Cross-Border Shopping: Evidence from Household Transaction Records*

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Abstract

Cross-border shopping allows purchasing comparable goods at lower prices abroad. At the same time, it can reduce domestic consumption, sales, or tax collection. During the COVID-19 pandemic, many countries restricted cross-border movements to mitigate the virus's spread, thereby also prohibiting cross-border shopping. I exploit the random timing of the Swiss border closure using administrative data and consumer-linked transaction data from the largest Swiss retailer on 500,000 customers to identify patterns in cross-border shopping. I find that grocery expenditures temporarily increase by 10-15% in border regions, and this effect declines linearly with distance for up to 40 minutes before flattening out. The effects are stronger if prices in the cross-border location's country are lower and citizens working close to the border combine their commuting trips with cross-border shopping.

Keywords: economic geography, consumption, consumption access, consumption inequality, spatial competition

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1. Introduction

Cross-border shopping has been a growing phenomenon in many countries, particularly along national borders, where consumers can purchase goods and services at lower prices from neighboring countries. This activity increases product variety for households living close to the border and pressures domestic prices but may have adverse effects on local employment, consumption, sales, or tax collection (see Leal, López-Laborda and Rodrigo 2010; Knight and Schiff 2012, or Baggs, Fung and Lapham 2018). Nevertheless, at many of these borders, cross-border shopping suddenly stopped in the year 2020 as numerous countries imposed rigorous travel restrictions at national borders to contain the spread of COVID-19.

This paper analyzes the Swiss closure of all national borders during the COVID-19 pandemic in order to examine patterns and heterogeneities in cross-border shopping and consumer mobility. On March 16, 2020, the Swiss government mandated the immediate closure of all national borders to neighboring countries to mitigate the COVID-19 pandemic. This policy was then upheld until June 2020.[1] Additionally, the Federal Council announced the closing of all restaurants, bars, entertainment, and leisure facilities, with the exception of essential stores, including supermarkets and pharmacies.

Among countries introducing comparable policies, Switzerland is a unique case to study cross-border shopping because of two reasons. First, members of the European Union surround the country (except for the Principality of Liechtenstein), allowing Swiss citizens to purchase comparable products at lower prices in Germany, Italy, Austria, or France.[2] These countries share a common currency, facilitating comparisons for Swiss households and eliminating exchange rate differences.[3] Hence, the relative attractiveness of these countries for Swiss consumers depends solely on their variety and prices of grocery products. Second, the exact timing of the border closure was random for Swiss residents, and Burstein, Lein and Vogel (2022) show that the policy was highly

[1] The borders to Liechtenstein remained open while crossing between Lichtenstein and Germany or Austria was prohibited. Nonetheless, crossings remained possible for work-related reasons for the 370,000 workers commuting from neighboring countries into Switzerland and the 29,000 Swiss residents working abroad.

[2] Groceries in neighboring countries are 35-40% cheaper according to Eurostat. Further, importation into Switzerland is exempt from VAT for a total value below 300 Swiss francs, as long as certain limits for meat, tobacco, etc., are met.

[3] The CHF/EUR exchange rate was stable throughout this period. Therefore, the border closure was the only shock at the time.

effective, as cross-border shopping shares almost fell to zero until the reopening.

I use a difference-in-differences framework to identify the causal effect of the border closure on grocery expenditures within Switzerland by comparing households living close to a national border to households residing further inland. The estimated increase in domestic grocery expenditures measures the magnitude of cross-border shopping during open borders as customers were forced to shift these expenditures to domestic retailers. I use this setting to analyze rich heterogeneities across households' socioeconomic characteristics, cultural backgrounds, and commuting behavior. Ultimately, I calculate a distance decay function (the decline in cross-border shopping with distance). To this end, I merge the universe of customer-linked transactions from the largest Swiss retailer for the year 2020 with administrative records on labor market income and household characteristics for the entire Swiss population. The final data set contains 70 million weekly shopping baskets for 1.1 million customers that I can uniquely link to the administrative data. My findings show that domestic expenditures responded strongly and immediately to the shock. First, I find that the policy increases expenditures by 10-15% in border regions. This effect vanishes instantly and entirely once the border reopens. Therefore, cross-border behaviors are deeply rooted and resist temporary shocks. Second, I document various heterogeneities and observe that households combine their trips to work with cross-border shopping if they commute towards the border. Regarding socioeconomic characteristics, I find that expenditures of larger households increase more in response to the policy, while any differences in income or cultural backgrounds are minor. Further, I assess the role of prices and find that cross-border shopping is more pronounced in areas with cheaper neighboring countries. Third, I calculate a distance decay function that is linear until the coefficient becomes negligible after 40 minutes. This indicates an extensive margin effect of travel fixed costs, such that most individuals having to drive more than 40 minutes avoid the trip altogether. Before this threshold, variable costs appear linear in travel time.

