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Frontiers: The Effect of an Ad Ban on Retailer Sales: Insights from a Natural Experiment

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Abstract. Advertising bans typically target products that deceive consumers in ways that can threaten their physical and mental health. An alternative policy objective might seek environmental protection through a ban on print advertising. Such measures would profoundly affect grocery retailers relying on printed leaflets to communicate weekly promotions. We measure the causal effect of banning advertising on retail performance by studying a temporary advertising ban implemented in a German federal state during the COVID-19 pandemic. The ban resulted in the suspension of all print advertising by grocery retailers, and the exogenous variation in advertising created by this natural experiment serves as our identification strategy. We apply difference-in-differences regressions to data from a national grocery retailer and find that the ban resulted in a 6% sales decrease in the treated state compared with an adjacent state. GfK Household Panel data reveal no effect of the advertising ban on the market level but a negative impact on retailers offering and advertising weekly promotional product assortments. We study the sensitivity of these results to the COVID pandemic and find that neither changes in COVID-19 incidence, vaccination rates, nor customers' mobility moderate the ad ban effect. The findings offer practical insights for regulators and retailers regarding the impact of ad bans and the value of advertising.

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1. Introduction

Regulators impose advertising bans to protect consumers from false or misleading information (Rao 2022) and safeguard their physical and mental well-being, as in the case of advertising fast food, tobacco, and alcohol (e.g., Hamilton 1972, Dhar and Baylis 2011, Goldfarb and Tucker 2011). Some new regulations also address the negative environmental impact of advertising, whether the ads promote environmentally harmful products, such as fossil fuels,[1] or create a negative environmental footprint themselves. For example, producing and distributing printed advertising materials require substantial natural resources, including paper and water, which contribute significantly to CO_2 emissions and generate large amounts of waste.[2]

Addressing such concerns might benefit the environment, but banning print advertising could have a profoundly negative impact on retailers. Such bans would especially affect grocery retailers, which generate almost all of their sales through physical stores (Redman 2021) and rely heavily on nondigital advertising, such as weekly printed promotion leaflets distributed via mail and newspapers, to increase their sales (Gijsbrechts et al. 2003, Ailawadi and Gupta 2014, Prediger et al. 2019). The health of the retail sector is critical; it accounts for approximately 6% of the U.S. gross domestic product (GDP) and 15% of U.S. advertising spending (U.S. Bureau of Economic Analysis 2022, Statista 2023). Thus, advertising regulations that target a crucial resource for (grocery) retailers represent an important research topic.

With the current study, we seek to evaluate how banning print advertising affects retailers. To this end, we measure the causal effect of grocery retailer advertising on consumer purchasing behavior and overall retailer performance. The identification strategy relies on a natural experiment resulting from a regulation that temporarily banned advertising for nonfood products at grocery retailers in Saarland, one of Germany's 16 federal states. The state implemented the ad ban in the spring of 2021, whereas all other states, including its only adjacent state Rhineland-Palatinate, remained unaffected. Because of the ban, retailers stopped *all* print advertising in the affected state for three weeks.

We use a difference-in-differences (DiD) research design to estimate the ad ban effect based on market basket data from a nationwide grocery retail chain operating in both states. The analysis of 8,032 store × day observations indicates that the ad ban led to a 6% decrease in sales revenue in the treated state. According to a revenue decomposition, this effect stems from fewer shopping trips, but we do not find any significant change in the size of shopping baskets. In additional analyses, we study the mechanism of the decrease in sales: The ad ban reduces sales revenue by approximately 6.5% for customers without loyalty cards, whereas the change in sales revenue is not statistically significant for loyalty card customers (the difference between customers with and without loyalty cards is not statistically significant). Customers without loyalty cards typically have higher search and store switching costs, which the ad ban may exacerbate by increasing search costs for retail promotions.

To study the market-level effects of the ad ban beyond the focal retailer, we apply a DiD regression to a second data set from the GfK Household Panel. We find no significant change in household expenditures or shopping trips at the market level. The ad ban does not shrink the market but instead shifts demand across retailers. Accordingly, we identify a negative effect of the ad ban on revenue and shopping trips for retailers that offer and advertise weekly promotional assortments, consistent with our findings at the focal retailer.

Our study thus offers three contributions. First, we contribute to emerging literature on sustainability-related advertising regulation (Guyt et al. 2023) by quantifying the effects of banning paper-based advertising on sales revenues and store visits. The natural experiment provides a relevant context in which all retailers stopped advertising, contrasting with experiments involving single retailers that temporarily stop advertising. Potential ad bans would affect all retailers, so our study context is consistent with such a scenario. Notably, we find no effect of the ad ban at the market level but a significant differential effect of the ban across retail formats. Therefore, policymakers need to account for differences among retailers when considering ad regulations.

Second, our research represents one of the few efforts to study the impact of ad bans on retailer sales. Previous ad ban research has mainly focused on specific products, brand sales, or product consumption (Dhar and Baylis 2011, Goldfarb and Tucker 2011) and offers mixed and inconclusive results about the effectiveness of ad bans (Saffer and Chaloupka 2000, Nelson 2004, Dhar and Baylis 2011). In contrast, we analyze how an ad ban affects overall retailer performance and show that advertising bans can significantly influence store sales and the frequency of consumers' shopping trips.

