Federal Cartel Office [2018]).

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.

First, we study competition effects. As described above, our price effect likely originates from the mechanism that alcohol-demanding consumers visit gasoline stations and consume gasoline on the side. Then, the price effect would arise from the opportunity costs of traveling 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 are more likely to be 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 1 km radius.[10] Figure 3 reports our results on heterogeneity analyses. Indeed, in the

[10] Our results also hold for different radii and sample splits not at the median.

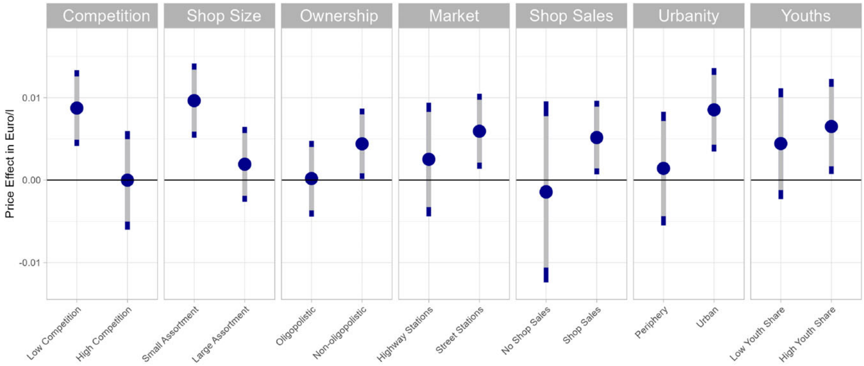

Figure 3
Heterogeneity Analyses: Intensive Margin

Notes: 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 clustered at the county level [Colour figure can be viewed at *wileyonlinelibrary.com*]

first panel of Figure 3, 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.[11] In cities, alcohol consumers may be motorized less often which does not incentivize changes in gasoline prices.

Second, 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 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. At the same time, larger shops imply larger per-consumer revenues from alcohol visitors, which fosters the cross-subsidization effect. Both arguments propose larger shops experience a lower price effect of the policy. In the second panel of Figure 3, we find premium stations with large product variety do 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

[11] This also holds when studying the effect heterogeneity across county differences in the population density.

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.

Third, 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]). Vertically integrated, so-called "oligopolistic" brands are, for example, Shell, Aral (BP), or Total. We study whether the effect differs across oligopolistic and nonoligopolistic brands. We find that especially nonoligopolistic brands increase nightly prices after the prohibition lifting. Our results in the third panel of Figure 3 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.

Fourth, we study a sample of only highway stations in the fourth panel of Figure 3. 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. In terms of our model, both channels are switched off. That is why this analysis might be interpreted as a "quasi-placebo" test. Indeed, at highway stations, no price effect is found.

Fifth, based on stations' names and brand affiliations, we define a group of stations that likely do not sell any alcohol-related products at night, so that the policy should not affect the outcome. In terms of the model in Section III, this reflects a situation where no consumer is interested in alcohol or where alcohol does not give any utility to consumers. For means of econometric power, the respective group of stations pools supermarket stations, unmanned stations and car dealer stations. Supermarket stations most of the time do not have a shop at all as they typically are owned by the supermarket nearby (Haucap *et al.* [2017b]). Though, supermarkets are closed during the nightly prohibition (10 pm to 5 am), so that supermarket stations are not affected by the policy lifting. Unmanned gasoline stations (e.g., by the brand AVIA Xpress) do not operate a shop (at night). Similarly, car dealer stations' main purpose is to provide fuel for the main business. As expected, we find that such stations, indeed, do not change prices in response to the policy (see fifth panel of Figure 3).

Sixth, we investigate whether the price effect is mitigated by the fact whether stations are located in urban counties or the periphery. We follow the county-level definition of urbanity by *Federal Institute for Research on Building, Urban Affairs and Spatial Development*. We find stations in urban vicinities to increase prices more strongly (see sixth panel of Figure 3). This likely reflects the higher share of youths in urban regions, which Marcus and Siedler [2015] found to increase their alcohol demand. Hence, in urban stations, more consumers should receive utility from the newly available product after the prohibition lifting.

In the last panel of Figure 3, we study heterogeneity in the local share of youths (18–25-year-olds). This traces back to Marcus and 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. With regard to the model, youths might reflect a consumer group, who is aware of the alcohol product (high λ in the model above). As they gain most from the availability of alcohol, the alcohol-driven demand shift should cause higher prices for stations with a high local share of youths (see Result 2 in Section III). Our estimates do not reveal a clear treatment effect heterogeneity when comparing stations from municipalities above and below the median youth share. Though, when zooming in on the heterogeneity of the youth share more intensively, a clear relation between a higher youth share and a higher treatment effect is evident. For example, see Figure A1 in the appendix for treatment effect heterogeneity across terciles and quartiles of the distribution.

Note that we, as a robustness check, also ran our heterogeneity analysis in a single regression instead of separate regressions. This should ensure that the different heterogeneity results are not driven by one and the same factor which correlates with several station characteristics. Table A2 in the appendix presents these results. Qualitatively our results do not change. Especially stations with few competitors at night and in municipalities with a high share of youths experience higher treatment effects. Also, small assortment stations increase prices more strongly.

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 the 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 in response to the policy lifting.

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, in the appendix, we provide some evidence in Table A3 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.

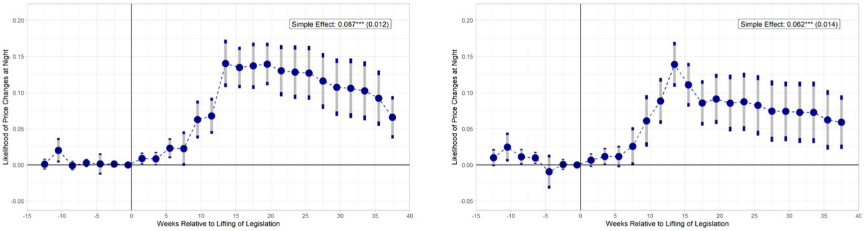

Figure 4
Dynamic Effects on Likelihood to be Active at Night

Notes: This plot gives dynamic estimates of the leads and lags from equation (4). The left plot defines $1[Active\ at\ Night]_{sw}$ with changing prices between 10 pm and 5 am, the right plot takes a more restricting definition of price changes between 11 pm and 4 am. Standard errors are clustered at the county level. The exact timing of the beginning and end 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 [Colour figure can be viewed at *wileyonlinelibrary.com*]

We determine whether a station has changed its price between 10 pm and 5 am and, for a second measure, whether there have been changes between 11 pm and 4 am. 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:

$$(4) \quad 1[Active\ at\ Night]_{sw} = \alpha_s + \lambda_w + \sum_{t=\underline{\tau}, t\neq -1,-2}^{\overline{\tau}} \gamma_t 1[BW_s \times Lifting_{w-t}] + \epsilon_{sw}.$$

$1[Active\ at\ Night]_{sw}$ 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: First, the variable will turn one if a station is active/changes the price at least once a week between 10 pm and 5 am. Second, the variable will turn one if a station is active/changes the price between 11 pm and 4 am 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 at some point between 10 pm and 5 am 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 fewer products,

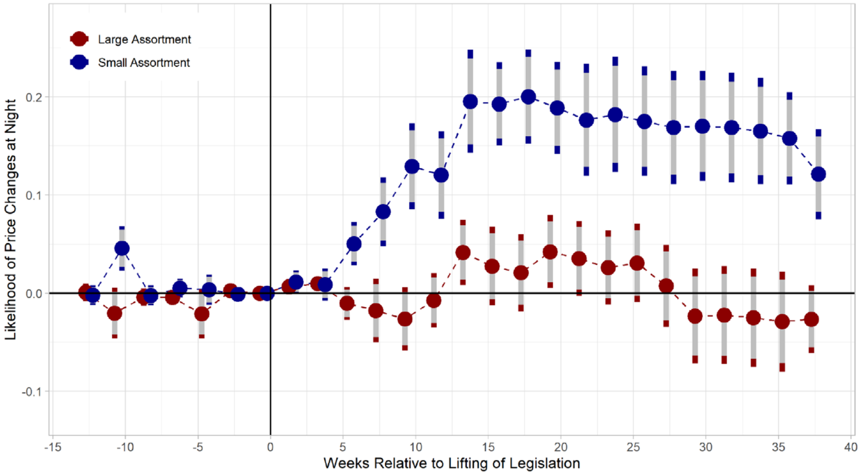

Figure 5

Dynamic Effects on Station Activity: Heterogeneity Along Assortment Variety

Notes: This plot gives dynamic estimates of the leads and lags from equation (4) for two subsamples of stations with heterogeneous store assortment. The outcomes $1[Active\ at\ Night]_{sw}$ is defined as the weekly share on which prices have been changed between 10 pm and 5 am. Standard errors are clustered at the county level. The exact timing of the beginning and end 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 [Colour figure can be viewed at *wileyonlinelibrary.com*]

so that a restriction on alcohol might hit them more strongly. Indeed, we find that such stations react more pronouncedly in activity during prohibition hours (see 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.

A concern is that price changes might not perfectly reflect opening hours. For example, stations that open at night but only start to change prices after the prohibition lifting, are implicitly understood to extend opening hours due to the policy. Hence, this would likely upward bias the estimated treatment effect. Therefore, we provide additional robustness checks on the effect of opening hours (see Section C in the Appendix for an in-depth analysis). Using historical opening hours for a subset of gasoline stations (≈25% of all stations), which we obtained from the internet archive `web.archive.org`, we find opening hour reactions in line with our results above. Again opening hours are increased significantly in Baden-Wuerttemberg after the policy lifting—especially at stations with smaller shop assortment. Though, our robustness check identifies smaller treatment effects. This is likely due to the potential upward bias in the analysis based on price changes as explained above.