This paper contributes primarily to the previous research on cross-border shopping. Chandra, Head and Tappata (2014) find that an appreciation of the US dollar increases the propensity to cross into Canada (and vice versa) and Campbell and Lapham (2004) analyze the retailers' response. Further, Asplund, Friberg and Wilander (2007) show that Danish tax cuts reduce alcohol sales in Sweden and Friberg, Steen and Ulsaker (2022b) estimate a hump-shaped demand elasticity for the effect of foreign price changes on store sales in Norway. While these papers shed light on broader patterns of cross-border shopping, I use customer-linked transaction data to analyze individual behavior and differences in travel costs. Following a similar approach, Friberg et al. (2022a) use Norway's COVID-19-related border closure to show that cross-border shopping reduces national tax revenues. Further, Burstein, Lein and Vogel (2022) develop a binary choice model and find substantial welfare gains from cross-border shopping for two counterfactuals: the appreciation of the Swiss Franc in 2015 and the border closure in 2020. To the best of my knowledge, this is the first study deriving rich socioeconomic heterogeneities and trip chaining in cross-border shopping from customer-linked transaction data on a high spatial precision.

In a broader context, this paper also links to the research on (i) spatial shopping and (ii) trip chaining, showing that customers deliberately plan and adapt their grocery expenditures and shopping trips. First, households travel sizeable distances for grocery shopping (see, for example, Allcott et al. 2019), and Agarwal, Jensen and Monte (2022) show that customers use the products' storability strategically for their travel length decisions. Further, Baker, Johnson and Kueng (2021) find that customers in the United States use cross-border shopping to escape local sales taxes, and Agarwal, Marwell and McGranahan (2017) show that sales tax holidays lead to substantial additional increases in spending. Second, previous work argues that customers incorporate their shopping trips consciously into their daily routine by analyzing trip-chaining between supermarkets and coffee shops (Relihan, 2021) or workplaces (Miyauchi, Redding and Nakajima, 2022).

This paper is structured as follows. Section 2 introduces the grocery and administrative data. Section 3 discusses the empirical strategy, while Section 4 presents my findings. Section 5 concludes.

2. Data

I combine unique transaction data from the largest Swiss retailer with administrative data from the Federal Statistical Office on a 100×100 meter spatial resolution.

The grocery data provides information on every customer-linked purchase at the retailer *Migros* in 2020, collected through their loyalty program in which customers identify themselves at the check-out with their loyalty card in exchange for exclusive offers and discounts. This loyalty program captures 79% of the retailer's total sales, and 2.8 million customers participate in it (i.e., 42% of all Swiss residents above legal age). Furthermore, Migros charges the same prices throughout the country, independently of local purchasing power, wages, and costs. Hence, prices are not endogenously lower close to the border. Stores of similar size also generally offer similar goods, except for local products. The data set contains the universe of 600 million customer-linked purchases for the year 2020 and provides information on individual customer characteristics, including the location of their residence coded on a grid of 100×100–meter cells, their age, and household type.

The outcome of interest throughout this analysis is a household's total expenditures in a given week. Hence, I aggregate the individual shopping trips into weekly baskets and exclude customers who moved in 2020 as well as those who spend less than 100 Swiss francs a month (110 USD on September 25, 2023), as their baskets might not capture the overall consumption accurately. This procedure generates 99 million weekly baskets for 1.7 million customers.

I enrich the purchase data with individual-level administrative records for the entire Swiss population (8.7 million inhabitants in 2020). The *Population and Households Statistics* includes individual and household characteristics, including information on gender, age, and residence location on the same 100 × 100-meter grid. The *Old Age and Survivors Insurance* provides annual gross labor market income, which I adjust by the square root of household size.[4] Finally, the administrative *Structural Surveys* 2018-2021 add education and commuting behavior for individuals participating in the survey.[5] Education is categorized as either primary, secondary, or tertiary education, and the commuting behavior includes travel times in minutes, means of transport, and the municipality of the work location.[6]

Both data sets measure addresses on the same grid containing 350,000 cells with a median population of 11 residents. Therefore, I merge the two data sets by identifying unique pairs of customers and residents using the common variables grid cell and age. This approach matches 483,000 customers uniquely to a citizen, representing 29% of regular customers and 12% of Swiss households.

[4]The calculation is income adjusted $= \frac{\text{income total}}{\sqrt{\#\text{household members}}}$, where I consider all household members, including small children. The adjustment follows one of the equivalence scales suggested by the OECD. I compute *income total* as the household's annual income by summing the income of all household members.

[5]This representative survey selects 200,000 people above age 15 every year, and participation is mandatory.

[6]Primary (or compulsory) education ends at the latest after around eleven mandatory years of school (including kindergarten). Individuals who completed high school or an upper-secondary specialized school have a secondary education. The completion of any degree at a university, university of applied Sciences, or university of teacher education results in a tertiary degree.