Third, this article extends the literature on the effectiveness of advertising and promotions in grocery retailing (Bell et al. 1999, Briesch et al. 2009). Debates about the extent to which advertising is effective persist (Shapiro et al. 2021), and the challenge of measuring the impact of nondigital advertising on store choice and retailer performance is well recognized (Srinivasan et al. 2004, Bodapati and Srinivasan 2006, Blake et al. 2015), partly because of retailers' reluctance to stop advertising. Furthermore, research that focuses on advertising variation at the margin has examined differences in discount depth or the selection of promoted categories and brands (Ailawadi and Gupta 2014). Our research takes advantage of a unique situation in which retailers had to stop all advertising. This natural experiment facilitates measuring the causal effects of advertising on retailer performance. Because all retailers, rather than a single retailer, stopped advertising, we obtain a conservative estimate of advertising effectiveness. The evidence indicates no effect at the market level. Our analysis thus suggests that retail advertising shifts sales among retailers. Retailers that offer temporary promotional assortments depend on retail advertising to attract customers, consistent with the results observed at the focal retailer.

Reflecting on these contributions, we note that the ad ban occurred during the COVID-19 pandemic. The specific context provided a rare opportunity to study the effects of stopping print advertising, but it also requires critical considerations of the sensitivity of our findings to the context. For example, we analyze whether COVID-19 incidence rates, vaccination rates, and changes in customers' mobility during the pandemic moderated the ad ban effect. In addition, we study market-level shopping behavior and basket composition during the COVID-19 pandemic. An alternative identification strategy based on a Bayesian structural time-series model (Brodersen et al. 2015) produces results similar to the findings in the main analysis. We also conduct three placebo tests, which yield no significant effects, and implement a wide array of robustness checks related to the model specification. These varied efforts offer no clear indication that the pandemic affected the key results, but we still cannot rule out an influence of the timing of the ad ban, namely, during the COVID-19 pandemic.

2. Natural Experiment

For our empirical analysis, we leverage a policy decision by Saarland's state government that temporarily banned advertising of nonfood products in March 2021 in response to lobbying efforts by nonfood retailers who opposed Germany's COVID-19 retail policy. In December 2020, the German government initiated a nationwide shutdown of nonessential businesses, including all nonfood retailers, to limit the spread of the virus. Grocery retailers and other essential businesses were allowed to remain open. Nonfood retailers perceived this policy as discriminatory, mainly because all grocery retailers sell and advertise at least some nonfood products (e.g., kitchen items, clothes, home improvement, consumer electronics). In addition, it is noteworthy that some grocery retailers, though not all, list a substantial number of nonfood products as part of their short-term promotional assortments for the duration of the promotion—typically for one week. Nonfood retailers, therefore, advocated for a ban on nonfood advertising to help level the playing field.

Responding to this pressure, Saarland announced the possibility of an ad ban on nonfood products on February 12, 2021, with a formal decision scheduled for February 16. The Saarland state government published a legally binding ordinance on February 16 that banned nonfood advertising, effective February 22. Noncompliance would result in substantial fines. Notably, Saarland was the only federal state in Germany that implemented this ad ban.

In response, grocery retailers temporarily stopped all print advertising activities, including the distribution of promotion leaflets.[3] There are several reasons retailers did not simply remove nonfood products from their weekly promotion leaflets while continuing to advertise food and near-food products (e.g., laundry care, body care). First, Saarland, the treated state, accounts for only approximately 1.2% of Germany's population and 0.9% of its GDP.[4] Retailers expected lower costs by not delivering promotion leaflets in Saarland rather than modifying their content. Second, we learned in conversations with the focal retailer that altering the advertising content would be infeasible on such short notice because of the complexity of its national operations. Third, promotion and pricing decisions must comply with existing manufacturer contracts and (international) product sourcing agreements and are often made months in advance.

Reflecting the legal start of the ad ban on February 22, the last distribution of leaflets occurred on February 20. These leaflets featured promotions active February 22–28. The first ad distribution prevented by the ad ban was on February 27, which affected promotions active between March 1 and March 7. The ad ban ended on March 10, when a local court ruling permitted all retail stores to reopen. Following the required lead time for printing and distribution, grocery retailers resumed distributing advertising materials on March 20, featuring promotions active from March 22 to March 28. The retailer did not change the advertising content during or after the ad ban but followed the initially planned promotion calendar. We summarize the timeline of key events in Table 1.

This setting provides a unique opportunity to study advertising effectiveness and advertising bans. First, the exogenous change in retail advertising creates a natural experiment that enables us to measure the causal effects of the ad ban on retailer performance. Prior literature has addressed advertising variation at the margin (e.g., promoted brands, size of leaflets); our setup allows us to evaluate the complete discontinuation of advertising.

Table 1. Ad Ban Timeline

Date	Event
February 12	Saarland announces potential ad ban for the first time.
February 16	Decision to start ad ban that forbids nonfood advertising (including retailers' print advertising) in Saarland on February 22; no ad ban in Germany's other federal states.
February 20	Last distribution of ads in Saarland for promotions in calendar week 8 (February 22–28).
February 22	Legal start of ad ban in Saarland; no ad ban in Rhineland-Palatinate and Germany's other federal states.
February 27	First skipped distribution of ads for promotions in calendar week 9 (March 1–8).
March 1	Start of first promotion week affected by ad ban in Saarland.
March 10	Legal end of the ad ban. Court decision allows nonessential retailers to reopen. The updated ordinance was published on March 13.
March 20	First possible leaflet distribution after ad ban in Saarland (see notes below).
March 22	First week in which promotions (advertised in leaflets) are active in Saarland.

