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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 reveals 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.

Keywords: Advertising Effectiveness, Retailing, Advertising Ban, Natural Experiment, Sustainability

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., Dhar and Baylis 2011, Hamilton 1972, 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 requires 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 (Supermarket News 2021) and rely heavily on non-digital 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 26% of U.S. advertising spending (Federal Reserve Economic Data 2022, Statista 2021). 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 non-food products at grocery retailers in Saarland, one of Germany's 16 federal states. The state implemented the ad ban in the spring of 2021, while all other states, including its only adjacent state Rhineland-Palatinate, remained unaffected. Due to 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 $\times$ day observations indicates that the ad ban led to a 6% decrease in sales revenue in the

[1] See, for example, banfossilfuelads.org, greenpeace.org, or verbiedfossielereclame.nl.
[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).

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 (Dhar and Baylis 2011, Nelson 2004, Saffer and Chaloupka 2000). 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 non-digital advertising on store choice and retailer performance is well recognized (Blake et al. 2015, Bodapati and Srinivasan 2006, Srinivasan et al. 2004), partly due to 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 non-food products in March 2021 in response to lobbying efforts by non-food retailers who opposed Germany's COVID-19 retail policy. In December 2020, the German government initiated a nationwide shutdown of non-essential businesses, including all non-food retailers, to limit the spread of the virus. Grocery retailers and other essential businesses were allowed to remain open. Non-food retailers perceived this policy as discriminatory, mainly because all grocery retailers sell and advertise at least some non-food 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 non-food products as part of their short-term promotional assortments for the duration of the promotion—typically for one week. Non-food retailers, therefore, advocated for a ban on non-food advertising to help level the playing field.

Responding to this pressure, Saarland announced the possibility of an ad ban on non-food 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 non-food advertising, effective February 22. Non-compliance 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 non-food 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, only accounts for 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 due to 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

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

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 non-food 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 non-essential 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 Web Appendix D.4; the key findings do not change.

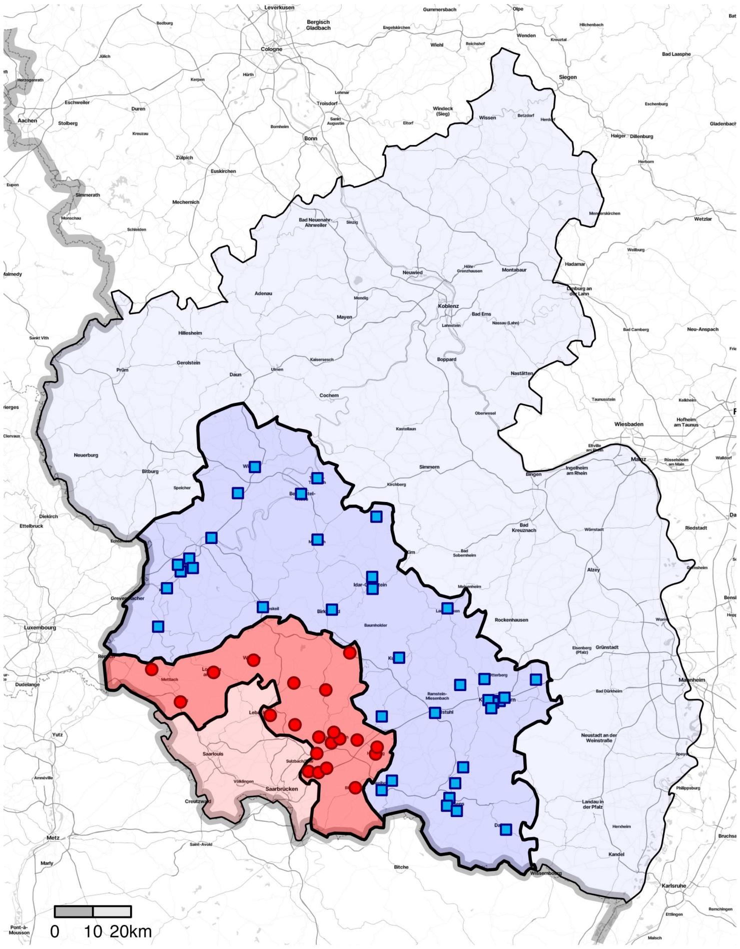
advertising variation at the margin (e.g., promoted brands, size of leaflets); our setup allows us to evaluate the complete discontinuation of advertising. Second, the advertising ban solely affected advertising distribution in the treated state. In contrast, it did not impact the advertising content 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 $\times$ 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

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

Figure 1. Store Locations in the Treatment State and Control State



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

(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

[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.

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 pre-treatment trends for treated and untreated (control) units in Web 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 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 mm/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 below 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 Web 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 *Vaccination-RateDose1* 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

[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.

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

NOTES: 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.

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.

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%]

NOTES: The variables are multiplied by the same scaling factors as in Table 2. ** $p < 0.01$ and * $p < 0.05$.

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:

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

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 post-ban period in the treated state (Post $\times$ 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.

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,

Table 4. Main Regression Results

Variable	Sales Revenue	Number of Trips	Basket Size
Ad ban (β)	−0.060 *** (0.017)	−0.051 *** (0.014)	−0.008 (0.007)
Incidence	0.419 (0.244)	0.223 (0.154)	0.165 (0.084)
Delta incidence	−0.198 (0.479)	−0.048 (0.331)	−0.149 (0.165)
Vaccination rate dose 1	−0.052 (0.069)	−0.042 (0.055)	−0.008 (0.029)
Vaccination rate dose 2	0.089 (0.099)	0.083 (0.077)	−0.003 (0.041)
Mobility change	0.199 (0.101)	0.242 ** (0.062)	−0.045 (0.047)
Unemployment	0.048 (0.025)	0.022 (0.021)	0.024 (0.013)
Rain	−0.013 (0.013)	−0.014 * (0.006)	0.002 (0.003)
Post × Treatment state	−0.027 (0.017)	−0.015 (0.014)	−0.011 (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

NOTES: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Robust standard errors (clustered by store) are in parentheses.

possibly due to 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 Web 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. Web Appendix D.2 contains further details regarding the analyses and results. Third, we use a Bayesian time-series model (Brodersen et al.

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

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% 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 0 (CI = [−3.3%, 3.0%]). We provide further details for this analysis in Web Appendix D.3.

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 only available during the promotion; they might include food, near-food items (e.g., laundry care), and non-food 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. Non-promotion 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

[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.

Table 5. Results for Split by Customer Loyalty

	Without Loyalty Card		With Loyalty Card	
	Sales Revenue	**N Trips**	**Sales Revenue**	**N Trips**
Ad ban	−0.065 *** (0.017)	−0.055 *** (0.015)	−0.041 (0.023)	−0.028 (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: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. 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 Web Appendix C.1.

the promotional assortment, and non-promotion 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 Web 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 ($N = 50$, two 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 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 Web 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 ($SE = 0.025$) 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 Web 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.