Finally, the extended opening hours likely cause our baseline price effect to be downward-biased as more competitors have been found to correlate with lower prices in gasoline markets (Haucap *et al.* [2017a]; Martin [2023]; Pennerstorfer *et al.* [2020]). In the appendix, we show how a change in the number of nighttime competitors affects nighttime prices. Table A4 reports the results of an interaction term analysis in columns (1) and (2). We find that the treatment effect is larger in concentrated markets. In Figure A3, we exploit the staggered timing of competitors' nighttime entry across incumbents and show that nighttime entry in a 1km radius decreases prices by up to 1 Eurocent/l.[12] The effect size is very similar to Fischer *et al.* [2023] who estimate the causal effect of station entry on incumbent prices to be around 0.5ct/l. This indicates, that, indeed, our baseline results are downward-biased.

As nighttime entry decreases prices by up to 1 Eurocent/l, it absorbs the policy-induced price effect completely in markets where nighttime entry takes place. However, nighttime entry is costly and might not be possible or profitable in all markets, so that positive price effects remain in the majority of markets, which are not entered. This leads to the on average positive price effect of the policy found above. In columns (3) and (4) of Table A4, we try to quantify by how much nighttime entry decreases the price effect which would have been observed absent nighttime entry. For this, we include the entry of competitors as "bad control" in the price regressions. We show that the price effect changes only slightly in comparison to the estimated baseline effect. This indicates that entry only marginally decreases the policy's average price effect.

Traffic Flow Analysis. To better understand the mechanism underlying the observed price effects, we study traffic flow reactions to the policy. The analysis is twofold: First, we analyze whether nightly traffic increases in response to the policy lifting in Baden-Wuerttemberg and especially near open gasoline stations. Secondly, we study how traffic at the federal state's border is affected by the shock.

We start by running the triple difference-in-differences regression from above on the logged number of counted cars for traffic counters near gasoline stations open at night ($\leq$ 2km linear distance). Figure 6's blue estimates report the dynamic effect of the policy lifting on traffic near gasoline stations in Baden-Wuerttemberg in four-week bins. After the policy lifting, nightly traffic in Baden-Wuerttemberg persistently increases by up to 5%–10%. This is indicative of more cars traveling near and, hence, likely also to gasoline stations. This is in line with a demand expansion through the *service quality* channel as alcohol is available after the policy. To show that this effect really reflects an increasing interest in gasoline stations, we run this analysis

[12] Similar procedures can be found in the reduced-form entry literature as in Arcidiacono *et al.* [2020], Goolsbee and Syverson [2008] and Matsa [2011].

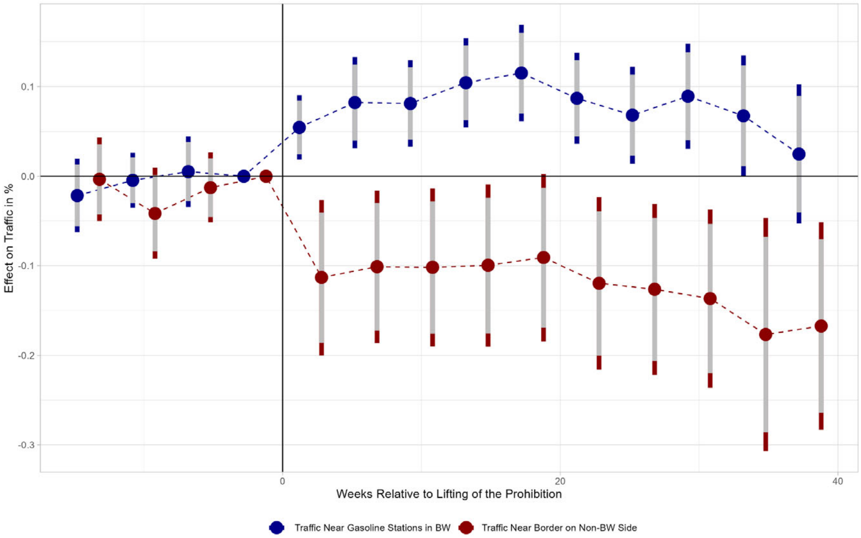

Figure 6
Dynamic Effects on Traffic Flows

Notes: This plot gives dynamic estimates of the leads and lags from equation (4) where the outcome variable is logged traffic flows. The blue estimates give the effect of the policy on traffic counts in Baden-Wuerttemberg near gasoline stations (≤ 2km linear distance) in a subsample of traffic counters of maximum 2km linear distance to gasoline stations open at night. The red estimates give the effect of the policy of traffic counts near the border (≤ 2km linear distance) to Baden-Wuerttemberg at non-Baden-Wuerttemberg counters in a subsample of non-Baden-Wuerttemberg traffic counters. To account for the logarithm of very few zero traffic observations, we use the hyperbolic sine transformation of the outcome variable. Standard errors are clustered at the county level. The exact timing of the beginning and end of the prohibition is indicated by the black vertical line. We provide 90 and 95% confidence intervals for all coefficients [Colour figure can be viewed at *wileyonlinelibrary.com*]

separately for groups of traffic counters that have different distances to the nearest open gasoline station. The traffic effect should be highest for counters near gasoline stations if traffic increases really relate to more visits to gasoline stations. Figure 7, indeed, shows that this is the case. While traffic in Baden-Wuerttemberg overall increases by around 5%, this effect is strongest for counters right next to gasoline stations (≤1 km linear distance). There is no significant effect on traffic flows for counters more than 2 km away from open gasoline stations. We take this as support for our demand expansion channel.

In addition, Figure A2 in the appendix reveals that the increase in traffic is especially high for traffic counters in municipalities with a high share of youths. This fits the story that especially youths respond to the policy change.

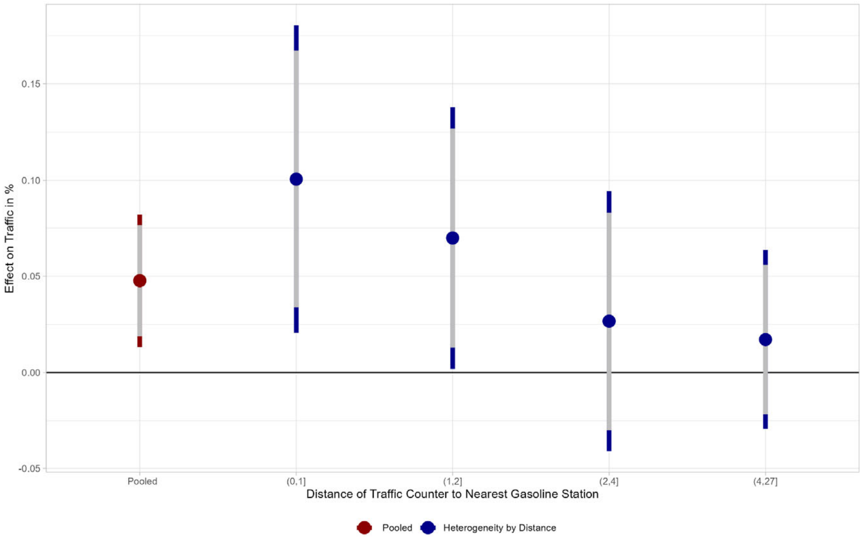

Figure 7

Effects on Traffic Flow by Counter Distance to Station

Notes: This plot gives the estimates from the static version of the equation (4) where the outcome variable is logged traffic flows. Stations are grouped by the minimum distance to a gasoline station open at night. To account for the logarithm of very few zero traffic observations, we use the hyperbolic sine transformation of the outcome variable. Standard errors are clustered at the county level. The exact timing of the beginning and end of the prohibition is indicated by the black vertical line. We provide 90 and 95% confidence intervals for all coefficients [Colour figure can be viewed at *wileyonlinelibrary.com*]

We further study border traffic. Before the policy lifting, consumers living in Baden-Wuerttemberg had to leave the federal state to get alcohol at night at off-premise locations. This border traffic should have been reduced after the policy lifting. For this, we compare traffic at traffic counters outside of Baden-Wuerttemberg but near the border ($\leq$2 km linear distance) to all other non-Baden-Wuerttemberg traffic counters before and after the policy. Figure 6's red estimates report the results of the triple difference-in-differences regression. Indeed, traffic near the border to Baden-Wuerttemberg but outside of Baden-Wuerttemberg falls in response to the policy. This can be interpreted as a demand shift to gasoline stations in Baden-Wuerttemberg. Also, this result indicates that alcohol consumption has a sufficiently high value to consumers to induce border travel. Note that we also tried out other distance thresholds up to 5 km distance to the border and our results qualitatively remain the same.

We, further, reproduce the heterogeneity analysis from Figure 3 with traffic flows as an outcome to support the mechanisms described above. To conduct heterogeneity analyses along gasoline station characteristics (brand, shop size, etc.), we match counters to the nearest station. The results in Figure A4 in the appendix show that traffic increases more strongly at counters with many stations nearby and also is stronger in urban areas with a high youth share.