Notes. All dates refer to 2021. This timeline assumes that retailers, printing companies, and logistics companies resumed the production and distribution of leaflets within five working days after the ban ended. We report results for robustness analyses that assume a longer time window without leaflet advertising in Online Appendix D.4; the key findings do not change.

Second, the advertising ban solely affected advertising distribution in the treated state. In contrast, it did not impact the advertising content or pricing and assortment decisions of the affected retailers, which are large national retail chains. We thus employ a DiD research design to measure the causal effect of the ad ban, contrasting sales in treated stores with those of the same retailer in the adjacent control state. Figure 1 shows the store locations in the treated state (circles) and the adjacent control state (squares), focusing on counties along the state border. Third, the ad ban required all retailers to stop advertising, resembling scenarios in which policymakers force all retailers to cease print advertising or retailers voluntarily stop print advertising to enhance sustainability.

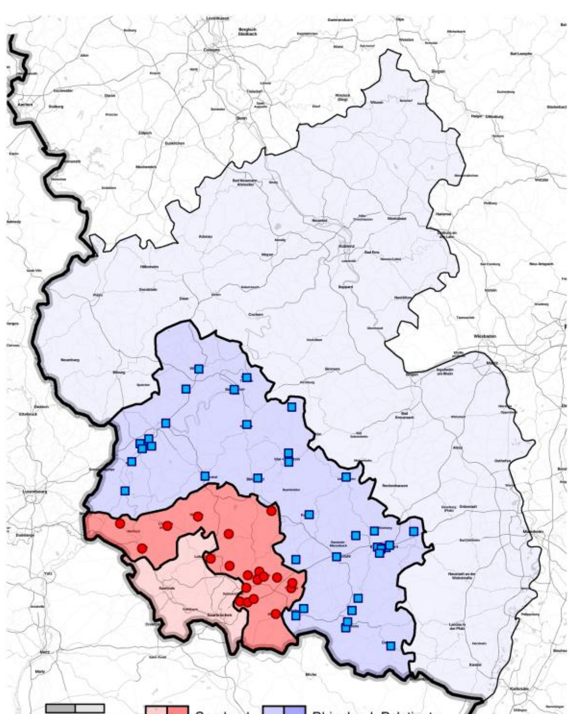
3. Data and Descriptive Results

We derive the dependent variables in our analysis from market basket data obtained from one of Germany's national grocery retailers.[5] The unit of observation is a store × day combination. The data span 25 weeks, January 4–June 27, 2021. We observe aggregate sales, the number of shopping trips, and the average basket size for 56 stores in the border counties between Saarland (treatment state) and Rhineland-Palatinate (control state). The final data set consists of 8,032 observations. We enrich this data set with data from four additional sources: daily COVID-19 incidence and vaccination counts from the German Center for Disease Control, daily mobility data from the COVID-19 mobility project, daily weather data provided by the German weather service, and monthly unemployment data sourced from the Federal Employment Agency.[6] To obtain store-level weather data, we match each retail store to the nearest weather station.[7] All other variables are measured at the county level.

The primary dependent variable is $SalesRevenue_{it}$, which indicates the revenue of store i at time t. Two additional dependent variables, $NumberOfShoppingTrips_{it}$ and $BasketSize_{it}$, decompose store revenues, enabling us to identify drivers of revenue changes during the ad ban. A fundamental assumption underpinning our estimation strategy is that the average change of dependent variables is the same for both the treatment and control states in the absence of the treatment (Goldfarb et al. 2022). We provide time-series plots for all dependent variables that illustrate common pretreatment trends for treated and untreated (control) units in Online Appendix A.

The key independent variable is $AdBan_{it}$, a binary variable denoting whether a given store i is affected by the ad ban at time t. The ban lasted three weeks in March, starting on March 1 and ending on March 21. $Incidence_{jt}$ represents the COVID-19 incidence in county j at time t, which was a crucial policy metric in Germany and the most frequently cited indicator in media coverage related to the spread of COVID-19.[8] Furthermore, the variable $IncidenceDelta_{st}$ captures time-varying differences in

Figure 1. (Color online) Store Locations in the Treatment State and Control State



Notes. Treated state with ad ban (Saarland) and adjacent control state without ad ban (Rhineland-Palatinate). We focus on the stores in the counties along the state border (circles in treated state and squares in control state). The states share borders with France and Luxembourg in the south and west.

COVID-19 incidences between the (treated and control) states s at time t. This variable accounts for possible behavioral responses related to differing infection levels. $VaccinationRateDose1_{jt}$ and $VaccinationRateDose2_{jt}$ measure the fraction of county j's population that is vaccinated against COVID-19 on day t (first and second vaccine dose, respectively). $MobilityChange_{jt}$ quantifies changes in consumers' mobility in county j on day t relative to the corresponding month in 2019, before the onset of the pandemic.[9] $Unemployment_{jm}$ is the monthly unemployment rate in county j and month m, which we use as a control variable to account for changes in economic conditions. $Rainfall_{it}$ measures precipitation (in millimeters/10) around store i at time t. It is a proxy for bad weather, which may affect consumers' shopping behavior (Chintagunta et al. 2012). Table 2 contains the key variables and their descriptive statistics.