Table 1: Summary statistics

	Matched Customers					All Transactions				
	Mean	SD	p1	Median	p99	Mean	SD	p1	Median	p99
Costumers										
Weekly Expenditures	63	58	2	46	253	60	56	2	43	247
Weekly Shop Visits	4.4	2.9	1.1	3.7	14.1	4.5	3.1	1.1	3.8	14.7
Cardholder's Age	58	18	23	57	92	55	17	23	55	91
Income Total	89'013	124'117	0	71'900	424'250					
Income Adjusted	52'502	68'818	0	44'989	238'464					
Household Size	2.6	2.3	1.0	2.0	6.0					
Expenditures by household size										
(0,1]	38	35	2	30	162					
(1,2]	58	49	2	46	213					
(2,4]	78	66	3	62	276					
(4,10]	89	77	3	70	317					
Expenditures by income quintile										
(0,36]	59	54	2	44	241					
(36,59]	58	55	2	42	244					
(59,80]	57	52	2	42	233					
(80,109]	66	57	2	51	248					
(109,11'650]	78	67	2	61	283					
Expenditures by age quintile										
(16,36]	57	52	2	42	222					
(36,45]	80	67	3	63	279					
(45,53]	80	69	3	61	294					
(53,60]	69	62	2	52	268					
(60,104]	51	46	2	39	209					
Households	483'574					1'739'927				
Observations	27'563'708					99'175'829				

Notes: The table shows summary statistics for the customers uniquely matched to the administrative data (27 million observations) and for the full grocery transaction data (99 million observations). In both cases, I summarize weekly transaction data for the year 2020. *Income total* measures total annual household income, and *income adjusted* adjusts the annual income with the square root of household size. I display income quintiles in 1000 Swiss francs.

The final data set includes 27 million of the weekly consumption baskets.[7] Throughout the paper, I use this matched data set while running robustness checks using the complete grocery data.

Table 1 shows summary statistics. The average matched household has 2.6 members and an

[7]See Kluser and Pons (2023) and Kluser, Seidel and von Ehrlich (2022) for additional information on the two data sources and the matching procedure.

income of 52,000 CHF (adjusted for the square root of household size). The mean cardholder is 57.6 years old. Further, the average household makes four transactions and spends 63 CHF per week. Expenditures increase with household size and income, while they are hump-shaped for age. A comparison to the entire transaction data shows that the matched customers' shopping behavior matches expenditures in the full sample as well as the cardholders' age.

Finally, I calculate car travel times to foreign shopping locations as follows. (i) I scrape the location and Google review counts of all foreign supermarkets within 20 km of the Swiss border from *Google Maps*. This results in 117 cross-border locations with 2 million inhabitants and a grocery supply featuring 1,787 stores, of which 691 have at least 100 Google ratings. Table A.1 displays the largest identified cross-border locations, showing the number of stores with a certain minimum amount of Google ratings. A municipality with a large number of stores typically also has many larger stores with more than 100 or 500 Google reviews, and all correlations between the population, the number of stores, and the number of stores with more than 100 and 500 Google ratings are very high, lying between 0.83 and 0.92. (ii) As cross-border shoppers likely focus on larger stores, I define a cross-border location as a foreign municipality with at least one store that has more than 100 Google Ratings.[8] (iii) I calculate the car travel time from every raster cell to all these locations from a national online mapping service (*search.ch*) and select the shortest trip for each cell. One-fifth of all households reaches the closest cross-border location within a 30-minute car drive, while the maximum distance is three hours.

3. Empirical Strategy

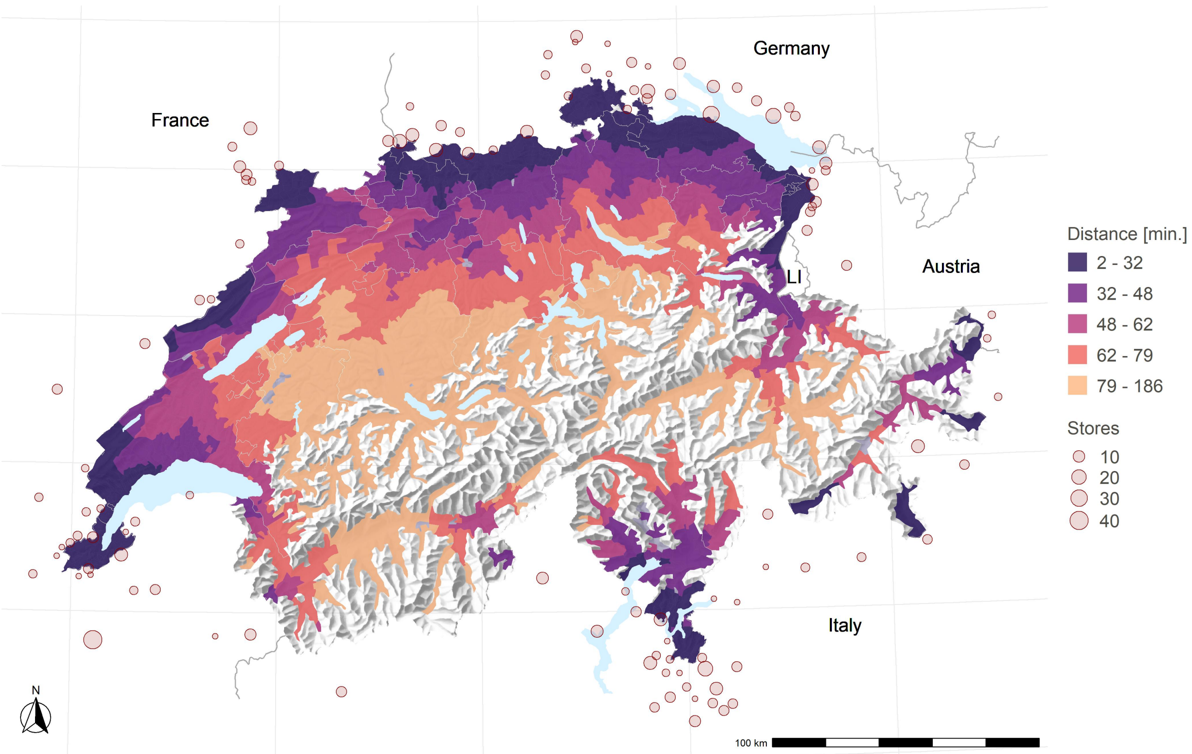
I study the impact of the border closure on household expenditures by comparing households living within a 30-minute car drive from a cross-border location (the first quintile) to those living far enough inland such that they typically do not shop abroad. Hence, I choose a comparison distance of 80 minutes (the fifth quintile) and drop all individuals living within the doughnut area in between to ensure a clean control group.[9] Figure 1 shows these travel distance bins to the closest