We complement the traffic data results on a demand expansion mechanism with an analysis of geo-coded traffic accidents with personal damage in Germany.[13] In Table A5, we, at the extensive margin, do not find an effect of the policy lifting on the overall number of accidents with personal damage in Baden-Wuerttemberg.[14] However, we show that the likelihood of accidents being very near (≤1 km) to open gasoline stations increases by 3% after the policy lifting. On average, the distance of accidents to the nearest gasoline station at night decreases by 7% after the policy lifting. This shows that traffic flows likely shift toward areas surrounding gasoline stations.

Bite of the Policy. To quantify the consequences of the policy for gas stations as well as consumers, it is not sufficient to show that the price effect is around 5% of an average station's margin. We need to understand how many consumers visit gasoline stations at night. To approximate *daytime-specific* demand, we rely on *Google Popularity* data,[15] which we scraped for all stations available once in July 2019 (≈ 85% of all German stations). Figure 8 plots the average distribution of gas station visits over the course of the day. Non-negligible 7%–8% of visits lie in the treatment time between 10 pm and 5 am.[16]

Moreover, stations do not only use revenues from gasoline sales during the prohibition but also lose alcohol revenues. Industry surveys (Scope Ratings [2018]) show that the annual alcohol revenues of an average station are approximately 100,000 Euro.

Furthermore, consumers potentially switching away from stations, which increase prices more strongly after policy lifting, are a concern when discussing the exposure of consumers to the policy. Especially informed consumers would not be affected by the policy then and distributional implications would arise. While we do not observe actual transactions—so where consumers fuel—we can show that consumers can hardly avoid being affected by the policy effect as long as the policy shifts the complete price

[13] The data comes from the "Unfallatlas" (*https://unfallatlas.statistikportal.de/*) of the Federal Statistical Office and the Statistical Offices of the German States and covers traffic accidents with personal damage for 12 out of 16 federal states.

[14] This is in line with the results in Baueml *et al.* [2023] who do not find the policy's introduction in 2010 to affect alcohol-related traffic accidents.

[15] On *Google Maps*, it is reported how crowded and popular a business is for every hour of the day. Popularity is based on measures such as mobile phone mobility and traffic and is reported in an index between 0 and 100 at the station level.

[16] In a telephone survey of the German Ministry for Economic Affairs and Energy from 2016, the share of respondents who fuel at this time is of similar magnitude (Bundesregierung [2018]).

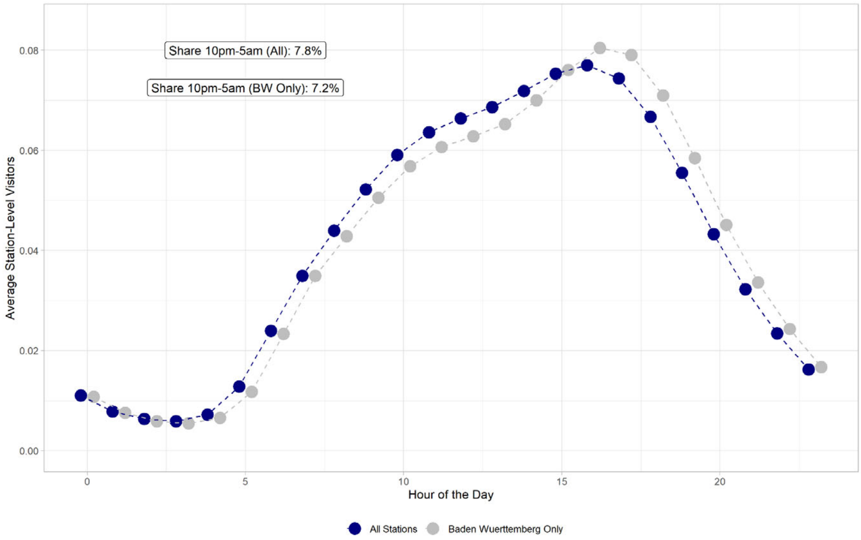

Figure 8

Average Share of *Google Popularity* Across Stations

Notes: This plot gives the distribution of *Google Popularity* as a proxy of demand over the course of the day. This is separately reported for all stations in Germany and Baden-Wuerttemberg only. As in the rest of the article, we only consider those stations which open 24/7, that is, those for which popularity data is available for all hours of the week [Colour figure can be viewed at *wileyonlinelibrary.com*]

distribution in a first-order stochastic dominance manner. Then, consumers at all percentiles of the distribution are affected.

In Appendix B, using the method of Chernozhukov *et al.* [2013], we show that the prohibition lifting indeed shifts the price distribution in a first-order stochastic manner. As prices at all quantiles increase, consumers can hardly avoid the exposure to the policy's price effect.

Other Mechanisms. While we argue for a trade-off between a *service quality*-induced demand expansion channel and a *cross-subsidization* channel, other policy-induced changes in consumer or firm behavior could potentially explain the observed price effects. Examples are a policy-induced change in consumer information about prices or the demand elasticity. We discuss relevant, alternative mechanisms subsequently to justify that they do not drive the findings.

It is a concern that the composition of consumers changes in response to the treatment. First, some consumers might only visit stations to buy alcohol and do not anticipate buying gasoline. These consumers do not compare gasoline prices in advance and, hence, the declining relevance of competition nearby might rationalize the observed price increase. Though, this then should only bite in less concentrated markets. To the contrary, our heterogeneity analysis in Figure 3 reveals the opposite as the price effect is especially driven by stations with few competitors.

Second, the share of informed consumers might change with the treatment. For example, fewer people might compare gasoline prices as some consumers mainly come for alcohol and do not anticipate buying gasoline. This could result in increasing nighttime prices, too. In this case, the share of consumers, which do not consider gasoline prices at competing stations, increases. Such consumers behave like nonshoppers in Varian [1980]. While we have no explicit information about changes in the information structure of consumers, we can test in the data whether the effect of a change in consumer information on market-level prices reflects the theoretical predictions in Varian [1980]. We do this in Section D of the appendix in very detail and show that observed changes in the estimated upper bound of the market-level, monthly price distribution in response to the treatment suggest a change in the observed valuation of a purchase instead of a change in the share of informed consumers. We interpret this in favor of the outlined *service quality* channel.

We, moreover, test whether consumer frictions at night develop differently in Baden-Wuerttemberg after the policy lifting. For this, we extend the rank reversal test in Chandra and Tappata [2011] to a difference-in-differences setup. The test's intuition is that rank reversals of a station couple's prices over time in a homogenous product market arise from consumer frictions. We do not find that rank reversals become more likely in Baden-Wuerttemberg after the policy lifting. This contradicts the argument of changing consumer information in response to the policy. See Section E for more precise explanations and results of this analysis.

Third, nighttime consumers might be less price-sensitive and less price-elastic with respect to the gasoline price after the policy lifting–beyond the discussed changes in information and competition relevance above. A less elastic cross-price elasticity would imply that the relevance of competitor prices decreases. In the most extreme case, stations become quasi-monopolies and prices of neighboring stations are not strategic responses to each other anymore. Hence, a less elastic cross-price elasticity likely results in a weaker price comovement. We empirically investigate whether price comovement of neighboring stations changes with the policy lifting by comparing the correlation of prices between couples in Baden-Wuerttemberg and other states over time in a difference-in-differences setup. Section F of the appendix gives a detailed explanation of the empirical analysis and the results. We do not find any evidence for a change in the strength of price comovement (see Table F1). Hence, we interpret this as evidence for no change in the demand elasticity as the driving mechanism behind the observed price effects.

Fourth, the treatment might have affected Edgeworth cycle characteristics in Baden-Wuerttemberg which could explain the observed price effects. For example, the policy-induced market entry at night might change the cycle structure as other papers have found the competition to shape cycles (Noel [2007]; Siekmann [2017]). Table A3 shows that there are no significant changes in typical cycle characteristics related to the policy lifting.

Fifth, one might argue that alcohol could also be the bait for gasoline, which would explain why introduction of alcohol sales increases gasoline prices. Though, this is unlikely for three reasons: First, the share of consumers not considering to fuel while traveling to a gasoline station for alcohol likely is low. Hence, most consumers willing to fuel will account for gasoline prices. This effectively limits the potential for gasoline price increases. Second, high price transparency for gasoline through price apps, websites, and price signs in front of gasoline stations limits the extent to which firms can change the add-on price. Pure gasoline consumers would then switch away. Lastly, the prohibition hours at night lie in the time period of the day in which intra-day gasoline price cycles peaked in Germany at this time. Hence, consumers can become better off by avoiding high add-on prices by switching intertemporally. This is not the case for alcohol prices, which do not vary between daytimes.

Lastly, our baseline, reduced-form results leave it open whether the dominated *cross-subsidization* channel exists at all. However, our heterogeneity analysis for shops with a smaller and larger assortment reveals smaller price effects for larger shops. This likely reflects the higher incentive to cross-subsidize for shops with larger assortments.