To assess the validity and effectiveness of the control variables, we analyze the relationship between *MobilityChange* and the other control variables (see Online Appendix B for detailed results). We find that a higher COVID-19 incidence and more rainfall decrease customers' mobility, whereas higher vaccination rates increase mobility. These results suggest that *Incidence* and *VaccinationRateDose1* capture the effects of the pandemic on consumers' mobility. Furthermore, we do not find evidence in this analysis that the ad ban impacts consumers' mobility, so we conclude that *MobilityChange* has discriminant validity from the treatment variable.

Table 3 presents model-free evidence for the effect of the ad ban on our three dependent variables: sales revenue, number of trips, and average basket size. The analysis reveals that sales in both states are lower during the ad ban, but the decrease is larger in the treatment state. A descriptive DiD analysis measures a 5.24% ($p < 0.05$) decline in sales revenue during the ad ban. The effect is primarily driven by a decreased number of shopping trips (−4.73%, $p < 0.01$), whereas basket sizes remain unaffected.

In the next section, we present a model-based DiD analysis of the ad ban effect that incorporates the control variables and time and store fixed effects. We also explore the underlying mechanism for the ad ban effect.

4. Model and Estimation Results

4.1. Model Specification

The model-based analysis employs a DiD research design to estimate the causal effect of the ad ban on retailer performance (Seiler et al. 2017). We estimate the impact of the ad ban by comparing the changes in the three store-level performance indicators—sales revenue, number of shopping trips, and average basket size—in the treated state during and outside the ad ban period, relative to the changes in the control state. Equation (1) specifies the regression equation:

$$Y_{it} = \beta\, AdBan_{ij} + \boldsymbol{\gamma}\, Controls_{ijt} + \mu_i + \tau_t + \varepsilon_{it}, \qquad (1)$$

where Y_{it} denotes the dependent variable, which is (1) the log-transformed sales revenue of store i at time t, (2) the log-transformed number of shopping trips to store i at time t, or (3) the log-transformed basket size at store i at time t. The unit of time t is a day.

Our primary focus is estimating β, which measures the effect of the ad ban on retail store performance. In addition, the parameter vector $\boldsymbol{\gamma}$ captures the effects of a matrix of control variables, including time-variant COVID-19 factors (i.e., log-transformed incidence, incidence delta, vaccination rates), mobility change, unemployment, and rainfall at the county level j or store level i, respectively. Moreover, we incorporate a dummy variable to account for the postban period in the treated state (Post × Treatment state). Our two-way fixed effects model includes store fixed effects μ_i and time fixed effects τ_t. Store fixed effects μ_i account for all unobserved differences among stores, whereas the time (day) fixed effects τ_t capture all unobserved temporal differences. We use robust standard errors clustered by store.

Table 2. Descriptive Statistics

Variable	Mean	SD	2.5th Pct.	97.5th Pct.
Sales revenue (in €)	211,710.27	70,960.21	108,000.39	385,342.75
Number of shopping trips	3,934.20	1,124.53	2,228.71	6,500.29
Basket size (in €)	53.96	9.67	36.04	72.33
Ad ban (dummy)	0.04	0.21	0.00	1.00
Incidence	0.07	0.04	0.00	0.16
Incidence delta	0.00	0.01	−0.02	0.04
Vaccination rate dose 1	0.17	0.21	0.00	0.73
Vaccination rate dose 2	0.07	0.11	0.00	0.44
Mobility change	−0.04	0.14	−0.29	0.27
Unemployment	6.36	2.21	3.30	12.00
Rainfall	0.27	0.53	0.00	1.84

Note. We multiply the three dependent variables—sales revenue, number of shopping trips, and average basket size—by scaling factors (with $s_{sr} = s_{nt} \cdot s_{bs}$) to retain the confidentiality of the data source.

Table 3. Model-Free Results

Variable	Sales revenue	Number of trips	Basket size
Treated state before ad ban	179,689.32	3,361.07	53.05
Treated state during ad ban	174,661.37	3,320.07	52.35
Control state before ad ban	207,309.93	3,876.43	53.92
Control state during ad ban	212,009.60	4,001.48	53.48
Difference-in-differences mean	−5.24%*	−4.73%**	−0.49%
Difference-in-differences 95% CI	[−9.44%, −1.29%]	[−8.87%, −0.34%]	[−2.63%, 1.84%]

Note. The variables are multiplied by the same scaling factors as in Table 2.
$^{**}p < 0.01$; $^{*}p < 0.05$.

4.2. Main Results

Table 4 presents the regression results for our three dependent variables. Starting with sales revenue, we find that the ad ban coefficient is negative and significant ($\beta^{Sales} = -0.060$, $p < 0.01$); the ad ban decreases sales revenue by approximately 6%. Similarly, the ad ban coefficient for the number of shopping trips is negative and significant ($\beta^{Trips} = -0.051$, $p < 0.001$), indicating that the number of store visits decreases by approximately 5.1% as a direct consequence of the ad ban. In contrast, we find an insignificant ad ban coefficient for basket size ($\beta^{Size} = -0.008$, $p > 0.10$). These results suggest that the decline in sales revenue is almost entirely driven by reduced store visits. In contrast, the size of the shopping baskets largely remains unaffected by the ad ban.