[8] My results are robust if I define cross-border locations alternatively as (i) locations with at least three stores with 100 Google Maps reviews or as (ii) locations with at least three stores with 500 Google Maps reviews.

[9] The results are robust if I use alternative comparison distances of 90 or 100 minutes. If a fraction of control units would still react to the border closure, my results would provide a lower bound of the effect.

foreign location across Switzerland, resulting in 348,000 treated and control households. The figure further illustrates the importance of explicitly using travel times to cross-border locations rather than the Euclidean distance to the border due to the dispersion of these shopping locations and the mountainous landscape of the area.

Figure 1: Distance to the closest cross-border shopping location



Notes: The figure shows the quintiles of car driving times to the closest cross-border shopping location on the municipality level. The dots show all 117 cross-border locations within 20 kilometers from the Swiss border, and the dots' size indicates the number of supermarkets at this location.

I use a difference-in-differences model to estimate the average treatment effect. Since all political regulations, grocery supply adaptations, and consumers' behavioral changes affect both the treatment and control group, I attribute any deviation after the intervention to cross-border shopping. As some households record zero expenditures in a given week, I follow the suggestions in Chen and Roth (2023) and Wooldridge (2023) to handle such data by estimating a QMLE-Poisson model.[10] Reporting the transformed coefficients $\beta_{ATT\%} = exp(\hat{\beta} - 1)$ gives the average proportional treat-

[10] Chen and Roth (2023) show that using a linear model with $log(Y + 1)$ as a dependent variable does not allow interpreting the coefficients as percentage changes.

ment effect, allowing to interpret the coefficients as percentage changes. I always report in the results section the transformed coefficients $\beta_{ATT\%}$ and calculate standard errors using the delta method. Therefore, I estimate the following model:

$$Y_{it} = \exp\left(\alpha_i + \gamma_t + \sum_{k=1}^{52} \beta_k (D_i \times T_k) + \tau z_{it}\right) \epsilon_{it}, \tag{1}$$

where Y_{it} are the grocery expenditures of household i in week $t \in 1, \ldots, 52$. α_i and γ_t are the household- and week-specific fixed effects, controlling for unobserved heterogeneity. D_i is an indicator variable that equals one if household i is in the treatment group, T_k indicates the week of the year 2020, and z_{it} are time-varying covariates including civil status, the number of children, and the cantonal reported cases of COVID-19. Controlling for the COVID-19 cases accounts for the differential exposure to the pandemic over time, as the first wave of COVID-19 hit Switzerland in 2020 from the South, with the largest initial number of cases in the Italian-speaking region (Ticino). Therefore, these households were sooner and stronger affected by the outbreak than people in the north, and z_{it} controls for these varying exposures, changing constantly over time. Finally, β_k are the associated pre- and post-treatment coefficients, estimating one coefficient for every week T_k of the year. Treatment starts in week twelve, and I normalize coefficients to the average in the pre-treatment period.

To analyze heterogeneities in the treatment effect, I use a static version of this model with additional dummies for the $k \in \mathcal{K}$ categories of a time-constant covariate x_i. The dummies x_{ik} equal one if $x_i = k$ and the model equation is

$$Y_{it} = \exp\left(\alpha_i + \gamma_{tk} + \sum_{k \in \mathcal{K}} \beta_k (D_i \times Post_t \times x_{ik}) + \tau z_{it}\right) \epsilon_{it}, \tag{2}$$

where β_k estimates the average treatment effect for group k. I allow the time effect to vary between the different groups k by including week-group fixed effects γ_{tk} as the pandemic might affect the individual groups differently.