VI. 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 could reflect a stronger demand shock. Hence, consumer awareness might be essential. Even though consumers might be implicitly steered through shops, some consumers actively decide to visit gasoline stations to buy products in the shop. While Baueml *et al.* [2023] provide survey evidence that people were aware of the prohibition, there is no evidence on the familiarity with the policy lifting. We investigate consumer awareness by studying search queries in *Google Trends*, which documents standardized search frequencies for keywords 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 and Schiprowski [2021]). Google documents weekly search frequencies for given phrases at the state level. We gather time series of search frequencies for 23 policy-related keywords through the API of the *R* package *gtrends* at the keyword-state level and estimate a dynamic DID setup with the standardized search frequency across states and over time. The respective regression is as follows:

$$(5) \qquad Search_{kfw} = \theta_f + \eta_{kw} + \sum_{t=\underline{\tau},t\neq-1}^{\overline{\tau}} \phi_t 1[BW_f \times Lifting_{w-t}] + \epsilon_{kfw},$$

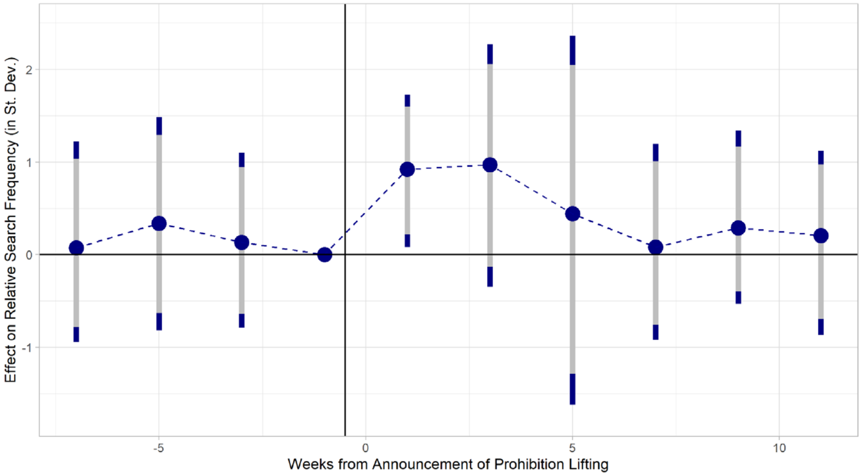

Figure 9
Dynamic Effects on Policy Awareness

Notes: This plot gives dynamic estimates of the leads and lags for the DID model in equation (5). The outcome is the standardized search frequency. Standard errors are clustered at 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 end of the prohibition is indicated by the black vertical line. We provide 90 and 95% confidence intervals for all coefficients. N = 7360 observations across 20 weeks, 16 federal states, and 23 keywords [Colour figure can be viewed at *wileyonlinelibrary.com*]

where $Search_{kfw}$ is the standardized search frequency for keyword k in federal state s and week w. θ_f and η_{kw} are state and keyword-week fixed effects, so that identification stems from within-keyword changes in the search frequency over time. $\sum_{t=\underline{\tau},t\neq-1}^{\overline{\tau}} \phi_t 1[BW_f \times Lifting_{w-t}]$ are dummies which will be one if f =Baden-Wuerttemberg and if the prohibition's lifting is t periods ago. Hence, ϕ_t are the coefficients of interest and document whether there have been more or fewer search frequencies in comparison to the control states relative to one period before the treatment.

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 9 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.

Beyond studying policy awareness, we implemented further robustness checks. First, we combined our difference-in-differences regression with a propensity score matching to eliminate differences across treatment and control stations in observable characteristics. We matched treated stations to control stations within the last pre-treatment period. We match treated stations to their nearest neighbor without replacement. We drop treated stations that did not set a price in the pre-treatment week (≈10%) and arrive at 733 station pairs. Tables A6 and A7 show that the sample is balanced after matching and that our regression results do not change qualitatively.

We, further, tested our empirical setup's robustness to including other fixed effects combinations or additional state-level time trends and price effects near state borders in Tables A8 and A9. 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 the 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 A5 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 A10. In fact, clustering at the county level is a conservative approach as markets are often defined on the granular municipality level (Pennerstorfer *et al.* [2020]) or studies cluster at the market level (Assad *et al.* [2023]).

Lastly, we study how the treatment effect varies when changing the pre- and post-treatment effect window. Table A11 shows that the treatment effect is quite robust across different window choices.

VII. CONCLUSION

This article 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 (≈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 revenue 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.

APPENDIX

Table A1
Descriptive Statistics: Traffic

		Control	BW	Δ
		(Pre-Lifting)		(p-value)
Statistic	Units	(1)	(2)	(3)
Outcomes				
ln(Traffic (Day))	#	11.902	11.872	0.81
ln(Traffic (Night))	#	9.527	9.584	0.71
Location				
Distance to station open at night	km	3.802	3.175	0.06*
ln(distance to state border)	#	4.972	3.712	0.00***

Notes: This table compares descriptive statistics of counters in Baden-Wuerttemberg with counters outside of Baden-Wuerttemberg (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 standard errors clustered at the county level.

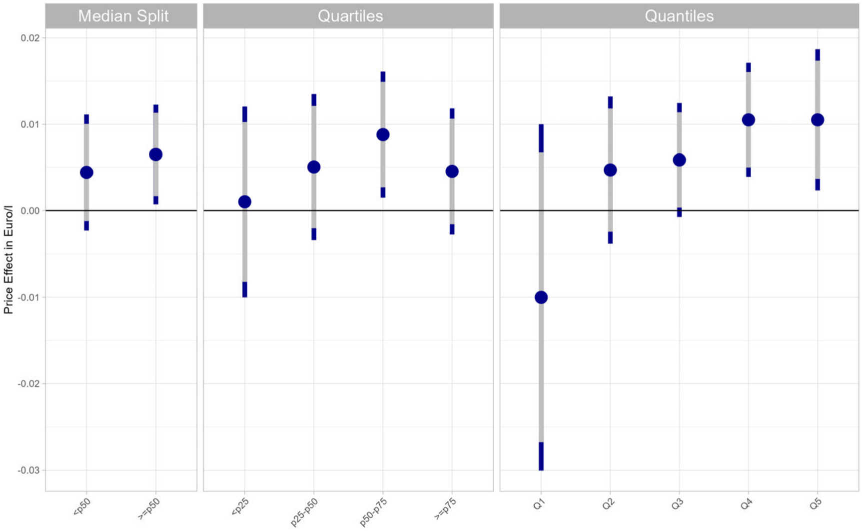

Figure A1
Price Effect: Heterogeneity Along "Youth Share" Distribution

Notes: Heterogeneity analysis based on sample splits along the distribution of the variable "Youth Share". 90% and 95% confidence bands are reported. Standard errors are clustered at the county level [Colour figure can be viewed at *wileyonlinelibrary.com*]

Table A2
Heterogeneity Analysis: Robustness Check

	Gasoline price in Euro/l	
	(1)	(2)
BW × Post × 1[Street Station]	−0.0064	
	(0.0079)	
BW × Post × 1[Urban]	0.0054	
	(0.0051)	
BW × Post × 1[Below median competition]	0.0064***	
	(0.0024)	
BW × Post × 1[Below median youth share]	−0.0048*	
	(0.0027)	
BW × Post × 1[Large assortment]	−0.0127***	
	(0.0046)	
BW × Post × 1[Oligopolistic]	−0.0024	
	(0.0042)	
BW × Post × 1[No shop sales]	−0.0102*	
	(0.0062)	
BW × Post × Night × 1[Street station]		−0.0059
		(0.0076)
BW × Post × Night × 1[Urban]		0.0056
		(0.0050)

TABLE A2
Continued

	Gasoline price in Euro/l	
	(1)	(2)
BW × Post × Night × 1[Below median competition]		0.0063**
		(0.0024)
BW × Post × Night × 1[Below median youth share]		−0.0045*
		(0.0026)
BW × Post × Night × 1[Large assortment]		−0.0131***
		(0.0046)
BW × Post × Night × 1[Oligopolistic]		−0.0024
		(0.0042)
BW × Post × Night × 1[No shop sales]		−0.0110*
		(0.0062)
Sample	Night prices	All prices
Observations	296,598	593,193
Adjusted R^2	0.894	0.911

Notes: All results are based on OLS regressions with standard errors clustered at the county level. The regression setup extends the regression equation from the "Data and Empirical Strategy" section by additional interactions. Model (1) only uses night prices and a triple difference-in-differences estimator, while model (2) uses quadruple interactions to extend the baseline triple difference-in-differences estimator to account for effect heterogeneity. Other interactions not reported in the regression table.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

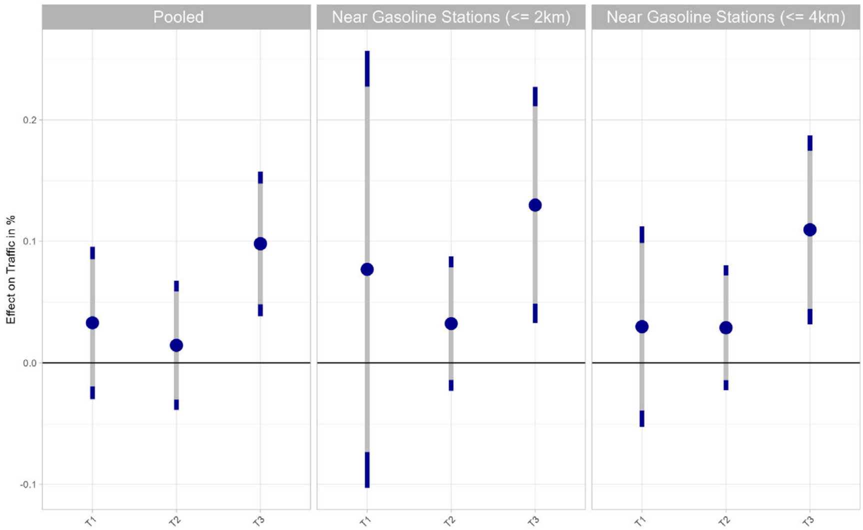

Figure A2
Heterogeneity in Traffic Response Along Youth Share Distribution

This plot gives the estimates from the triple DiD model presented in the Section "Data and Empirical Strategy" with trafficflows as outcome. The analysis is run for all stations, only stations in a radius of 2 km linear distance to gasoline stations open at night or a 4 km radius. 90% and 95% confidence bands are reported. Standard errors are clustered at the county level *Notes:* [Colour figure can be viewed at wileyonlinelibrary.com]