Regarding the control variables, we observe that a higher mobility change increases the number of trips ($p < 0.01$). The effect of mobility on sales is positive but not statistically significant, possibly because of the negative impact of mobility on basket size. This finding is plausible and suggests that mobility is an effective covariate that can be used as a potential treatment effect moderator (see Section 6). Rain reduces the number of trips ($p < 0.05$), reflecting the impact of weather conditions on shopping behavior. No other coefficients, including Post × Treatment state, are statistically significant.

To assess the validity and robustness of our identification strategy, we conduct three analyses.[10] First, several placebo tests yield effects that are not significantly different from zero. We present a more detailed description of the placebo tests and the complete regression results in Online Appendix D.1. Second, with a series of additional robustness checks, we evaluate if differences between the treated and control states affect the main findings. Online Appendix D.2 contains further details regarding the analyses and results. Third, we use a Bayesian time-series model (Brodersen et al. 2015) to measure the effect of the ad ban on sales. This approach models outcomes in the two states separately and relies on variation over time to measure the ad ban effect. The analysis yields results in line with the main analysis; we find a significant ad ban effect on sales revenues of −5.6% in Saarland, the state affected by the ad ban (95% confidence interval (CI) = [−8.6%, −2.5%]). Notably, we do not find a significant ad ban effect in the control state (0.1%), with 95% CI centered around zero (CI = [−3.3%, 3.0%]). We provide further details for this analysis in Online Appendix D.3.

Table 4. Main Regression Results

Variable	Sales revenue	Number of trips	Basket size
Ad ban (β)	−0.060***	−0.051***	−0.008
	(0.017)	(0.014)	(0.007)
Incidence	0.419	0.223	0.165
	(0.244)	(0.154)	(0.084)
Delta incidence	−0.198	−0.048	−0.149
	(0.479)	(0.331)	(0.165)
Vaccination rate dose 1	−0.052	−0.042	−0.008
	(0.069)	(0.055)	(0.029)
Vaccination rate dose 2	0.089	0.083	−0.003
	(0.099)	(0.077)	(0.041)
Mobility change	0.199	0.242**	−0.045
	(0.101)	(0.062)	(0.047)
Unemployment	0.048	0.022	0.024
	(0.025)	(0.021)	(0.013)
Rain	−0.013	−0.014*	0.002
	(0.013)	(0.006)	(0.003)
Post × Treatment state	−0.027	−0.015	−0.011
	(0.017)	(0.014)	(0.008)
Store fixed effects	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes
R^2	0.54	0.67	0.75
N	8,032	8,032	8,032

Note. Robust standard errors (clustered by store) are in parentheses.
$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

4.3. Heterogeneity Analysis

We examine the differential impact of the ad ban across customers, products, and time to understand the mechanism and factors leading to this ad ban effect. First, we distinguish customers who use loyalty cards from those who do not. Customers with loyalty cards self-identify at the checkout, allowing them to receive additional discounts. By calculating our focal dependent variable—daily sales revenue per store—according to whether the transactions involve a loyalty card, we can attribute sales to customer groups with varying degrees of search and switching costs.[11] We expect the ad ban effect to be greater for customers without loyalty cards, who tend to have higher switching costs from other retailers to the focal retailer

(Rossi and Chintagunta 2022). The ad ban further increases search costs for retail promotions, resulting in increased store switching costs. As the results in Table 5 show, the ad ban reduces sales revenue by approximately 6.5% for customers without loyalty cards. In contrast, the change in sales revenue for loyalty card customers is not statistically significant. The number of shopping trips also decreases by around 5.5% for non-loyalty-card customers, with no significant change in the number of trips among loyalty card customers. One potential explanation is that retail advertising attracts new customers, some of whom might be lost during the ad ban. However, we note that a Wald test reveals that the difference in the estimated coefficients for Ad ban between customers with and without loyalty cards is not significant for sales revenue ($p = 0.409$) or number of trips ($p = 0.284$).

Second, we differentiate shopping trips that include products from the retailer's promotional assortments and those that do not. Products in promotional assortments are available only during the promotion; they might include food, near-food items (e.g., laundry care), and nonfood items (e.g., clothes, consumer electronics). We then define promotion sales revenue as the total revenue generated from *baskets* that contain at least one item from the promotional assortment. Nonpromotion revenue instead denotes the total revenue from baskets without promotional items. Similarly, promotion trips are the number of shopping baskets that contain at least one item from the promotional assortment, and nonpromotion trips refer to baskets without items from the promotional assortment. We find a statistically significant ad ban effect for promotion baskets (revenue −8.2%, number of trips −11.2%). The impact on baskets without products from the promotional assortments is negative but not statistically significant (revenue −3.7%, number of trips −2.9%). The difference between the types of baskets is statistically significant for the number of shopping trips ($p < 0.01$) but not for revenue ($p = 0.153$).