Finally, adding another set of dummies δ_{il} for travel time bins $l \in \mathcal{L}$ estimates the decay of the treatment effect with distance for different household characteristics:

$$Y_{it} = \exp\left(\alpha_i + \gamma_{tk} + \sum_{k\in\mathcal{K},l\in\mathcal{L}} \beta_{kl}(D_i \times Post_t \times x_{ik} \times \delta_{il}) + \tau z_{it}\right)\epsilon_{it}, \tag{3}$$

where β_{kl} estimates the average treatment effect for group k in distance bin l.

4. Results and Discussion

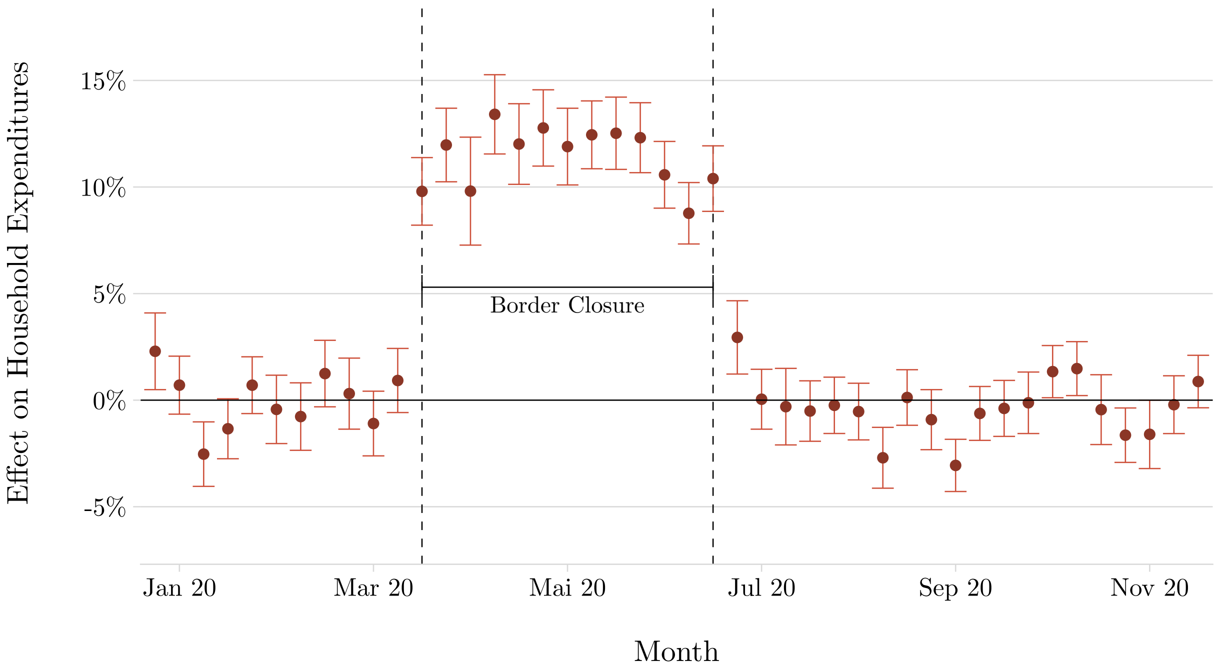
I report my empirical findings in three parts. I start by discussing (i) the dynamic average treatment effects before analyzing (ii) diverse heterogeneities of the static average treatment effect, including socioeconomic household characteristics, culture, and commuting behavior, as well as foreign grocery prices. Ultimately, I document (iii) the effect's decay with distance. I cluster standard errors in the QMLE Poisson regressions on the zip-code level and report in all tables and figures the transformed $\beta_{ATT\%}$ with corresponding standard errors based on the delta method.[11]

4.1. Dynamic treatment effects

Figure 2 shows the results for the dynamic difference-in-differences outlined in model (1). The borders close in week 12 and reopen in week 25, and both events are indicated by vertical dashed lines. I find that the border closure temporarily increases domestic grocery expenditures by 10-15% at the border in comparison to households residing further inland. Figure 2 shows further that this shift is immediate and remains constant as long as the border is impassable. After the reopening, expenditures immediately drop to the previous level. Hence, although households in border regions temporarily increased their spending at domestic supermarkets, they did not adjust their cross-border shopping behavior through the COVID-19 pandemic and completely switched back to their old behavior as soon as possible. This result suggests that cross-border shopping follows deeply rooted routines that withstand temporary shocks. There may even be a temporary catch-up effect, as some coefficients in the weeks after the reopening are below zero.

[11] Alternatively, I calculate standard errors from 1,000 clustered bootstrap replications. The bootstrapped standard errors are lower and I report the more conservative alternative.

Figure 2: Dynamic treatment effects



Notes: The figure shows the border closure's effect on household expenditures within a 30-minute car ride from a cross-border location compared to households living further away than 80 minutes. I indicate the period of border closure by vertical dashed lines. The regression estimates model (1) and uses 4.7 million observations. Coefficients are normalized to the pre-treatment periods' average, and standard errors are clustered at the zip code level.