TABLE A3
EDGEWORTH CYCLE CHARACTERISTICS

	Median price change (1)	ln(# Price Changes) (2)	Price spread (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

Notes: All results are based on OLS regressions with standard errors clustered at the county level. The regression setup follows a simple DID.
$^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

TABLE A4
REGRESSIONS ON MITIGATING ENTRY EFFECT

	Gasoline price in Euro/l			
	(1)	(2)	(3)	(4)
BW × Post	0.0097***		0.0082***	
	(0.0027)		(0.0024)	
BW × Post × (# Competitors ∈ [0,1] km)	−0.0043***	−0.0039**		
	(0.0015)	(0.0015)		
BW × Post × (# Competitors ∈ (1,2] km)	−0.0004	−0.0002		
	(0.0011)	(0.0011)		
# New competitors active ∈ [0,1] km			−0.0101***	−0.0091***
			(0.0033)	(0.0031)
# New competitors active ∈ (1,2] km			−0.0042	−0.0033
			(0.0028)	(0.0028)
Station FE	✓	✓	✓	✓
Week FE	✓	×	✓	×
State × Week FE	×	✓	×	✓
Observations	296,598	296,598	296,598	296,598
Adjusted R^2	0.878	0.882	0.876	0.880

Notes: All results are based on OLS regressions with standard errors clustered at the county level. The regression equation is a triple difference-in-differences regression for nighttime prices comparing prices across federal states, before and after the policy and across competition environments. The other interaction terms of triple difference-in-differences estimator in columns (1) and (2) are omitted.
$^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

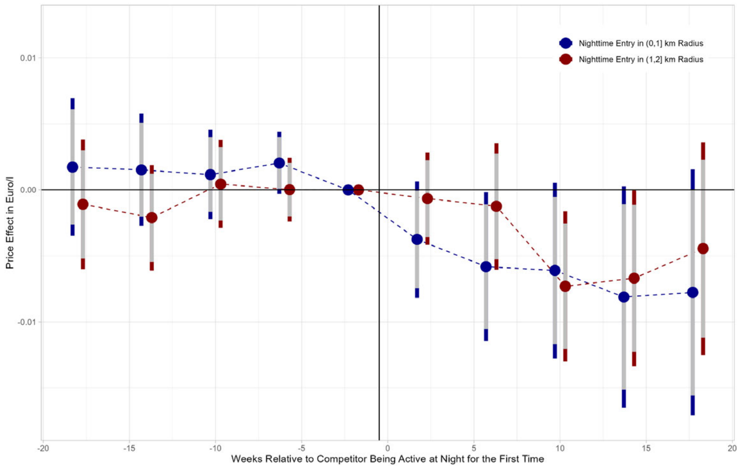

Figure A3

Effects of Nighttime Entry on Prices

Notes: This plot gives estimates from an event study regression of nightly prices on leads and lags of the nighttime entry of competitors in a 1 km or 1–2 km radius around incumbents. Station and state-week fixed effects are included. Nighttime entry of stations is identified in the week after which a station operates two consecutives weeks at night for the first time. Endpoints are binned and not reported due to an unbalanced panel in event time (Fuest *et al.* [2018]). Standard errors are clustered at the county level. The exact timing of entry is indicated by the black vertical line. We provide 90% and 95% confidence intervals for all coefficients [Colour figure can be viewed at *wileyonlinelibrary.com*]

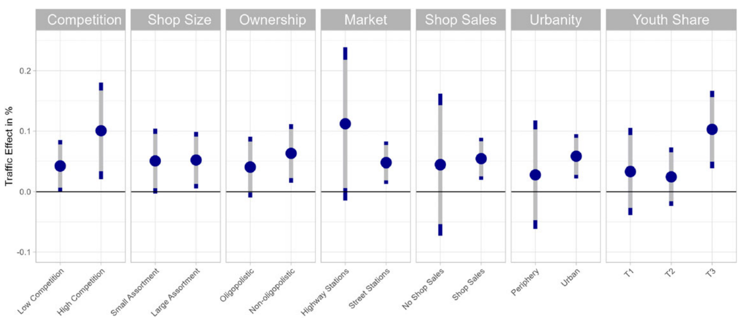

Figure A4

Heterogeneity Analyses: Traffic

Notes: This plot gives the treatment effect of the policy on traffic flows for subsamples along counter and station characteristics. The y-axis documents the effect size in %, the x-axis gives the respective subsample. 90% and 95% confidence bands are reported. Standard errors are clustered at the county level. To be able to conduct heterogeneity analyses along station characteristics, we match counters to the nearest stations operating 24/7. We only include counters in the analyses that are closer than 5km to a gasoline [Colour figure can be viewed at *wileyonlinelibrary.com*]

TABLE A5
POLICY LIFTING'S EFFECT ON ACCIDENTS

	ln(# Accidents)			1[Distance Station ≤ 1]			log(Distance Station)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BW × Post × Night	−0.017 (0.022)			0.031** (0.015)			−0.069** (0.035)		
BW × Post		−0.027 (0.023)	−0.043 (0.030)		0.004 (0.004)	0.036** (0.014)		−0.003 (0.010)	−0.069** (0.034)
County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month × Hour FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Approach	TDID	Only Day	Only Night	TDID	Only Day	Only Night	TDID	Only Day	Only Night
Observations	182,016	128,928	53,088	393,445	368,544	24,901	393,445	368,544	24,901
Adjusted R^2	0.512	0.426	0.241	0.170	0.158	0.187	0.169	0.148	0.273

Notes: Regressions (4) to (9) are at the individual accident-level and hence also include controls for whether the accident included bicycles, motorbikes and pedestrians. Regressions (1) to (3) are at the county-month-hour level. The distance to the nearest *open* station depends on the time of the day. The post-treatment period is the first full month after the policy lifting and beyond. Standard errors are clustered at the county level.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

TABLE A6
ROBUSTNESS CHECKS: PROPENSITY SCORE MATCHING—BALANCING CONDITION

	Before Matching			After Matching		
	Control	BW	Δ (*p*-value)	Control	BW	Δ (*p*-value)
Outcomes						
ln[E5 gasoline price (Day)]	0.320	0.314	0.00***	0.315	0.314	0.74
ln[E5 gasoline price (Night)]	0.368	0.364	0.03**	0.364	0.364	0.99
ln[Margin (Day)]	-2.385	-2.473	0.00***	-2.463	-2.473	0.59
ln[Margin (Night)]	-1.943	-1.989	0.01**	-1.988	-1.989	0.97
Competition						
# Competitors 0.5 km Radius (Day)	0.471	0.452	0.51	0.467	0.452	0.70
# Competitors 0.5 km Radius (Night)	0.258	0.229	0.17	0.231	0.229	0.96
# Competitors 1 km Radius (Day)	1.078	1.097	0.71	1.079	1.097	0.79
# Competitors 1 km Radius (Night)	0.546	0.538	0.79	0.502	0.538	0.41
Stations characteristics						
Share of Youths (18-25-year-old, Munic. Level)	0.075	0.083	0.00***	0.083	0.083	0.99
Premium station	0.439	0.415	0.21	0.394	0.415	0.43
Oligopolistic station	0.373	0.276	0.00***	0.247	0.276	0.21

Notes: Matching was done in a sample of observations from the last pre-treatment week only. Matching was conducted with nearest neighbor matching without replacement. Only stations, which set a price (i.e., which were active) in the respective week, were included in the matching regression. Only observations with positive margins included in the matching regression.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

TABLE A7
ROBUSTNESS CHECKS: PROPENSITY SCORE MATCHING—DID RESULTS

	Gasoline price in Euro/l			ln(Gross Margin)
	(1)	(2)	(3)	(4)
BW × Night × Post	0.0051**			0.0702***
	(0.0025)			(0.0219)
BW × Post		0.0068**	0.0018	
		(0.0028)	(0.0020)	
Approach	TDID	DID	DID	TDID
Sample	Baseline	Only Night	Only Day	Baseline
Observations	147,818	73,909	73,909	147,796
Adjusted R^2	0.887	0.865	0.952	0.768

Notes: All results are based on OLS regressions with standard errors clustered at the county level. The regression setup follows the regression equation from the "Data and Empirical Strategy" section. The sample is based on a propensity score matching estimator with nearest-neighbor matching without replacement within the last pre-treatment period.
$^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

TABLE A8
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

Notes: All results are based on OLS regressions with standard errors clustered at 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.
$^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

TABLE A9
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 (≤1 km)	Border (≤2.5 km)
Observations	1,682	7,310
Adjusted R^2	0.874	0.879

Notes: All results are based on OLS regressions with standard errors clustered at the county level. The regression setup follows the regression equation from the "Data and Empirical Strategy" section. We subsample stations near the policy border.
$^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