Third, we measure the ad ban effect separately for the days when the promotional offers start (Mondays and Thursdays), compared with all other days. The ad ban effect is significantly larger on main promotion days for both outcome variables ($p < 0.001$), in line with our proposition that retail advertising appears to generate store traffic. Notably, promotion baskets and baskets on main promotion days contain products from the retailer's regular assortment. Thus, attracting more customers through promotional assortments increases sales of promoted items and also leads to increased sales of regular products. Detailed results for these analyses are available in Online Appendices C.2 and C.3.

5. Market-Level Effects of the Ad Ban

Thus far, we have analyzed the effects of the ad ban on a specific retailer, using that retailer's store-level sales data. The key driver of revenue losses is a decline in shopping trips, which is significantly greater for baskets that contain products from the retailer's promotional assortment. To understand the broader implications of the ad ban, we also analyze the ban's market-level effects.

We base this analysis on the GfK Household Panel. The data set contains the expenditures of approximately 1,800 households in the two focal states: 400 in the treated state and 1,400 in the control state. All households report data during the entire analysis time window (identical to the main analysis; see Section 3). The limited number of households in the control state necessitates aggregating the panel data by state and week, which results in a smaller sample size ($n = 50$, 2 states $\times$ 25 weeks) but reduces noise in the outcome variables.[12] After data aggregation, we follow the modeling approach outlined in Section 4.1 and employ a DiD model with two-way fixed effects for states and weeks. We then evaluate the impact of the ad ban on households' total expenditures and the number of shopping trips.

With a second data set, we also differentiate households' expenditures at two distinct types of retailers: first, retailers that list a substantial number of products as part

Table 5. Results for Split by Customer Loyalty

	Without loyalty card		With loyalty card	
Variable	Sales revenue	No. of trips	Sales revenue	No. of trips
Ad ban	−0.065***	−0.055***	−0.041	−0.028
	(0.017)	(0.015)	(0.023)	(0.020)
Control variables	All	All	All	All
Store fixed effect	Yes	Yes	Yes	Yes
Day fixed effect	Yes	Yes	Yes	Yes
R^2	0.54	0.69	0.64	0.74
N	8,032	8,032	8,032	8,032

Notes. Robust standard errors (clustered by store) are in parentheses. We omit the control variables here to simplify the exposition. The full results are available in Online Appendix C.1.
***$p < 0.001$; **$p < 0.01$; *$p < 0.05$.

of their short-term promotional assortments for the duration of the promotion and advertise them in their weekly circulars (this retailer group includes the focal retailer of our study); second, retailers that do not list a substantial number of promotional items to attract customers. This analysis and retailer categorization reflect our previous finding, namely, that advertising promotional assortments seems to be a key driver of additional shopping trips at the focal retailer (see Section 4.3 and Online Appendix C.2).

The results in Table 6 detail the effects of the ad ban on the total market (Ad ban) and the difference in the ad ban effect between the two retailer groups (Ad ban $\times$ $\mathbb{1}$(Promo)). We find no significant change at the market level, with estimates of -0.008 (standard error (SE) = 0.025) for household expenditures and 0.002 ($SE = 0.024$) for the number of shopping trips. That is, households' expenditures seem to shift across retailers (from those offering promotional assortments to those that do not) rather than shrink. In line with this interpretation, we observe statistically significant differences in the ad ban effects between the retailer types: -0.085 for revenues ($p < 0.10$) and -0.078 for the number of shopping trips ($p < 0.05$). After the ban, revenue differences are not significantly different from zero, at the market level (0.009, $SE = 0.016$) and for both retailer types (0.007, $SE = 0.026$, and -0.001, $SE = 0.024$). The negative ad ban effect on the number of shopping trips for retailers with substantial promotional assortments disappears; we even observe a positive difference in the number of shopping trips after the ad ban, and even among retailers offering promotional assortments. In Online Appendix E, we clarify that the difference mostly occurs in the week following each of the two school holidays after the ban, and in June 2021.

Because these results are consistent with our findings at the focal retailer, they help underscore the importance of advertising for retailers offering promotional assortments to attract consumers. However, we acknowledge that the relatively small sample size for the household panel analysis limits its statistical power.

6. Sensitivity of the Results to the COVID-19 Pandemic

We conducted two additional analyses to consider whether and how the COVID-19 pandemic affected the results. First, we leverage variations in mobility, COVID-19 incidence rates, and vaccination rates at the county level (see Table 2). The *MobilityChange* variable measures changes in customers' daily mobility relative to the same month in 2019. We estimate group-specific treatment effects by splitting observations into high (1.7%) and low (−10.9%) mobility.[13] The results, displayed in Table 7, reveal no significant differences in the treatment effect across the mobility levels; the ad ban's impact on retailer performance does not appear to be significantly affected by pandemic-related mobility changes. The zero mobility delta suggests that current mobility levels are identical to those recorded during the prepandemic reference month (2019), such that mobility in the high-mobility group is slightly above prepandemic levels. We also repeat this analysis with different COVID-19 incidence rates, reflecting the number of COVID-19 cases per 100,000 people over seven days. When we compare the counties with the highest (8.0%) and lowest (4.2%) incidence rates during the ad ban, we find no significant differences in the treatment effect. Likewise, analyzing the impact of COVID-19 vaccination rates reveals no significant differences in treatment effects (13.6% versus 3.4%). We report the full results for all analyses in Online Appendix F.1.[14]