[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.

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.

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

	Revenue		**N Trips**	
	Market-Level	**Retail Type**	**Market-Level**	**Retail Type**
Ad ban	−0.008 (0.025)	0.022 (0.042)	0.002 (0.024)	0.020 (0.0.31)
Ad ban × $\mathbb{1}$(Promo)		−0.085 † (0.045)		−0.078 * (0.033)
Post × Treatment state	0.009 (0.016)	0.007 (0.026)	0.052 ** (0.016)	0.025 (0.019)
Post × Treatment state × $\mathbb{1}$(Promo)		−0.001 (0.024)		0.056 ** (0.018)
$\mathbb{1}$(Promo)		−0.363 *** (0.013)		−0.529 *** (0.010)
State fixed effects	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes
N	50	100	50	100

NOTES: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, † $p < 0.10$. We estimate four models for two dependent variables (Revenue and N 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.

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

[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 Web Appendix F.1).

are identical to those recorded during the pre-pandemic reference month (2019), such that mobility in the high mobility group is slightly above pre-pandemic 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 county 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% vs. 3.4%). We report the full results for all analyses in Web Appendix F.1.[14]

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 Web 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.

[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% 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 Web Appendix F.3 for details.

[16] See de.statista.com.

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

Variable	**Main Model**	**Mobility Model**	**Incidence Model**	**Vaccination Model**
Ad ban	−0.060 ** (0.018)	−0.061 ** (0.019)	−0.056 ** (0.018)	−0.059 ** (0.019)
Ad ban × $\mathbb{1}$(High Mobility)		0.001 (0.014)		
Ad ban × $\mathbb{1}$(High Incidence)			−0.009 (0.010)	
Ad ban × $\mathbb{1}$(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: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Robust standard errors (clustered by store) are in parentheses. We report the full estimation results for all analyses in Web Appendix F.1.

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% due to 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 Web Appendix G). In addition, discontinuing all print advertising would save 1.3 million trees annually (see Web Appendix H).

Third, our findings can inform policy decisions if policymakers attempt to reduce store traffic or limit face-to-face contact due to 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 non-pandemic 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 non-grocery retail contexts provide interesting avenues for further research.

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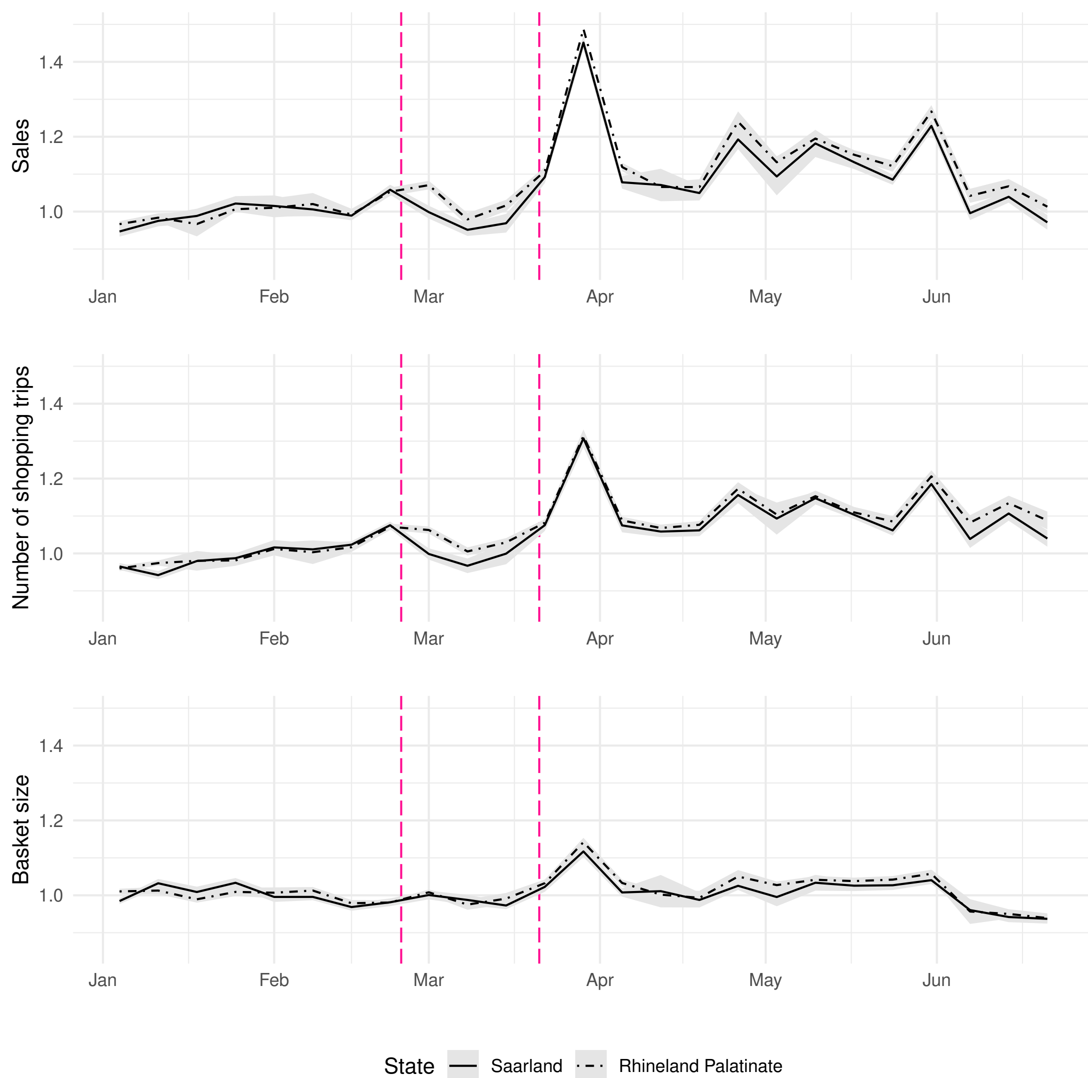
Web Appendix for

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

Web Appendix A Time Series Plots

Figure A1 below depicts time series plots for all dependent variables. The plots indicate the presence of parallel trends before the start of the ad ban, as marked by the first vertical line. The increase in sales, number of shopping trips, and basket size in April coincides with the Easter week. The retailer reports that similar peaks occur every year.

Figure A1. Time-series plots for all dependent variables



NOTE. Treatment state Saarland (solid line), and the control state Rhineland-Palatinate (dot-dashed line). Vertical lines indicate the start and end of the ad ban.

Web Appendix B Analysis of Mobility Data

To substantiate the validity of using mobility as a moderator, we study mobility change as a dependent variable. The data set is identical to the main analysis, and we use the DiD regression approach to evaluate (1) the effect of the ad ban and (2) the influence of control variables on mobility. As seen in Table A1, the Ad ban indicator does not significantly influence mobility change, suggesting that mobility is not correlated with the ad ban. This result supports the discriminant validity of the treatment variable Ad ban. This also indicates that changes in mobility caused by the COVID-19 pandemic are insufficient to explain the sales differences observed during the ad ban. Moreover, COVID-19 incidence and rainfall significantly reduce mobility, whereas vaccination rates increase mobility. This further validates the effectiveness of the control variables and the meaningfulness of using mobility as a moderator.