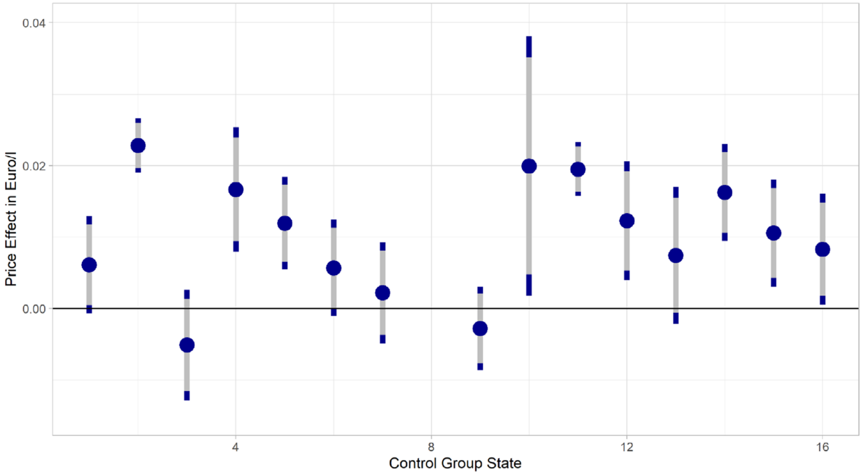

Figure A5
Treatment Effect for Individual Federal State as Control Group

Notes: This plot gives the 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 clustered at 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) [Colour figure can be viewed at *wileyonlinelibrary.com*]

TABLE A10
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

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

TABLE A11
DIFFERENT EFFECT WINDOWS

	Gasoline Price in Euro/l					
	(Baseline)	(2)	(3)	(4)	(5)	(6)
BW × Night × Post	0.0056**	0.0039***	0.0049**	0.0066***	0.0068***	0.0070***
	(0.0023)	(0.0014)	(0.0019)	(0.0020)	(0.0022)	(0.0023)
Effect window (in Weeks)	[-13, 38]	[-10, 10]	[-10, 20]	[-20, 20]	[-20, 30]	[-20, 40]
Observations	593,193	239,037	353,416	467,986	582,423	696,355
Adjusted R^2	0.889	0.833	0.846	0.833	0.865	0.888

Notes: All results are based on OLS regressions with standard errors clustered at the county level. The outcome variable gives where a station chages the price at least one per week in the time period between 10 pm and 5 am or 11 pm and 4 am. The independent variable gives whether a station opens 24/7 in a certain week or not. Observations are at the station × week level.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

APPENDIX B

QUANTILE TREATMENT EFFECTS–ROBUSTNESS CHECK

To show that the policy lifting shifts the gasoline price distribution in a first-order stochastic manner, that is, the unconditional quantile treatment effects are positive at all quantiles, we elicit the counterfactual price distribution—so prices in Baden-Wuerttemberg absent the policy lifting after December 08, 2017—in the style of Chernozhukov *et al.* [2013]. To be precise, we estimate by how much the policy lifting increases/decreases the likelihood of price observations to lie below/above certain price thresholds. Formally, the value of the empirical distribution function (ECDF) of the counterfactual distribution at price p is given through the following

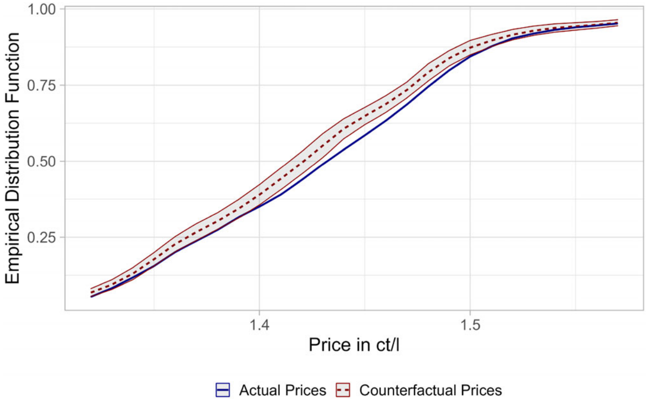

Figure B1
Distributional Effects of the Policy: Counterfactual Price Distribution

Notes: This plot gives the empirical distribution function of observed nighttime post-lifting prices in Baden-Wuerttemberg (blue) and the counterfactual distribution for a scenario without policy lifting (red). The counterfactual distribution comes from distribution regression in the style of Chernozhukov *et al.* [2013] in one Eurocent/l steps. We provide 95% confidence intervals. Standard errors are clustered at the county level. The distributions are trimmed at the 5th and 95th percentile [Colour figure can be viewed at *wileyonlinelibrary.com*]

"distribution regression" which estimates the change in the propensity of a price to be below p due to the treatment:

$$1[P_{sw}^{E5} < p] = \beta BW_s \times Post_w + \alpha_s + \lambda_w + e_{sw}. \tag{B1}$$

The value of the counterfactual ECDF is given by the ECDF of the observed prices minus β. Repeating the procedure for multiple p constructs the full counterfactual distribution.

Figure B1 visually compares observed and counterfactual prices. The policy lifting shifted the price distribution in a first-order stochastic manner to the right. This is indicative of all consumers being affected as the effect is not just driven by one part of the distribution. Instead, consumers at all quantiles of the distribution are affected.

Note that we use weekly average prices and hence we do not fully show that there is no switching opportunity for consumers at a certain point in time which avoids price increases. Admittedly, weekly average prices are indicative of a lack of such switching opportunities. Nevertheless, we encounter this concern by running the same distribution analysis for daily station prices at midnight. We chose midnight as timing as there are barely any price changes after midnight until the end of the prohibition (5 am) (see Figure B2). Hence, the price distribution at midnight likely reflects the price distribution at 1 am, 2 am and, hence, most parts of the nightly prohibition period between 10 pm and 5 am. Results do not change qualitatively (see Figure B3).

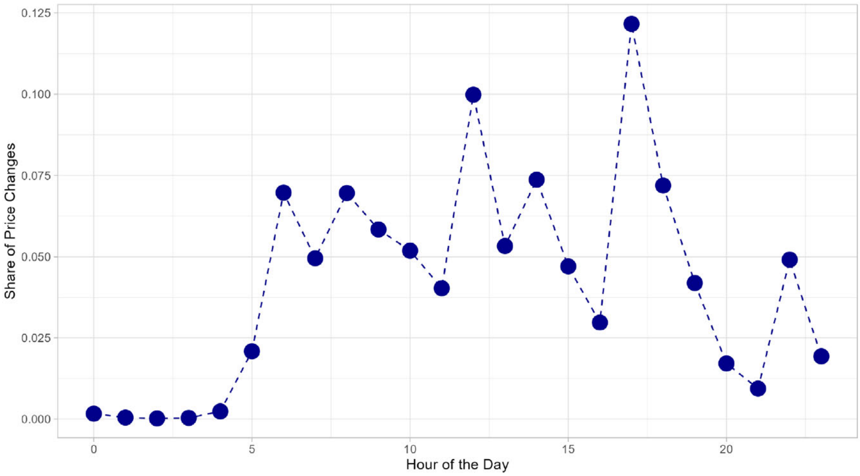

Figure B2
Timing of Price Changes

Notes: This plot gives the timing of price changes of stations in the sample [Colour figure can be viewed at *wileyonlinelibrary.com*]

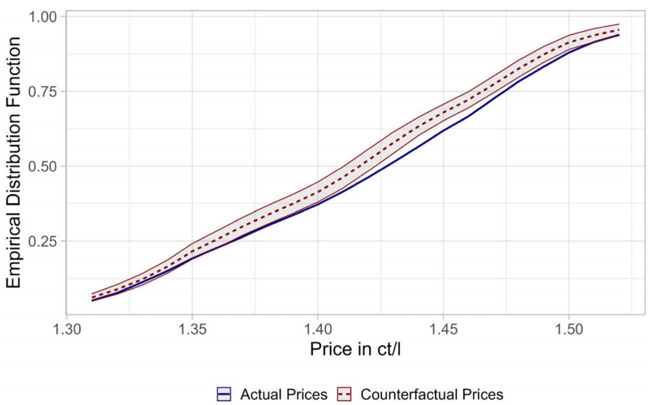

Figure B3
Distributional Effects of the Policy: Counterfactual Price Distribution—Midnight

Notes: This plot gives the empirical distribution function of observed midnight post-lifting prices in Baden-Wuerttemberg (blue) and the counterfactual distribution for a scenario without policy lifting (red). The counterfactual distribution comes from distribution regression in the style of Chernozhukov *et al.* [2013] in one Eurocent/l steps. We provide 95% confidence intervals. Standard errors are clustered at the county level. The distributions are trimmed at the 5th and 95th percentile [Colour figure can be viewed at *wileyonlinelibrary.com*]

APPENDIX C

OPENING HOURS–ROBUSTNESS CHECK

In our main analysis, we use price changes as a measure of stations' nightly activity. While price changes are only meaningful for stations, which are open at a certain time of the day, price changes might not perfectly reflect opening hours. For example, stations, which do not change prices at night, will not be identified as open stations then. To test the robustness of our findings above, we, therefore, provide further results on opening hours. Subsequently, we discuss the data we use and the analysis we conduct.

Data. We collect additional data on station-level opening hours from historic webpages of the price comparison website *clever-tanken.de* through the internet archive *web.archive.org*. The internet archive saves historic webpages erratically and inconsistently across webpages. But it allows us to extract historic opening hours from the gasoline stations' pages on *clever-tanken.de*. Each gasoline station has its own webpage on this domain, which is regularly updated in response to changing prices or opening hours. This website holds up-to-date opening hours since the data is provided by either consumers or the Federal Cartel Office. We can match the opening hours to stations in our dataset based on the name, address, and brand of the stations in the URL code of the archived webpages.