Table 6. Market-Level Effects: Results for DiD Model with Two-Way Fixed Effects

	Revenue		No. of trips	
Variable	Market level	Retail type	Market level	Retail type
Ad ban	−0.008	0.022	0.002	0.020
	(0.025)	(0.042)	(0.024)	(0.0.31)
Ad ban × $\mathbb{1}$(Promo)		$-0.085^{\dagger}$		−0.078*
		(0.045)		(0.033)
Post × Treatment state	0.009	0.007	0.052**	0.025
	(0.016)	(0.026)	(0.016)	(0.019)
Post × Treatment state × $\mathbb{1}$(Promo)		−0.001		0.056**
		(0.024)		(0.018)
$\mathbb{1}$(Promo)		−0.363***		−0.529***
		(0.013)		(0.010)
State fixed effects	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes
N	50	100	50	100

Notes. We estimate four models for two dependent variables (revenue and no. of trips) and two different levels of aggregation (market level and two retail types, retailers with and without promotional assortments). Promo refers to retailers that list many products as part of their short-term promotional assortments for the duration of the promotion and advertise them in their weekly circulars.

***$p < 0.001$; **$p < 0.01$; *$p < 0.05$; $^{\dagger}p < 0.10$.

Table 7. Regression Results for Sensitivity Analyses (Sales Revenue)

Variable	Main model	Mobility model	Incidence model	Vaccination model
Ad ban	−0.060**	−0.061**	−0.056**	−0.059**
	(0.018)	(0.019)	(0.018)	(0.019)
Ad ban × 𝟙(High Mobility)		0.001		
		(0.014)		
Ad ban × 𝟙(High Incidence)			−0.009	
			(0.010)	
Ad ban × 𝟙(High Vaccination)				−0.005
				(0.011)
Control variables	All	All	All	All
Store fixed effects	Yes	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes	Yes
R^2	0.54	0.54	0.54	0.54
N	8,032	8,032	8,032	8,032

Notes. Robust standard errors (clustered by store) are in parentheses. We report the full estimation results for all analyses in Online Appendix F.1.
***$p < 0.001$; **$p < 0.01$; *$p < 0.05$.

Second, we assess the effect of COVID-19 incidence rates on customers' total grocery expenditures, using data from the GfK Household Panel (2019–2022). This data set contains the expenditures of the 1,800 households we included in the market-level analysis (Section 5). By aggregating household expenditures at the state-month level and employing a linear model, we can estimate the relationship between expenditures and COVID-19 incidence rates. We also control for economic indicators and seasonality. The findings indicate that the incidence rates of COVID-19 do not significantly correlate with aggregate expenditures during these periods, in the treated state ($Incidence_{\text{treated}} = 0.309$, $SE = 0.683$) or in the control state ($Incidence_{\text{control}} = -0.034$, $SE = 0.964$). We provide more details in Online Appendix F.2.[15]

In summary, the varying severity of the pandemic across regions (measured by mobility, incidence, and vaccination rates) does not significantly alter the effect of the advertising ban on retailers' performance. Similarly, customers' overall expenditures seem stable, despite varying intensity levels of the pandemic. Two possible explanations for these results are the essential nature of grocery shopping, which typically results in less variability and elasticity in consumer demand over time, and the slow adoption of online grocery shopping in Germany.[16] Nonetheless, we acknowledge that these analyses cannot entirely rule out some potential influences of the timing of the ad ban, during the COVID-19 pandemic.

7. General Discussion

We investigate the impact of an advertising ban on retail performance. Our identification strategy is based on a natural experiment in March 2021 that affected grocery retailers in one of Germany's federal states. The ad ban stopped retailers' print advertising in the treated state for three weeks. Using a DiD research design, we contrast the effect of the ad ban on retailer performance for stores in the treated state with stores of the same retailer in the adjacent control state. The findings reveal that the ad ban reduced sales by 6% because of fewer shopping trips; the size of shopping baskets did not change.

The ad ban decreases sales revenue and the number of shopping trips for customers without loyalty cards. The effect was not statistically significant for loyalty card customers. Moreover, the ad ban only affects the revenue and number of shopping trips for shopping baskets that include at least one promotional item, and it is significant only on the days when promotional products become available. As possible explanations for these findings, we suggest the influences of higher switching costs and stronger brand loyalty toward competing retailers of customers without loyalty cards, as well as the informative role of advertising in driving store traffic (Nelson 1974).

When we analyze the market-level effects of the advertising ban, using a second data set from the GfK Household Panel, we find no significant change in revenue and shopping trips at the overall market level. However, the ad ban exerted a negative effect on revenue and shopping trips for retailers offering promotional assortments, which is consistent with our findings at the focal retailer.

These findings have significant implications for both regulators and retailers. First, our results highlight the importance of print advertising for retailers. The 6% reduction in sales resulting from the ban is considerable for the retail industry, revealing the crucial role of advertising in drawing customers to stores. Should any regulation ban print advertising, retailers must intensify their efforts to develop alternative (e.g., digital) advertising channels to attract customers with higher switching costs (from other retailers). These strategies likely cannot be limited to proprietary apps or loyalty cards; they need to include other digital channels for advertising weekly price promotions or geo-targeting provided by general shopping apps (Molitor et al. 2020).