Consumers may have adapted their shopping behavior before the actual introduction of pandemic restrictions, especially in strongly affected areas. Yet, the insignificant pre-treatment coefficients in Figure 2 indicate no violation of the parallel trend assumption between treated and control units, suggesting that households living in the border region and further inland did not react differently to the pandemic's onset. This conclusion remains unchanged (and pre-treatment coefficients insignificant) if I do not control for the local number of COVID-19 cases.

4.2. Variation across socioeconomic characteristics

Consumers may benefit differently from cross-border shopping based on their socioeconomic background. Hence, I analyze heterogeneities in the average treatment effect for different household characteristics. This corresponds to the static model in Equation (2), interacting the treatment with the household characteristic x_i I am interested in. Note that for each heterogeneity, the individual fixed effects control for all other time-constant differences between households. Table 2

Table 2: Treatment effects by socioeconomic subgroups

Dep. Variable: log(Household Expenditures)							
a) HH size		b) Age		c) Income		d) Education	
Group	Coeff	Group	Coeff	Group	Coeff	Group	Coeff
1	0.067***	(18,40]	0.140***	Q1	0.132***	Primary	0.112***
	(0.016)		(0.029)		(0.024)		(0.022)
2	0.102***	(40,51]	0.143***	Q2	0.125***	Secondary	0.108***
	(0.020)		(0.030)		(0.024)		(0.022)
3-4	0.149***	(51,60]	0.137***	Q3	0.131***	Tertiary	0.142***
	(0.027)		(0.026)		(0.025)		(0.023)
>5	0.160***	(60,72]	0.111***	Q4	0.125***		
	(0.034)		(0.023)		(0.027)		
		(72,107]	0.091***	Q5	0.139***		
			(0.018)		(0.032)		
p-value	0.000	p-value	0.007	p-value	0.513	p-value	0.000
n	4,238,741	n	4,238,741	n	3,554,738	n	2,780,416
R^2	0.47	R^2	0.47	R^2	0.46	R^2	0.47

Notes: The table shows the border closure's average treatment effect on household expenditures within a 30-minute car ride from a cross-border location compared to households living further away than 80 minutes, separately for different household characteristics. These characteristics include the *household size*, *age* of the registered cardholder, household *income* adjusted by the square root of household size, and the highest *education* in the household. The regression estimates model (2), standard errors are clustered at the zip code level, and the reported p-values test the equality of all coefficients.

reports estimation results separately for each group of the socioeconomic variables education, income, age, and household size in the panels a) to d). The reported p-values test for the equality of all coefficients.

First, I find that the effect increases in household size. While a one-person household increases her expenditures by 8% in response to the border closure, I see an increase of 15% for households with at least three members. Hence, larger households seem to engage in more cross-border shopping. Traveling abroad to shop at lower prices is particularly tempting if you buy large quantities, as it increases the trip's savings while the trip's traveling costs are fixed. Hence, relative costs decrease. Such economies of scale likely explain this finding, as the summary statistics in Table 1 show that larger households spend more money on groceries overall and consume larger quantities, making cross-border shopping more attractive for them.

Second, I find a hump-shaped life cycle in the response to the border closure. The estimated effect lies around 14% for the first three age quintiles spanning working age before decreasing towards retirement at age 65 and afterwards to roughly 10%. Note that this life cycle can either be due to age or cohort effects, as the short sample period does not allow for disentangling them.

Third, one might expect households with a lower income to engage in more cross-border shopping as they have higher import elasticities (see Auer, Burstein, Lein and Vogel (2023)) and spend a higher share of their income on groceries. In my data, high-income households (with a monthly income above 12,000 Swiss francs) spend 1.6% of their income on groceries compared to 3.5% for lower-income households (with a monthly income between 4,000 and 8,000 Swiss francs). Yet, I find no statistically significant differences between income categories. Contrarily, I even observe the strongest point estimate for high-earning households and find no evidence that income influences the decision to engage in cross-border shopping. One potential explanation is the fact that car ownership varies with income, according to the Federal Statistical Office. While 90% of high-income households (with a monthly income above 12,000 Swiss francs) own a car, this holds for only 77% of lower-income households (with a monthly income between 4,000 and 8,000 Swiss francs). Furthermore, lower-income households are also less mobile and travel, on average, shorter distances on a given day (30.2 kilometers vs. 40.8 kilometers). Hence, the limited mobility likely restricts lower-income households from increasing their cross-border shopping activity.

Fourth, I observe that households with at least one member holding a tertiary education react stronger to the border closure than comparable households further inland.[12] While high-educated households increase their expenditures by 14.3%, I estimate a lower effect of 9.9% for low-educated households. This complements the results on income that households with a lower socioeconomic status do not more often shop abroad.

Overall, these socioeconomic heterogeneities suggest that households do not primarily engage in cross-border shopping out of an economic necessity or because of large savings relative to their income, but they rather cross the border if they have high overall grocery expenditures and can,

[12] Primary (or compulsory) education ends at the latest after around eleven mandatory years of school (including kindergarten). Individuals who completed high school or an upper-secondary specialized school have a secondary education. The completion of any degree at a university, university of applied Sciences, or university of teacher education results in a tertiary degree.