As the stations' sites are archived unregularly, we cannot retrieve opening hours for each station in the relevant time period. Hence, the panel is unbalanced and might be prone to selection issues, which we will investigate later on. Overall, our sample ranges from mid-2016 to mid-2018 and consists of more than 3500 stations ($\approx$ 25% of all stations) and 16,199 observations after subtracting highway stations and stations not able to increase opening hours. On average, each station's opening hours are observed four to five times in the sample. We construct an opening hour measure $1[\text{Active between 10 pm and 5 am}]_{st}$ which will turn one if a station s opens at least once per week during the prohibition period 10 pm and 5 am on the scraped website from date t. The outcome definition follows the definition from the main analysis in Section V where a station was active when setting at least one price between 10 pm and 5 am per week. Moreover, we add two alternative outcomes to test the robustness of our results. First, the logged number of days per week a station opens during the nightly prohibition time. Second, a dummy for a station opening on at least five days per week during the nightly prohibition time.

Results. Table C1 gives the simple difference-in-differences effects of the treatment on the likelihood to open in the prohibition period for a sample of all stations and two subsamples of stations with small or large shop assortment (classifications as in the article above). The regression follows the approach in equation (4). Similar to Figure 4 in the article, we find opening at night to become more likely in response to the treatment. The treatment increases the likelihood to open at night by 1.5 percentage points (see column (1)). In line with Figure 5, the effect is especially driven by shops with a small assortment (see columns (2) and (3) of Table C1). For the two alternative outcomes, a significant opening hour effect is also only evident for shops with a small assortment.

To provide support that these effects can be interpreted as causal effects of the policy lifting, Figures C1 and C2 show rather flat pre-trends for the pooled effect and the subsamples.

Table C1
Robustness Check: Opening Hours

	1[Active between 10 pm and 5 am]			ln(# Days/Week Open between 10 pm and 5 am)			1[Active > 5 Days/Week between 10 pm and 5 am)]		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BW × Post	0.0154*	0.0118	0.0173**	0.0271	0.0168	0.0335*	0.0069	−0.0000	0.0118***
	(0.0091)	(0.0194)	(0.0087)	(0.0223)	(0.0551)	(0.0186)	(0.0106)	(0.0241)	(0.0044)
Sample	All	Large Assortment	Small Assortment	All	Large Assortment	Small Assortment	All	Large Assortment	Small Assortment
Station FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	16,199	6331	9868	16,199	6331	9868	16,199	6331	9,868
Adjusted R^2	0.982	0.983	0.982	0.985	0.984	0.985	0.981	0.978	0.982

Notes: All results are based on OLS regressions with standard errors clustered at the county level. The regression setup follows a simple DID. To include zero value observations in a logged transformation, we use the inverse hyperbolic sine transformation.
$^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

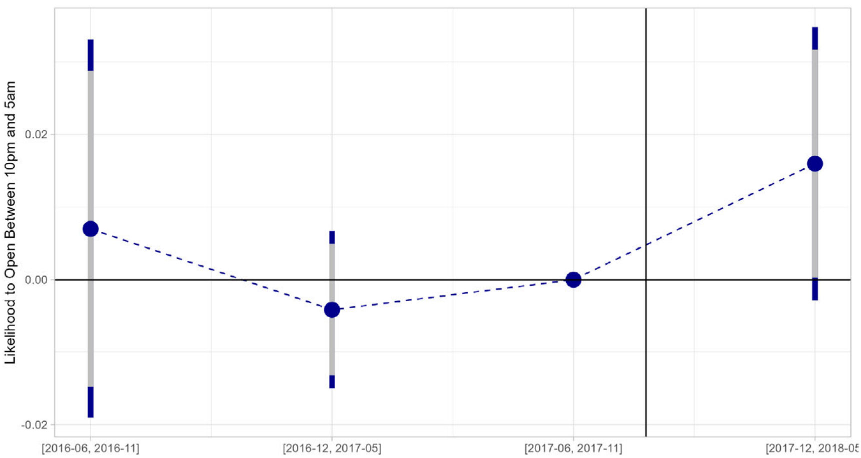

Figure C1
Robustness Check: Opening Hours

Notes: This plot gives dynamic estimates of the leads and lags for the pooled sample. The outcomes variable is a dummy and turns one if a station opens between 10 pm and 5 am at least once a week. Standard errors are clustered at the county level. The exact timing of the beginning and end 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 [Colour figure can be viewed at *wileyonlinelibrary.com*]

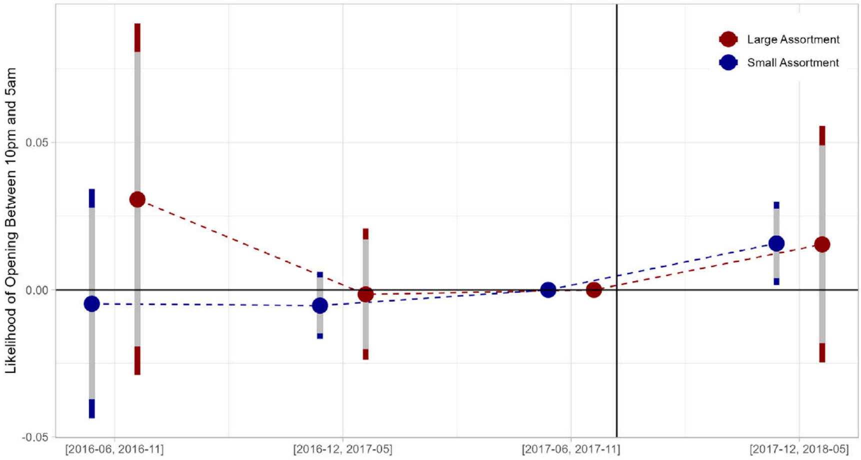

Figure C2
Robustness Check: Opening Hours—Heterogeneity

Notes: This plot gives dynamic estimates of the leads and lags for two subsamples of stations with heterogeneous store assortment. The outcomes variable is a dummy and turns one if a station opens between 10 pm and 5 am at least once a week. Standard errors are clustered at 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 [Colour figure can be viewed at *wileyonlinelibrary.com*]

We also show that this effect is robust to extending the sample period by six additional months after the policy lifting. Figure C3 shows that the positive effect for small assortment stations is persistent over time.

Note that the effects found are smaller than those on the likelihood of price changes in Figures 4 and 5. A potential reason for this is that the treatment effect on the likelihood of price changes there is upward biased. For example, stations which opened at night before the policy lifting but only start to change prices at night after the policy, are misleadingly detected as stations that extend opening hours. A reason might be that a station is located near a station, which extends opening hours in response to the policy. The incumbent station might then react by changing prices as well without extending opening hours. That is, opening hour effects likely spill over to the price setting of others, so that an upward bias in the regressions on price changes is likely.

Sample Selection. To understand in how far our sample is representative for the overall population of stations, we compare the characteristics of the sampled stations to the overall population. Table C2 provides evidence that the sample of historic prices includes stations from more strongly contested markets with slightly lower prices right before the treatment. In strongly contested markets, we did not find a price effect in response to the policy (see Section V). If such markets react less strongly to the policy, our sample might underestimate the opening hour effect for the general sample. On the other hand, sampled stations are located in counties with an on average higher share of youths, which may increase the reaction to the policy. Hence, ex-ante it is not clear that the selected sample induces an over- or underestimation of the population effect.

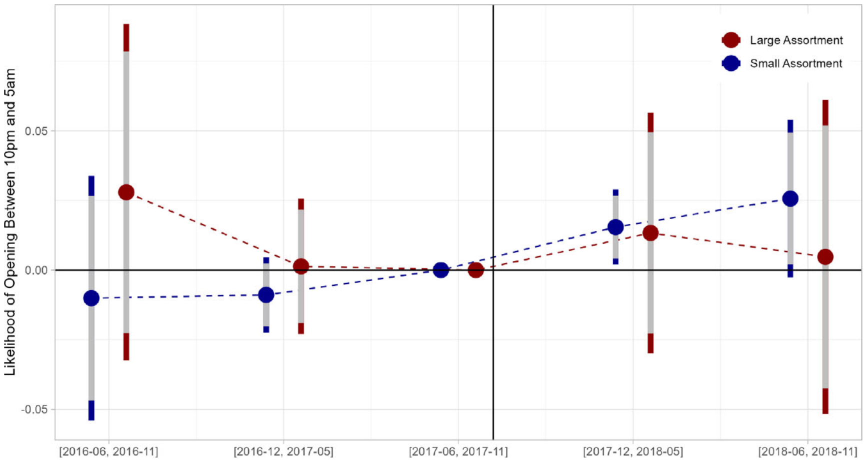

Figure C3

Robustness Check: Opening Hours—Extended Sample Period

Notes: This plot gives dynamic estimates of the leads and lags for the pooled sample and two subsamples of stations with heterogeneous store assortment. The outcomes variable is a dummy and turns one if a station opens between 10 pm and 5 am at least once a week. Standard errors are clustered at 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 [Colour figure can be viewed at *wileyonlinelibrary.com*]

Table C2

Representativeness of Historic Opening Hours Data

	Scraped Data	Full Sample	Δ (p-value)
1[Baden-Wuerttemberg]	0.148	0.130	0.12
E5 gasoline price (Day)	1.361	1.366	0.00***
# Competitors 0.5 km radius (Day)	0.424	0.402	0.05*
# Competitors 0.5 km radius (Night)	0.163	0.164	0.98
# Competitors 1 km radius (Day)	1.134	0.986	0.00***
# Competitors 1 km radius (Night)	0.447	0.402	0.00***
Premium station	0.477	0.494	0.05*
Oligopolistic station	0.459	0.434	0.02**
Share of youths (18–25-year-old, county level)	0.091	0.087	0.00***

Notes: p-values of differences come from regressions of the outcome on a constant and a dummy for stations that are part of the scraped dataset. Standard errors are clustered at the county level. Prices are average prices from the last pre-treatment week for such stations which operated in this week.