Second, retailers and policymakers can use our methods to quantify the impact of a ban on print advertising.

Retailers proactively considering discontinuing print advertising (REWE Group 2023) could evaluate the trade-off between the financial benefits of advertising (e.g., sales and profit impact) and the associated costs or environmental footprint. The ability to assign a price tag to bans can foster more effective communication between retailers and policymakers. A simple back-of-the-envelope analysis indicates that halting all print advertising for a single grocery retailer in Germany would save approximately 76,335 tons of CO_2 per week. At a cost of \$185 per ton of CO_2 (Rennert et al. 2022), it represents a weekly social cost of €12.7 million—more than the estimated €11.5 million of incremental sales achieved through print advertising (see Online Appendix G). In addition, discontinuing all print advertising would save 1.3 million trees annually (see Online Appendix H).

Third, our findings can inform policy decisions if policymakers attempt to reduce store traffic or limit face-to-face contact because of public health concerns. Banning retail advertising is a relatively unobtrusive regulation (cf. complete store closures) that can reduce shopping trips. However, we note that the ad ban did not affect consumers' overall mobility.

Our identification strategy uses a natural experiment supported by various robustness checks and a series of placebo tests, which collectively lend credibility to our findings. We acknowledge two potential limitations. First, analyzing the ad ban effect during the COVID-19 pandemic may influence estimates of the ban's impact. During the pandemic, consumers displayed a reduced propensity for shopping trips and a heightened focus on essential purchases, so ad bans in nonpandemic conditions could have larger effects. We find a positive relationship between the change in mobility and the number of shopping trips (Table 4) but no significant differences in the treatment effect across different mobility levels (Table 7). Similarly, we detect no significant differences in the treatment effect across varying COVID-19 incidence and vaccination rate levels. However, we cannot completely rule out the possibility of other unobserved effects.

Second, our analyses are based on data from one retail chain and the GfK Household Panel. These data can only provide some insights into the behavioral mechanisms. They suggest that the ad ban primarily reduces the number of shopping trips to the focal retailer. Further exploration of the effect mechanism reveals that the effect of the ad ban is not statistically significant for loyalty card customers. The cross-retailer household panel data analysis demonstrates that the ad ban has no effect at the market level but that it shifts customers' expenditures among retailers. This outcome might reflect our specific study context, in the sense that grocery retailers sell products essential for daily life. Experimental studies of the behavioral mechanisms and the study of the effects of ad bans in nongrocery retail contexts provide interesting avenues for further research.

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Endnotes

[1] See, for example, https://www.greenpeace.org/international/story/49826/ban-fossil-fuel-ads-and-sponsorships/, or https://verbiedfossielereclame.nl/only-words/.

[2] See theglobeandmail.com. Some retailers have already tested stopping some of their printed leaflets voluntarily, claiming an annual reduction of up to 73,000 tons of paper and a significantly reduced CO_2 footprint (REWE Group 2023).

[3] All leading retailers in the treatment and control regions, being part of national chains with similar advertising strategies, stopped the distribution of print advertising in the treated state during the ad ban period.

[4] With one million inhabitants, Saarland is comparable in size and population to the U.S. state of Rhode Island.

[5] For confidentiality reasons, we cannot reveal the retailer's identity.

[6] Available from the German Center for Disease Control, the COVID-19 mobility project, the German weather service, and the Federal Employment Agency.

[7] The average distance between a retail store and the closest weather station is 18.03 kilometers.

[8] Number of COVID-19 cases per 100,000 inhabitants over the past seven days; we divide this metric by 1,000.

[9] Beyond using *VaccinationRateDose1*, *Incidence*, and *MobilityChange* as controls, we conduct additional analyses that use these variables as moderators of the ad ban treatment effect (see Section 6) to evaluate the sensitivity of our results to the COVID-19 pandemic.

[10] We also conduct robustness checks related to our model specification (e.g., store selection and control variable selection). We provide details in Online Appendix D.4.

[11] Analyzing pre-ad-ban sales data, we find no significant differences in loyalty card penetration between the states, including the fraction of baskets and revenue with loyalty cards.

[12] The low number of households in the focal state, combined with a weekly household penetration between 1.1% and 23.0% (fraction of households buying at a given retailer in a given week), renders estimating models at the chain level infeasible.

[13] We use an interaction between the treatment effect and dichotomized control variables to simplify interpretability and exposition; interacting the treatment effect with continuous control variables does not change our key findings (see Online Appendix F.1).

[14] In a related analysis, we evaluate how the effect of the ad ban differs across product categories by comparing the treatment effects in categories that were more affected by the pandemic with the treatment effects in categories that were less affected by the pandemic (based on the findings of Zuokas et al. 2022). We find comparable ad ban effects, for example, −4.9% for cereals and −5.2% for spirits (both less affected by the pandemic), and −5.2% for flour and baking and −4.3% for cleaning products (both more affected by the pandemic).

[15] We also evaluated whether customers' shopping behavior at the focal retailer systematically differed during and after the pandemic and whether the focal retailer changed its assortment during the

pandemic. Neither analysis showed meaningful differences; see Online Appendix F.3 for details.

[16] See https://de.statista.com/statistik/daten/studie/475255/umfrage/marktanteil-des-online-handels-am-umsatz-mit-lebensmitteln-in-deutschland/.

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