Table 3: Treatment effects by cultural and spatial subgroups

Dep. Variable: log(Household Expenditures)			
a) Nationality		b) Country	
Group	Coeff	Group	Coeff
Swiss	0.106***	AT	0.007
	(0.023)		(0.016)
Asian	0.244***	GER	0.104***
	(0.042)		(0.013)
European	0.170***	FR	0.101***
	(0.026)		(0.029)
N.American	0.216***	IT	0.214***
	(0.049)		(0.054)
S.American	0.167***		
	(0.040)		
African	0.238***		
	(0.041)		
p-value	0.021	p-value	0.000
n	4,238,338	n	4,423,333
R^2	0.47	R^2	0.50

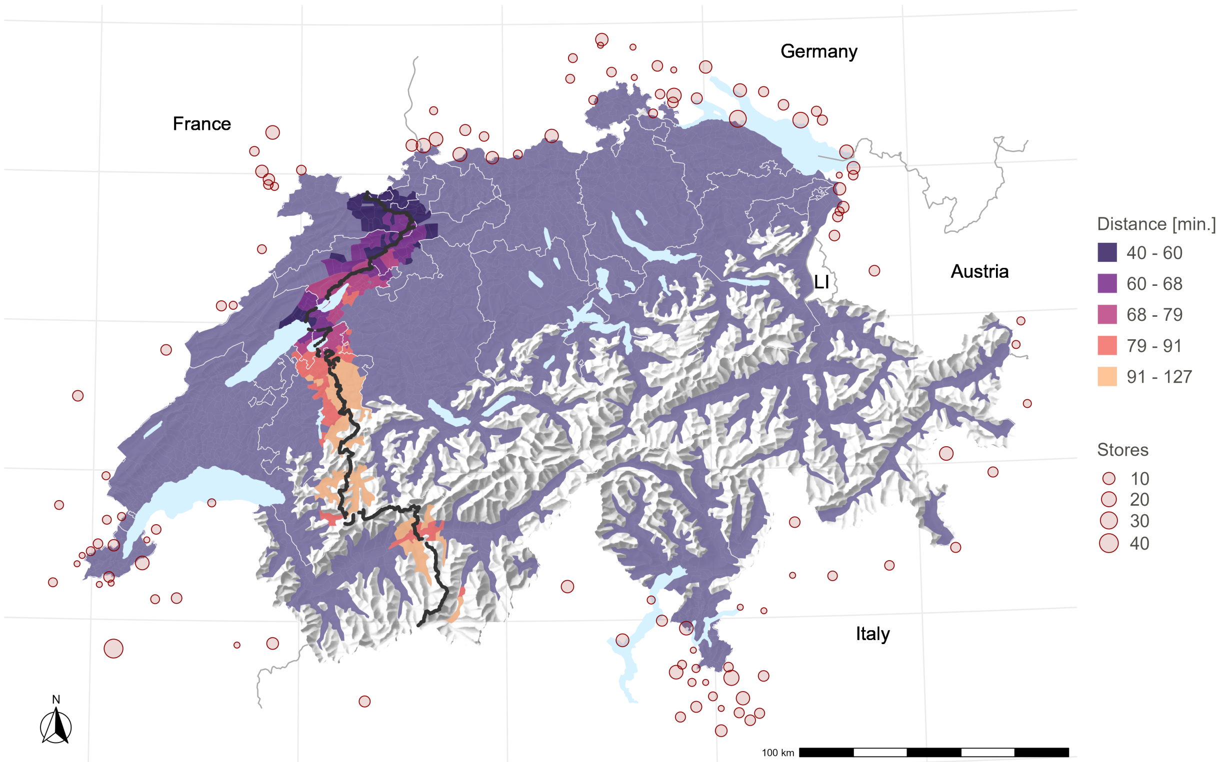
Notes: The table shows the border closure's average treatment effect on household expenditures within a 30-minute car ride from a cross-border location compared to households living further away than 80 minutes, separately for different household characteristics. These characteristics include the cardholders' *nationality* and the *country* of their closest cross-border shopping location. The regression estimates model (2), standard errors are clustered at the zip code level, and the reported p-values test the equality of all coefficients.

therefore, save more money in absolute terms. Yet, one might worry about these conclusions because of the correlation between these socioeconomic variables. For example, the observed age heterogeneity may be solely driven by the correlation between age and household size. To this end, I report in Table A.2 a single regression including all variables. Although the interpretation of these coefficients is more cumbersome, the relationship between a variable's individual categories remains unchanged for all four variables, supporting my conclusions.