Table A1. Regression Results for Dependent Variable Mobility Delta

Variable	**Mobility Change**
Ad ban	−0.009 (0.011)
Incidence	−0.147 * (0.062)
Delta incidence	−0.034 (0.049)
Vaccination rate dose 1	0.090 ** (0.030)
Unemployment	0.045 (0.023)
Rain	−0.014 *** (0.002)
Post × Treatment state	−0.021 (0.014)
Store fixed effects	Yes
Day fixed effects	Yes
R^2	0.93
N	8,032

NOTES: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Robust standard errors (clustered by store) are in parentheses.

Web Appendix C Supplementary Information for the Heterogeneity Analysis

C.1 Loyalty Card Customers

Following the heterogeneity analysis in the central part of the paper, Table A2 shows the full results of the loyalty card analysis, which distinguishes between customers with and without loyalty cards. We find a significantly negative ad ban effect for customers who do not use the retailer's loyalty card. Still, the ad ban effect is not statistically significant for loyalty card customers. 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) and number of trips (p = 0.284). We note that model specifications without COVID-19 controls (i.e., incidence and delta incidence) produce similar results, so control variables, particularly incidence, do not drive the non-significant effects for customers with loyalty cards.

Table A2. Regression Results for Loyalty Card and Non-Loyalty Card Customers

	Without Loyalty Card		**With Loyalty Card**	
Variable	**Sales Revenue**	**No. of Trips**	**Sales Revenue**	**No. of Trips**
Ad ban	−0.065 *** (0.017)	−0.055 *** (0.015)	−0.041 (0.023)	−0.028 (0.020)
Incidence	0.316 (0.230)	0.170 (0.144)	0.652 * (0.300)	0.398 (0.211)
Delta incidence	−0.248 (0.463)	−0.056 (0.321)	0.087 (0.561)	0.034 (0.339)
Vaccination rate dose 1	−0.032 (0.077)	−0.033 (0.061)	−0.022 (0.115)	0.001 (0.114)
Vaccination rate dose 2	0.025 (0.108)	0.040 (0.082)	0.251 (0.153)	0.237 (0.129)
Mobility change	0.187 (0.096)	0.237 *** (0.066)	0.218 (0.144)	0.261 * (0.123)
Unemployment	0.037 (0.026)	0.014 (0.024)	0.078 (0.049)	0.056 (0.048)
Rain	−0.013 (0.008)	−0.015 (0.006)	−0.008 (0.010)	−0.004 (0.006)
Post × Treatment state	−0.025 (0.017)	−0.017 (0.015)	−0.029 (0.032)	−0.002 (0.030)
Store fixed effects	Yes	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes	Yes
R^2	0.54	0.69	0.64	0.74
N	8,032	8,032	8,032	8,032

NOTES: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Robust standard errors (clustered by store) are in parentheses.

C.2 Baskets with Promotional Assortment Purchases

We also differentiate between shopping trips with promotional assortment and those without. The retailer defines promotional assortment as items only listed for the duration of the promotion, sometimes referred to as in-and-out promotions. These products include items from food, near-food (e.g., laundry care, body care), and non-food categories (e.g., home improvement, kitchen items, clothes, consumer electronics) sold as short-term promotional items. We define promotion sales revenue as the total revenue generated from shopping baskets containing at least one item from the promotional assortment. In contrast, non-promotion revenue is the total revenue from baskets without items from the retailer's promotional assortment. Similarly, promotion trips are the number of shopping baskets that contain at least one item from the promotional assortment. In contrast, non-promotion trips encompass shopping trips that result in baskets devoid of any item from the promotional assortment. The corresponding results are displayed in Table A3 below.

Intuitively, we expect the ad ban will lead to lower promotion sales revenue and fewer shopping baskets, including items from the promotional assortment. The decrease should be smaller for non-promotion sales revenue and the number of non-promotion baskets. As anticipated, the ad ban effect can be attributed to baskets containing items from the promotional assortment, as seen in Table A3. Precisely, we measure an 8.2% decrease in promotion revenue and an 11.2% reduction in promotion trips. Both effects for baskets without promotional assortment are negative but not statistically significant. A Wald test comparing the estimated coefficients of the ad ban between baskets with and without promotional assortments shows no significant difference for sales revenue ($p = 0.153$) but a significant difference for the number of trips ($p < 0.01$).

C.3 Main promotion days

Finally, we differentiate between what the retailer and customers consider the main promotion days and all other days. The main promotion days are the days when specific promotions are launched, which in our setting are Mondays and Thursdays. We expect the ad ban coefficient to be larger on these promotion days, as advertising is supposed to generate store traffic and sales. Our findings in Table A4 reveal that the effect of the ad ban is only significant on promotion days, namely, Mondays and Thursdays. On these days, the sales revenue drops by approximately 13.8%, while the number of shopping trips experiences a decline of about 11.9%. A Wald test shows that the estimated ad ban coefficients are significantly different between promotion days and other days ($p < 0.001$). The concentration of the effect on main promotion days can be attributed to the typically limited availability of the most attractive offers and promotional assortments.

Table A3. Regression Results for Baskets With and Without Promotional Assortment Purchases

	Promotion Baskets		Non-promotion Baskets	
Variable	**Sales Revenue**	**No. of Trips**	**Sales Revenue**	**No. of Trips**
Ad ban	−0.082 ** (0.025)	−0.112 *** (0.022)	−0.037 (0.019)	−0.029 (0.016)
Incidence	0.421 (0.261)	0.248 (0.156)	0.366 (0.242)	0.196 (0.156)
Delta incidence	0.016 (0.571)	0.005 (0.423)	−0.286 (0.464)	−0.068 (0.323)
Vaccination rate dose 1	−0.041 (0.101)	−0.071 (0.086)	−0.142 (0.088)	−0.111 (0.065)
Vaccination rate dose 2	−0.009 (0.149)	−0.041 (0.126)	0.212 (0.127)	0.165 (0.092)
Mobility change	0.127 (0.098)	0.171 * (0.064)	0.2229 * (0.108)	0.258 *** (0.069)
Unemployment	0.073 * (0.030)	0.050 (0.027)	0.043 (0.029)	0.017 (0.024)
Rain	−0.006 (0.017)	0.000 (0.007)	−0.015 (0.008)	−0.017 * (0.006)
Post × Treatment state	−0.012 (0.021)	0.011 (0.017)	−0.036 (0.020)	−0.023 (0.016)
Store fixed effects	Yes	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes	Yes
R^2	0.70	0.82	0.57	0.71
N	8,032	8,032	8,032	8,032

NOTES: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Robust standard errors (clustered by store) are in parentheses.