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Importantly, the sample is similar in the share of premium stations—so stations with a large shop assortment.

APPENDIX D

EFFECT ON MAXIMUM WILLINGNESS TO PAY

This section sets up and tests hypotheses of how a policy-induced change in the share of informed consumers affects the theoretical, market-level price distribution. In a second step, we test in a difference-in-differences setup whether the effects in the data are better explained by a change in informed consumers or an increasing valuation of gasoline.

Information—in this setting—means the share of consumers who actually are aware of the gasoline price. The policy might induce that a higher share of consumers actually visits gasoline stations expecting to only get alcohol. Also, more consumers might not care about the gasoline price after the policy lifting. Then, the policy would increase the share of consumers, who buy at a random price.

We build on the canonical search model by Varian [1980].[17] His model sets up a market of N symmetric firms, which each sell a homogenous product to a unit mass of consumers. All consumers have a willingness to pay of v. In our case, the product is gasoline, which arguably is homogenous (Martin [2023]; Montag *et al.* [2023]; Pennerstorfer *et al.* [2020]) and demand is quite inelastic. A share η of consumers know all prices in the market and hence buy at the lowest price. $1-\eta$ consumers do not observe prices and buy at a random station. In our case, the policy lifting could induce that more consumers do not expect to buy gasoline and hence buy at a random station. This is equivalent to a lower η.

Following Varian [1980], the unique equilibrium is given by the price distribution

$$F(p) = 1 - \left(\frac{1-\eta}{N\eta}\frac{v-p}{p-c}\right)^{\frac{1}{N-1}},$$

where c are marginal costs and the support of $F(p)$ is given by $[\underline{p}, \overline{p}] = \left[c + \frac{v-c}{1+\frac{N\eta}{1-\eta}}, v\right]$.

One can easily see that the upper bound of the support of $F(p)$ is independent of the share of informed consumers. Changes in the upper bound can only be rationalized through changes in consumers' valuation for gasoline.

We test empirically whether the upper bound of prices $\overline{p}$ is affected by the treatment. As argued in Wildenbeest [2011], the maximum price observed in a market is a consistent estimate of the upper bound of $F(p)$. For each market and month, we get the estimate as the maximum price observed.[18] We abstract from markets with only one firm as the model's predictions do not hold for monopolies. Markets are defined

[17] Other papers such as Pennerstorfer *et al.* [2020] model consumer information in gasoline markets similarly.

[18] Note that the estimate heavily varies with the oil price. Nevertheless, we are only interested in the difference and comparison between markets in Baden-Wuerttemberg and other federal states. Hence, the level of the estimate is less relevant as we account for month fixed effects in the difference-in-differences regression later on.

TABLE D1
EFFECT ON MONTHLY, MARKET-LEVEL WILLINGNESS TO PAY

	$\hat{\bar{p}}$		
	(1)	(2)	(3)
BW × Post	0.0058*	0.0049**	0.0070***
	(0.0032)	(0.0023)	(0.0020)
Market	0.5 km radius	1 km radius	2 km radius
Market FE	✓	✓	✓
Month FE	✓	✓	✓
Observations	11,066	22,986	40,709
Adjusted R^2	0.871	0.771	0.767

Notes: As the model only predicts outcomes for markets with at least two competitors, we drop markets with only one station. Maximum prices based on daily, station-level midnight prices. We provide 90 and 95% confidence intervals. Standard errors are clustered at the market level. Related literature uses similar market radii for market delineation such as 2 miles linear distance (Chandra and Tappata [2011]) or 2 miles driving distance (Pennerstorfer *et al.* [2020]). December 2017 is the first month classified as post-lifting.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

at the station level by drawing circles around each station. We apply a 0.5, 1, and 2 km radius.

We, then, run the following regression

$$\hat{\bar{p}}_{mt} = \alpha_m + \lambda_t + \theta(BW_m \times Post_t) + \epsilon_{mt},$$

where $\hat{\bar{p}}_{mt}$ is the estimate for $\bar{p}$ in market m and month t. Table D1 shows the treatment effect for different market definitions. Results show that that $\hat{\bar{p}}$ increases with the treatment. This is not in line with a change in the share of informed consumers but with an increasing valuation for gasoline. This supports the demand expansion mechanism described in the main part of the article.

APPENDIX E

DYNAMIC RANK REVERSAL TEST

In a homogenous product market, price rank reversals of two gasoline stations can hardly be explained without considering consumer frictions. For example, input price changes over time do not cause prices of one station to increase more than those of others.

Chandra and Tappata [2011] propose a test for consumer frictions following this intuition. For each station couple c (station A and station B), one calculates the share of days, on which the usually cheaper station A is the more expensive one. Formally, this is given by:

$$rr_{ct} = \frac{1}{N_{ct}} \sum_{\tau=1}^{N_{ct}} 1[p_{A\tau} > p_{B\tau}],$$

where rr_{ct} gives the rank reversal measure for couple c for period t. N_{ct} is the number of days both stations report a price and $p_{A\tau}$ and $p_{B\tau}$ are prices at midnight for stations A and B on day τ. We calculate the rank reversal measure for each couple twice—once

Table E1
Effect of Policy Lifting on Likelihood of Rank Reversals

	rr_{ct}			
	(1)	(2)	(3)	(4)
1[1 ≥ Distance in km ≤ 1.5]	−0.0109**			
	(0.0043)			
1[0.5 ≥ Distance in km ≤ 1]	−0.0108**			
	(0.0047)			
1[0.15 ≥ Distance in km ≤ 0.5]	−0.0159***			
	(0.0060)			
1[Distance in km ≤ 0.15]	−0.0260***			
	(0.0083)			
BW × Post		−0.0040	0.0101	0.0090
		(0.0194)	(0.0122)	(0.0084)
Couple Distance	≤ 2 km	≤ 0.5 km	≤ 1 km	≤ 2 km
Couple FE	×	✓	✓	✓
Observations	7,073	991	2,513	7,073
Adjusted R^2	0.002	0.408	0.359	0.345

Notes: Rank reversal measures based on daily prices at midnight. Standard errors are clustered at the station couple level. Post dummy included in the regressions.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

for all observations before and once for all observations after the policy lifting. Only data from dates on which both stations operate is used. We use data from midnight prices. As prices hardly change during nighthours (see Figure B2), this analysis likely holds for all other points in time during the nightly prohibition.

We then run the following regressions for a subsample of couples with a maximum linear distance between the two stations of 1 or 2 km:

$$rr_{ct} = \lambda_c + \gamma Post_t + \beta BW_c \times Post_t + \epsilon_{ct},$$

where β gives the change in rank reversals related to the policy lifting. A couple is considered to belong to Baden-Wuerttemberg ($BW_c = 1$) if both stations are located in Baden-Wuerttemberg but our results also hold when a couple is also treated in the case that only one station lies in Baden-Wuerttemberg.

Table E1 shows two results: First, column (1) shows that frictions decrease for a lower distance between stations which is in line with findings in the literature (Chandra and Tappata [2011]; Martin [2023]; Pennerstorfer *et al.* [2020]). Second, we show that the policy does not affect rank reversals in columns (2) to (4). Hence, there is no indication for a change in consumer information caused by the policy.

APPENDIX F

CORRELATION OF PRICES

In this section of the appendix, we show that prices of neighboring stations do not comove more or less in response to the policy lifting.

Demand could have become less elastic in Baden-Wuerttemberg after the policy lifting as more consumers visit the gasoline station for alcohol and, hence, might care less about the gasoline price. If this was the case, prices of competitors would become

TABLE F1
EFFECT OF POLICY LIFTING ON NEIGHBORING STATIONS' PRICE CORRELATION

	$corr_{ct}$					
	Actual Prices			Residualized Prices		
	(1)	(2)	(3)	(4)	(5)	(6)
BW × Post	0.0341	0.0157	0.0288**	−0.0343	−0.0200	−0.0114
	(0.0464)	(0.0240)	(0.0145)	(0.0524)	(0.0367)	(0.0217)
Couple Distance	≤ 0.5 km	≤ 1 km	≤ 2 km	≤ 0.5 km	≤ 1 km	≤ 2 km
Couple FE	✓	✓	✓	✓	✓	✓
Observations	957	2476	7017	990	2510	7062
Adjusted R^2	0.449	0.496	0.521	0.652	0.622	0.623

Notes: Correlations calculated based on daily, station-level midnight prices. Standard errors are clustered at the station couple level. Post dummy included in the regressions.
$^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

less important (less elastic cross-price elasticity). At the extreme, demand could be sufficiently inelastic so that stations become quasi-monopolists. Then, neighboring stations' prices will not be strategic responses.

To examine whether neighboring stations' price comovement is affected by the policy lifting, we setup the following difference-in-differences regression equation:

$$corr_{ct} = \lambda_c + \gamma Post_t + \beta BW_c \times Post_t + \epsilon_{ct},$$

where $corr_{ct}$ is the correlation between midnight prices of neighboring stations for two time periods t—before and after the treatment—separately. We later on use the correlation of actual prices as well as prices residualized from state-date specific price effects. The latter accounts for correlation purely caused by input price fluctuations. β gives the treatment effect.

Table F1 gives the estimation results. There is no robust evidence for a change in the correlation of neighboring stations' prices. This supports our claim that the price effect is not caused by less elastic demand in response to the policy. This would have likely resulted in a lower correlation of prices after the policy lifting in Baden-Wuerttemberg.

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