Table A4. Promotion Day Analysis

Variable	**Sales Revenue**	**No. of Trips**
Ad ban Mon & Thu	−0.138 ***	−0.119 ***
	(0.017)	(0.014)
Ad ban other days	−0.020	−0.016
	(0.018)	(0.014)
Incidence	0.415	0.220
	(0.244)	(0.154)
Delta incidence	−0.132	0.009
	(0.481)	(0.331)
Vaccination rate dose 1	−0.050	−0.040
	(0.069)	(0.055)
Vaccination rate dose 2	0.086	0.081
	(0.099)	(0.077)
Mobility change	0.197	0.240 ***
	(0.101)	(0.062)
Unemployment	0.048	0.022
	(0.025)	(0.021)
Rain	−0.012	−0.014
	(0.008)	(0.006)
Post × Treatment state	−0.026	−0.015
	(0.017)	(0.014)
Store fixed effects	Yes	Yes
Day fixed effects	Yes	Yes
R^2	0.55	0.67
N	8,032	8,032

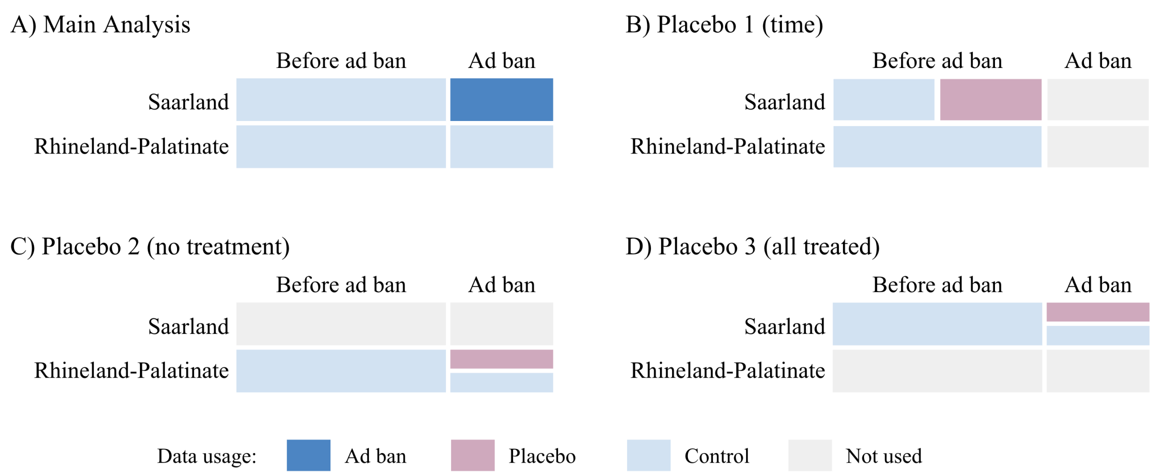
NOTES: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Robust standard errors (clustered by store) are in parentheses.

Web Appendix D Robustness Analyses

D.1 Results for Placebo Tests

We conducted three placebo tests to verify that the model in Equation (1) does not capture an ad ban effect when there is no reason to expect one. Figure A2 below summarizes our approach.

Figure A2. Specification of Placebo Checks



Panel A depicts the logic of the main analysis. We define alternative time windows in the first placebo test (Panel B). The data set comprises store-level sales in Saarland (treated) and Rhineland-Palatinate (control), and the ad ban was in effect in Saarland for three weeks. The first placebo test only uses the first eight weeks of the data, identical to the pre-treatment window in the main analysis. During this time, sales cannot be affected by the ad ban. Specifically, we define the first four weeks of this period as the pre-treatment window (light blue) and weeks 5 to 8 in Saarland as the placebo ad ban (pink). The remaining observations (grey) are not used. We base the analysis on the entire time window in the second placebo test (Panel C). However, we only consider stores in Rhineland-Palatinate, the control state unaffected by the ad ban. We randomly split the store set into two equally sized sets, S1 and S2, and then define a placebo ad ban in store set S1 (pink). The remaining observations serve as pre-treatment or control observations (light blue). The observations in Saarland are not used. Third, we repeat the logic of the second placebo test but instead only focus on stores in Saarland, the treated state, which was directly affected by the ad ban (Panel D). Again, we randomly split the store set into two equally sized sets (i.e., S1 and S2) and assume that the ad ban only affects stores in set S1 (pink). The remaining observations serve as pre-treatment or control observations (light blue). The observations in Rhineland-Palatinate (grey) are not used. Table A5 depicts the results for all placebo tests. We find null effects for the placebo ad ban coefficient in all three tests.

Table A5. Results for Placebo Tests (Sales Revenue)

Variable	**Pre-Treatment Observations**	**Only Control State**	**Only Treated State**
Ad ban	−0.015 (0.027)	−0.012 (0.028)	0.006 (0.017)
Incidence	0.417 (0.292)	0.654 (0.345)	−0.217 (0.253)
Delta incidence	0.076 (0.678)		
Vaccination rate dose 1	−0.559 (0.541)	−0.086 (0.093)	0.099 (0.145)
Vaccination rate dose 2	0.123 (0.834)	0.120 (0.143)	−0.107 (0.197)
Mobility change	0.144 (0.388)	0.234 (0.117)	−0.004 (0.211)
Unemployment	0.029 (0.118)	0.048 (0.027)	0.001 (0.041)
Rain	−0.052 (0.044)	−0.019 (0.012)	−0.019 ** (0.005)
Post × Treatment state		−0.016 (0.031)	0.001 (0.017)
Store fixed effects	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes
R^2	0.43	0.48	0.77
N	2,718	5,157	3,019

NOTES: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Robust standard errors are in parentheses. Model 1 only uses data before the ad ban, so we exclude the Post dummy. Models 2 and 3 only use data from one state, so we exclude the control variable delta incidence, which measures the incidence difference between the two states.

D.2 Potential Differences Between Treated and Control State

We conduct additional robustness checks to ensure that potential differences between the treated and control states do not affect our results. First, we noted slight differences in the reopening schedules of non-essential businesses between the treatment and control states. Specifically, non-essential stores in the control state reopened on March 8, while their counterparts in the treated state were allowed to open on March 10. This resulted in a two-day period where stores in the control state were already open, potentially increasing competition within the control state and possibly decreasing store traffic in the stores in the control state.

To evaluate any potential impact of these differences, we removed all observations from the second week of the ad ban (March 8–14), which include the two days on which non-essential stores in the control state were already open while non-essential stores in the treatment state remained closed. The estimation results, as shown in Table A6 below, reveal a slightly larger ad ban effect, but differences to the main results in Table 4 are not significant. This suggests that removing the second week of the ad ban does not significantly alter our key findings.[17]

Table A6. Regression Results for Data Excluding Week 2

Variable	**Sales Revenue**	**Number of Trips**
Ad ban	−0.072 *** (0.017)	−0.055 *** (0.014)
Incidence	0.409 (0.255)	0.236 (0.160)
Delta incidence	−0.206 (0.483)	−0.055 (0.334)
Vaccination rate dose 1	−0.048 (0.072)	−0.045 (0.057)
Vaccination rate dose 2	−0.089 (0.102)	0.089 (0.078)
Mobility change	0.199 (0.109)	0.249 *** (0.065)
Unemployment	0.051 * (0.025)	0.023 (0.021)
Rain	−0.013 (0.009)	−0.014 * (0.007)
Post × Treatment state	−0.027 (0.017)	−0.015 (0.014)
Store fixed effects	Yes	Yes
Day fixed effects	Yes	Yes
R^2	0.54	0.66
N	7,696	7,696

NOTES: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Robust standard errors (clustered by store) are in parentheses.

[17] Both states had similar general social distancing requirements in place, requiring a minimum distance of 1.5 m from other persons. We note that Rhineland-Palatinate had a slightly stricter regulation for business spaces over 800 m^2 in place during the entire duration of our study (i.e., before, during, and after the ad ban). For the first 800 m^2, the regular distance of 1.5 m applied; for each m^2 above 800 m^2, a retailer was allowed one customer per 20 m^2. The effective social distance depends on the store's size. The typical size of German retail stores similar to the ones we consider ranges from 700 m^2 to 1400 m^2. This results in an effective social distance requirement between 1.5 m and 1.8 m. This regulation did not change during the entire measurement period, so the fixed effects in our DiD model should account for potential differences in sales revenues and store visits between the states. We note that Rhineland-Palatinate also introduced a regulation for business spaces above 2000 m^2 (for each m^2 above 2000 m^2, the retailer was allowed one customer per 40 m^2, which results in an effective distance of 2.2 m for a retailer with a store size of 2500 m^2). However, the stores in our study fall well below the threshold of 2000 m^2, so we believe that this restriction does not affect our key findings.

Second, we identified differences in the socio-economic development between the treated and control states between 2019 and 2021, displayed in Table A7 below. We already control for unemployment in our models. However, differences in population growth and GDP—with the disparity in GDP becoming particularly noticeable in 2021—could potentially impact consumer demand for grocery retailing.

Table A7. Socio-economic Indicators

Variable	**State**	**2019**	**2020**	**2021**
GDP	Saarland (Treated)	−2.0%	−4.9%	1.3%
	Rhineland-Palatinate (Control)	0.5%	−3.5%	8.7%
Population	Saarland (Treated)	−0.37%	−0.29%	−0.17%
	Rhineland-Palatinate (Control)	0.22%	0.11%	0.20%

NOTES: Sources: Saarland State Statistical Office (https://www.saarland.de/stat/DE/home/home_node.html) and State Statistical Office of Rhineland-Palatinate (https://www.statistik.rlp.de).

D.3 Results for Bayesian Structural Time-Series Model

The identification strategy used in the main text measures the effect of the ad ban on sales revenue by comparing the revenues of stores in the treated state (affected by the ad ban) with the revenues of stores in the control state (not affected by the ad ban). In this Web Appendix, we leverage an alternative identification strategy that only relies on the weekly sales revenue in the treated state. Specifically, we aggregate the sales revenue across all stores and all days of the week and normalize the values based on Week 1. We then use a Bayesian Structural Time-Series model (Brodersen et al. 2015) to infer the causal impact of the ad ban on the aggregated sales revenue. This diffusion-regression state-space model predicts the counterfactual revenue level for the focal state using a synthetic control, simulating a scenario that would have occurred without the ad ban.

Table A8 summarizes the key results. We observe a 5.6% reduction in sales revenue in the treated state. The result is statistically significant and in line with the main analysis. We then repeat the analysis for the control state, which was unaffected by the ad ban. As expected, we do not find a significant change in sales revenue in the control state.

Table A8. Bayesian Structural Time-Series Model Regression Results

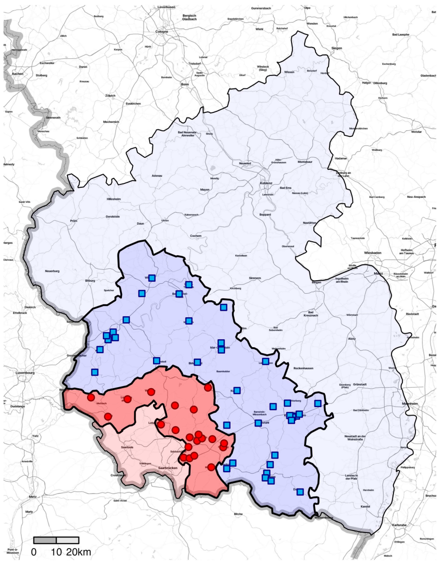
Dependent Variable	**State Affected by Ad Ban**	**State Not Affected by Ad Ban**
Relative effect	−5.60% (1.51%)	−0.061% (1.6%)
95% CI	[−8.63%, −2.49%]	[−3.3%, 3.0%]
Absolute effect	−0.063 (0.018)	−0.001 (0.017)
95% CI	[−0.100, −0.027]	[−0.037, 0.032]
Posterior probability of a causal effect	99.8%	51.0%

D.4 Store Selection, Model Specification, and Control Variable Selection

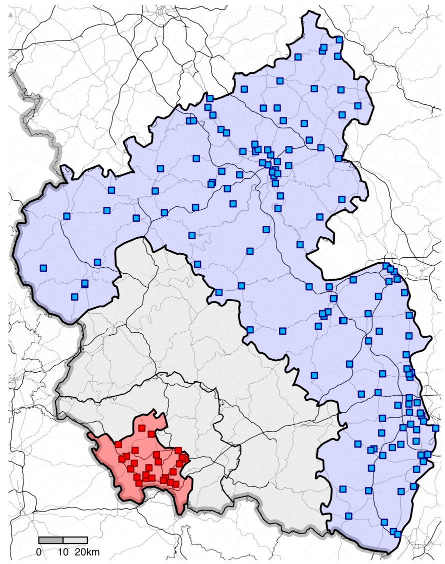
In this subsection, we first test the robustness of our results to different store selection criteria. Specifically, Store Set (b) excludes stores located within a 20 km border region between the treated and control states, Store Set (c) includes all stores in the treated state, and Store Set (d) includes all stores, irrespective of their location. The maps outlining the selected stores for each test (versus the main store specification) are shown in Figure A3 below. We caution the reader that broader store sets could introduce structural differences between the stores that might bias the measurement of the ad ban effect.

Figure A3. Maps Depicting Store Selection for Robustness Analyses

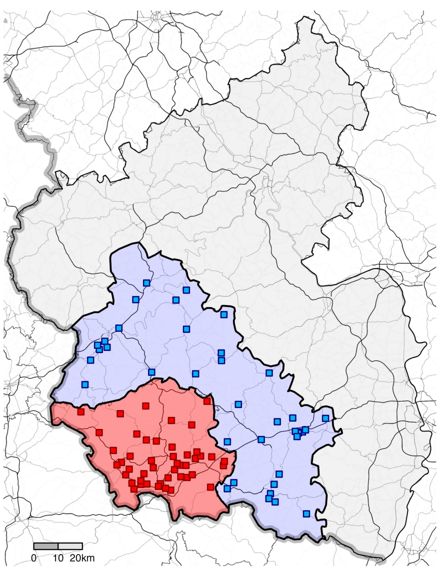
(a) Store selection in main analysis



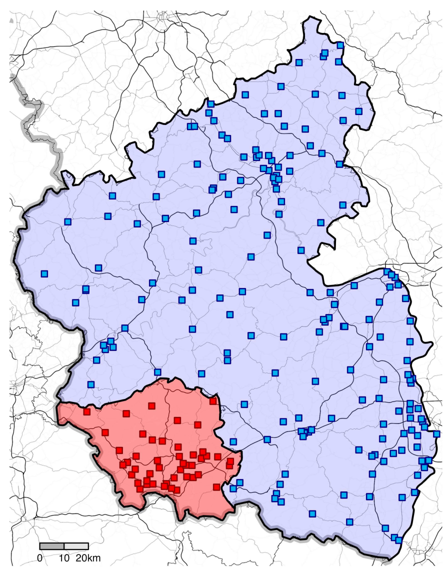
(b) Exclude stores close to state border



(c) All stores in Saarland (treated state)



(d) All stores



Second, we change the duration of the ad ban from three weeks to four weeks. According to our conversations with retailers, advertising started after the three-week ad ban (see Table 1). This additional analysis accounts for potential delays in restarting advertising after the legal end of the ban. We do not find significant differences in the measured ad ban effect across the different model specifications. Table A9 depicts the detailed estimation results.

Table A9. Model Specification (Sales Revenue)

Variable	**Main Model**	**Four Weeks w/o Ads**	**Store Set (B)**	**Store Set (C)**	**Store Set (D)**
Ad ban	−0.060 *** (0.017)	−0.056 *** (0.017)	−0.074 *** (0.016)	−0.056 *** (0.015)	−0.068 *** (0.010)
Incidence	0.419 (0.244)	0.422 (0.243)	−0.159 (0.186)	−0.254 (0.172)	−0.051 (0.150)
Delta incidence	−0.198 (0.479)	−0.293 (0.427)	−0.437 (0.492)	−0.097 (0.473)	0.146 (0.326)
Vaccination rate dose 1	−0.052 (0.069)	−0.054 (0.068)	−0.039 (0.089)	−0.080 (0.066)	0.001 (0.054)
Vaccination rate dose 2	−0.089 (0.069)	0.092 (0.098)	−0.011 (0.062)	0.074 (0.090)	−0.006 (0.055)
Mobility change	0.199 (0.101)	0.200 (0.100)	0.029 (0.032)	0.167 (0.097)	0.066 * (0.032)
Unemployment	0.048 (0.025)	0.048 (0.025)	−0.060 (0.042)	−0.004 (0.026)	−0.035 (0.029)
Rain	−0.013 (0.008)	−0.013 (0.008)	−0.005 (0.004)	−0.009 (0.008)	−0.007 (0.004)
Post × Treatment state	−0.027 (0.017)	−0.028 (0.017)	−0.058 *** (0.023)	−0.042 * (0.016)	−0.043 *** (0.012)
Store fixed effects	Yes	Yes	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes	Yes	Yes
R^2	0.54	0.54	0.45	0.54	0.47
N	8,032	8,032	22,510	11,334	30,542

NOTES: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Robust standard errors (clustered by store) are in parentheses.

Third, we test different control variable specifications for COVID-19-related controls, mobility changes, unemployment, and rain. In Table A10, we present results for models in which we systematically exclude control variables. In Table A11, we summarize results for models that use alternative control variables associated with COVID-19 parameters. The ad ban coefficient remains robust across all these models.

Table A10. Regression Results for Additional Control Variable Specifications (Sales Revenue)

Variable	**Model 1**	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**
Ad ban	−0.058 ***	−0.057 ***	−0.061 ***	−0.056 ***	−0.058 **	−0.060 **
	(0.015)	(0.015)	(0.017)	(0.010)	(0.015)	(0.016)
Incidence			0.387		0.406	0.404
			(0.245)		(0.235)	(0.239)
Delta incidence			−0.212	0.047		-0.198
			(0.485)	(0.440)		(0.477)
Vaccination rate dose 1			−0.013	-0.011	0.052	
			(0.058)	(0.065)	(0.069)	
Vaccination rate dose 2			0.032	0.012	0.089	
				(0.086)	(0.091)	(0.099)
Mobility change		0.178		0.179	0.200	0.192*
		(0.097)		(0.100)	(0.101)	(0.095)
Unemployment	0.033	0.030	0.052	0.029	0.048	0.047
	(0.023)	(0.021)	(0.026)	(0.019)	(0.025)	(0.025)
Rain	−0.015	−0.012	−0.015	−0.013	−0.013	−0.012
	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Post × Treatment state	−0.034	−0.029	−0.032	−0.029	−0.025	−0.026
	(0.018)	(0.017)	(0.019)	(0.017)	(0.018)	(0.016)
Store fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.54	0.54	0.54	0.54	0.54	0.54
N	8,032	8,032	8,032	8,032	8,032	8,032

NOTES: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Robust standard errors (clustered by store) are in parentheses.

Table A11. Regression Results for Different COVID-19 Control Specifications (Sales Revenue)

Variable	**Model 1**	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**
Ad ban	−0.060 **	−0.053 **	−0.069 ***	−0.069 ***	−0.058 **	−0.051 ***	−0.067 **	−0.056 ***
	(0.017)	(0.011)	(0.020)	(0.021)	(0.016)	(0.010)	(0.020)	(0.012)
Incidence								0.360
								(0.195)
log(Incidence)	0.456							
	(0.268)							
Cases				0.244				
				(0.164)				
log(Cases)					0.253			
					(0.166)			
Delta incidence	−0.199	0.087	−0.358	−0.453	−0.123	0.054	−0.319	−0.184
	(0.480)	(0.512)	(0.617)	(0.628)	(0.443)	(0.521)	(0.586)	(0.522)
Vaccination rate dose 1	−0.051	−0.040	−0.054	−0.025	−0.027	−0.027	−0.024	
	(0.069)	(0.071)	(0.069)	(0.070)	(0.067)	(0.068)	(0.065)	
Vaccination rate dose 2	0.087	0.072	0.088	0.035	0.038	0.046	0.032	
	(0.098)	(0.097)	(0.095)	(0.098)	(0.096)	(0.093)	(0.089)	
Incidence (lagged)		0.279						
		(0.184)						
Cases (lagged)						0.192		
						(0.108)		
Vaccination rate dose 1 (lagged)								−0.051
								(0.061)
Vaccination rate dose 2 (lagged)								0.102
								(0.094)
Incidence (moving average)			0.385					
			(0.307)					
Cases (moving average)							0.154	
							(0.188)	
Mobility change	0.200	0.108	0.197	0.186	0.189	0.102	0.183	0.115
	(0.101)	(0.063)	(0.101)	(0.107)	(0.100)	(0.062)	(0.102)	(0.058)
Unemployment	0.048	0.024	0.044	0.035	0.038	0.018	0.033	0.025
	(0.025)	(0.018)	(0.023)	(0.022)	(0.021)	(0.015)	(0.019)	(0.017)
Rain	−0.013	−0.017 *	−0.013	−0.013	−0.013	−0.017 *	−0.013	−0.017
	(0.008)	(0.007)	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	(0.007)
Post × Treatment state	−0.027	−0.021	−0.037	−0.040 *	−0.028	−0.023	−0.039	−0.023 *
	(0.017)	(0.012)	(0.020)	(0.021)	(0.017)	(0.012)	(0.020)	(0.011)
Store fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.54	0.70	0.54	0.54	0.60	0.70	0.54	0.70
N	8,032	7,336	7,696	7,696	8,032	7,336	7,696	7,336

NOTES: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Robust standard errors (clustered by store) are in parentheses.

Web Appendix E Results for Analysis of Market-Level Effects

Table A12 shows the results from the GfK Household Panel for models in which we differentiate promotion and non-promotion retailers (see Section 5). The columns "Main Model" show the model results from Table 6 in the main text. To explore what might cause the increase in the number of shopping trips after the end of the ad ban, we interact the Post × Treatment state effect with two dummy variables for (a) the two weeks after school holidays and (b) June 2021. The results show that the increase in the number of shopping trips after the end of the ad ban in the treated state (compared to the control state) mainly occurs in the two weeks following the two school holidays in the post-ban window (Easter in April, Pentecost in May) and in June 2021, while the main post treatment state effect (Post × Treatment state × $\mathbb{1}$(Promo)) is not statistically significant.

Table A12. Market-Level Effects: Results for DiD Model with Two-Way Fixed Effects

	Revenue		**N Trips**	
	Main Model	**Add. Controls**	**Main Model**	**Add. Controls**
Ad ban	0.022 (0.042)	0.022 (0.041)	0.020 (0.031)	0.020 (0.028)
Ad ban × $\mathbb{1}$(Promo)	−0.085 † (0.045)	−0.085 † (0.044)	−0.078 * (0.033)	−0.078 * (0.030)
Post × Treatment state	0.007 (0.026)	0.007 (0.026)	0.025 (0.019)	0.025 (0.018)
Post × Treatment state × $\mathbb{1}$(Promo)	−0.001 (0.024)	−0.025 (0.027)	0.056 ** (0.018)	0.020 (0.019)
$\mathbb{1}$(Promo)	−0.363 *** (0.013)	−0.363 *** (0.013)	−0.529 *** (0.010)	−0.529 *** (0.009)
Post × Treatment state × $\mathbb{1}$(Promo) × Holidays		0.075 (0.047)		0.112 *** (0.032)
Post × Treatment state × $\mathbb{1}$(Promo) × June		0.045 (0.037)		0.072 ** (0.025)
State fixed effects	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes
N	100	100	100	100

NOTES: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, † $p < 0.10$. 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.

Web Appendix F Sensitivity of Results to COVID-19 Pandemic

F.1 Variations in Mobility, COVID-19 Incidence Rates, and Vaccination Rates

To assess how the COVID-19 pandemic might have affected the results, we evaluate to which extent mobility, COVID-19 incidence rates, and vaccination rates moderate the ad ban effect. Specifically, we split observations into two groups, a group with high levels and a group with low levels for each control variable. We use a median split for mobility and vaccination rates and estimate group-specific treatment effects. For incidence, we identify the county with the highest incidence rates and estimate a county-specific treatment effect. We operationalize the split by interacting the ad ban with binary indicators for high mobility, high vaccination rates, and high incidence rates. Table A13 below provides detailed estimation results. The results suggest no significant differences in treatment effects across groups and counties. We also repeat the analysis with interactions between the treatment dummy and continuous control variables instead of binary splits, as illustrated in Table A14 below. The key findings remain unchanged.

Table A13. Regression Results for Sensitivity Analyses (Sales Revenue)

Variable	**Main Model**	**Mobility Model**	**Incidence Model**	**Vaccination Model**
Ad ban	−0.060 *** (0.018)	−0.061 ** (0.019)	−0.056 ** (0.018)	−0.059 ** (0.019)
Ad ban × $\mathbb{1}$(High Mobility)		0.001 (0.014)		
Ad ban × $\mathbb{1}$(High Incidence)			−0.009 (0.010)	
Ad ban × $\mathbb{1}$(High Vaccination)				−0.005 (0.011)
Incidence	0.419 (0.244)	0.419 (0.245)	0.419 (0.244)	0.418 (0.245)
Delta incidence	−0.198 (0.479)	−0.198 (0.479)	−0.198 (0.479)	−0.198 (0.479)
Vaccination rate dose 1	−0.052 (0.069)	−0.052 (0.069)	−0.052 (0.069)	−0.052 (0.069)
Vaccination rate dose 2	0.089 (0.099)	0.089 (0.123)	0.087 (0.099)	0.088 (0.099)
Mobility change	0.199 (0.101)	0.199 (0.101)	0.200 (0.100)	0.200 (0.101)
Unemployment	0.048 (0.025)	0.048 (0.025)	0.047 (0.025)	0.047 (0.025)
Rain	−0.013 (0.008)	−0.013 (0.008)	−0.013 (0.008)	−0.013 (0.008)
Post × Treatment state	−0.027 (0.017)	−0.027 (0.017)	−0.027 (0.017)	−0.027 (0.017)
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: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Robust standard errors (clustered by store) are in parentheses.

Table A14. Regression Results for Sensitivity Analyses, Interaction with Continuous Controls (Sales Revenue)

Variable	**Main Model**	**Mobility Model**	**Incidence Model**	**Vaccination Model**
Ad ban	−0.060 *** (0.018)	−0.060 *** (0.019)	−0.060 *** (0.018)	−0.060 *** (0.019)
Ad ban × Mobility change		−0.025 (0.102)		
Ad ban × Incidence			−0.606 (0.351)	
Ad ban × Vaccination rate				−0.024 (0.079)
Incidence	0.419 (0.244)	0.418 (0.245)	0.421 (0.244)	0.418 (0.245)
Delta incidence	−0.198 (0.479)	−0.200 (0.479)	−0.194 (0.479)	−0.199 (0.480)
Vaccination rate dose 1	−0.052 (0.069)	−0.052 (0.069)	−0.052 (0.069)	−0.052 (0.069)
Vaccination rate dose 2	0.089 (0.099)	0.089 (0.099)	0.087 (0.099)	0.088 (0.099)
Mobility change	0.199 (0.101)	0.200 (0.101)	0.201 (0.100)	0.200 (0.101)
Unemployment	0.048 (0.025)	0.048 (0.025)	0.046 (0.025)	0.048 (0.025)
Rain	−0.013 (0.008)	−0.013 (0.008)	−0.013 (0.008)	−0.013 (0.008)
Post × Treatment state	−0.027 (0.017)	−0.027 (0.017)	−0.028 (0.017)	−0.027 (0.017)
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: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Robust standard errors (clustered by store) are in parentheses.

F.2 Purchase Behavior During the Pandemic: Total Household Expenditures

We assess the impact of COVID-19 incidence on aggregated household expenditures for 2019−2022 GfK Household Panel data covering the two focal states. This dataset includes information from 400 households in the treated state and 1,400 households in the control state. We aggregate the GfK data by state and month to analyze household expenditures. Our analysis utilizes Ordinary Least Squares to examine the periods affected by the pandemic, starting from March 2020, and contrasts these with the corresponding periods in 2019. This approach directly compares sales patterns before and during the pandemic. Moreover, we incorporate unemployment rates in the model to account for economic developments, and we control for seasonality by including year and month fixed effects. The results in Table A15 reveal that the coefficients for COVID-19 incidence do not significantly impact aggregated expenditures. Specifically, the models indicate that the incidence does not yield statistically significant results in affecting expenditures, irrespective of whether the analysis is performed for both states combined, $Incidence_{\text{combined}} = 0.175\,(0.721)$, or separately for each state, $Incidence_{\text{treated}} = 0.309\,(0.683)$ and $Incidence_{\text{control}} = -0.034\,(0.964)$.

Table A15. Results for Aggregated Expenditures Models

Variable	**Both States**	**Treated State**	**Control State**
Incidence	0.175 (0.721)	0.309 (0.683)	−0.034 (0.964)
Unemployment	0.048 ** (0.001)	0.037 (0.077)	0.066 ** (0.005)
Month fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
R^2	0.43	0.64	0.77
N	96	48	48

NOTES: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

F.3 Purchase Behavior During the Pandemic: Basket Composition

Another consideration for interpreting this study's results is whether the pandemic affects customers' shopping behavior. Table 4 shows that incidence and vaccination rates do not significantly influence sales revenue and the number of trips. Mobility positively affects the number of trips, whereas higher incidence rates increase basket size. The correlation between mobility and incidence ($\rho = -0.14$ with $p < 0.001$) suggests that the two mechanisms somewhat compensate for each other. Nevertheless, neither variable appears to moderate the treatment effect (see Table A13).

To further evaluate whether customers' shopping behavior during the pandemic systematically differs from their shopping behavior after the pandemic, we analyze the retailer's loyalty card data in more detail. First, we take a demand-side perspective and evaluate the sales distribution across retail categories based on the retailer's product hierarchy. To this end, we sample 10,000 shopping baskets and calculate the fraction of customers' expenditures for each hierarchy level during the pandemic (February 2021, the month before the ad ban) and after the pandemic (February 2022). The findings indicate a strong correlation of 0.97 ($p < 0.001$) at product hierarchy level 3 (product category, $N = 364$) and a correlation of 0.98 ($p < 0.001$) at product hierarchy level 2 (department, $N = 73$).

Next, we take a supply-side perspective and assess the assortment depth offered in each retail category. This analysis includes promotional assortment items and addresses potential concerns that the retailer changed its assortment strategy during the pandemic or faced supply chain challenges. According to our conversations with the retailer, neither was the case. Similar to the previous analysis, we sample 10,000 shopping baskets and calculate the number of products for each hierarchy level during the pandemic (February 2021, the month before the ad ban) and after the pandemic (February 2022). We find a correlation of 0.99 ($p < 0.001$) at product hierarchy level 3 (product category, $N = 364$) and a correlation of 0.98 ($p < 0.001$) at product hierarchy level 2 (department, $N = 73$).

Web Appendix G Social Costs of CO_2 Emissions Related to Print Advertising

In Table A16 below, we calculate the social costs caused by CO_2 emissions associated with producing weekly leaflets for a typical German national grocery retailer. We relate this to the revenue effect of leaflet advertising.

Table A16. Business case for one example set of parameters

Variable	**Values**
Inputs	
Number of leaflets per week[a]	30,000,000
Number of pages per leaflet[a]	70
Social cost of CO_2 per ton in \$[b]	185
CO_2 for tree production per 1,000 sheets of paper in kg[c]	60
CO_2 for paper production per 1,000 sheets of paper in kg[c]	32.5
Exchange rate \$ → €[d]	0.9
Recycling rate[a]	33%
Revenue per retailer and year in billion Euro[a]	10
Relative revenue delta from leaflet[e]	6.0%
Paper and CO_2 usage	
Sheets of paper per leaflet	35
Sheets of paper per week	1,050,000,000
CO_2 for tree production in t	42,210
CO_2 for paper production in t	34,125
Business value and social costs	
Revenue delta from leaflet in Euro per week	11,538,462
Social costs of CO_2 in Euro per week	12,709,778

NOTES: a: assumptions, b: (Rennert et al. 2022), c: https://except.eco/knowledge/is-digital-more-environmentally-friendly-than-paper/ d: https://www.ecb.europa.eu/stats/policy_and_exchange_rates/euro_reference_exchange_rates/html/eurofxref-graph-usd.en.html, e: own measurement

Web Appendix H Paper and Tree Usage

Table A17 below calculates the yearly paper used to produce weekly leaflets for a typical German national grocery retailer. We relate this to the revenue effect of leaflet advertising.

Table A17. Business case for one example set of parameters

Variable	**Values**
Inputs	
Number of leaflets per week [a]	30,000,000
Number of pages per leaflet [a]	70
Weight of ream of paper (500 sheets) in kg [b]	2.34
Wood from tree in kg [b]	126
Pulping process efficiency [b]	57%
Paper density in g/m2 [a]	40
Recycling rate [a]	33%
Revenue per retailer and year in billion Euro [a]	10
Relative revenue delta from leaflet [c]	6.0%
Paper usage	
Sheets of paper per leaflet	35
Sheets of paper per week	1,050,000,000
Tree usage	
Pulp per tree in kg	72
Weight of sheet of paper	0.00468
Sheets of paper per tree	15,346
Trees per week	68,421
... adjusted for recycling	45,842
... adjusted for paper density	24,449
Trees per year (adjusted for paper density)	1,271,354
Business value	
Revenue delta caused by the leaflet in Euro per week	11,538,462
Revenue delta per tree in Euro	471.94

NOTES: a: assumptions, b: https://www.linkedin.com/pulse/how-much-paper-can-you-get-from-tree-luciano-r-oliveira/, c: own measurement

Web Appendix References

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