4.3. Culture

Beyond the socioeconomic background of households, I address the role of cultural differences as citizens from various cultural origins may prefer products offered abroad over Swiss products. To this end, I analyze (i) a heterogeneity between customers of different nationalities and (ii) households living in close proximity but on opposite sides of the French-German language border

Figure 3: German-French language border



Notes: The figure shows the quintiles of car driving times to the closest cross-border shopping location in a 15-kilometer-band around the French-German language border on the municipality level. The dots show all 117 cross-border locations within 20 kilometers of the Swiss border. The dots' size indicates the number of supermarkets at this location, and the black line is the language border.

within Switzerland.

To begin with, Panel a) in Table 3 shows the heterogeneous response of individuals from different aggregated nationalities, estimating again the regression model (2). I observe that Swiss households are relatively less likely to shop abroad compared to foreign citizens. A Swiss citizen in the border region spent 10.6% more in response to the border closure, while Europeans increased their expenditures by 17%, Asians by 24%, Africans by 23.8%, and North- and South-Americans by 21.6% and 16.7%, respectively. To assess whether these observations are due to culture or socioeconomic differences between the nationalities, I add in Table A.3 the socioeconomic variables studied in Table 2. Most coefficients for foreign countries shrink sizably, suggesting, at best, a small difference between a Swiss and a foreign citizen conditional on the same socioeconomic characteristics. Hence, the role of international cultural differences appears minor once I take socioeconomic characteristics into account.

Table 4: Cultural differences: effect at language border

	Dep. Var: log(HH Expenditures)		
Dist. to ntl. border	German	French	p-value
Treat × 30-40 min.	0.106***	0.164***	0.049
	(0.012)	(0.027)	
Treat × 40-50 min.	0.058***	0.077*	0.022
	(0.011)	(0.029)	
Treat × 50-60 min.	0.031	0.030·	0.957
	(0.032)	(0.016)	

Notes: The figure shows the border closure's average treatment effect on household expenditures for households living within 7.5 kilometers of the German-French language border. I compare these treated units to same-language households living further away than 80 minutes from the closest cross-border location. The regression estimates model (3) using x million observations, and standard errors are clustered at the zip code level.

Furthermore, I use the intra-national Swiss language border between the French-speaking part of Switzerland in the West and the German-speaking part on the other side of this border to measure any cultural differences based on language. Figure 3 displays the language border crossing the entire country from North to South.[13] I use model (3) to estimate the treatment effect separately for French- and German-speaking households living within a 15-kilometer band around the language border compared to households further inland speaking the same language. I estimate treatment effects separately for households living between 30-40, 40-50, and 50-60 minutes from the national border compared to households farther away than 80 minutes.[14] This empirical strategy relies on the testable assumption that households within this 15-kilometer band are comparable. Table 4 displays the estimation results for different distance bins to the border and the reported p-value tests for equality of the coefficient in the two language regions. The response is stronger for households living in the French-speaking for every distance bin, although I do not find a statistically significant difference between languages. Hence, there is no significant evidence that comparable German- and French-speaking households should shop differently. Note that customers likely prefer to shop abroad at locations speaking their own language. Hence, price differences between Germany and France may partially drive these cultural differences. Yet, as grocery prices in Germany are

[13] I exclude in this analysis the German-Italian border in the South because very few people on both sides have comparable access to cross-border locations as this language border lies in the mountains.

[14] I do not report results for households living closer than 30 minutes from the next cross-border location as no household in the 15km band around the language border can reach a cross-border location in less than 30 minutes.

Table 5: Treatment effect for different commuting behaviors

	Dep. Var: log(HH Expenditures)		
Commuting Dist.	towards border	away f. border	p-value
Treat × 0-15 min.	0.133***	0.189**	0.059
	(0.039)	(0.042)	
Treat × 15-25 min.	0.181***	0.154***	0.322
	(0.034)	(0.034)	
Treat × 25-35 min.	0.188***	0.149***	0.360
	(0.036)	(0.041)	

Notes: The table shows the border closure's average treatment effect on household expenditures within a 30-minute car ride from a cross-border location compared to households living further away than 80 minutes for different household commuting trips. These trips include commutes by car for 0-15 minutes, 15-25 minutes, and 25-35 minutes by car, either towards the national border (bringing the commuter closer to a cross-border location) or further away from the border in comparison to the household's home. The regression estimates model (3), using x observations. Standard errors are clustered at the zip code level.

generally lower than in France (see Table 6), I expect my estimates to be a lower bound for the true cultural differences.

4.4. Commuting and trip chaining

A key determinant of a household's shopping behavior may be her daily commute to work. First, households can combine commuting and shopping through trip chaining if their workplace is closer to the border than their home. Second, frequent commuting trips to work may alter a household's perception of traveling costs and influence her likelihood of traveling abroad. Hence, I use model (3) to estimate the treatment effect separately for households commuting either from home (i) towards foreign shopping locations or (ii) farther inland, away from cross-border locations. I focus on households that report commuting by car.

Table 5 shows the estimation results. On the one hand, a household commuting towards the border increases her cross-border shopping as she commutes longer, meaning, closer to the border. The effect increases from 13.3% from a short commute to 18.8% for commutes of 25-35 minutes. On the other hand, the treatment effect decreases with driving time for households commuting away from the border (from 18.9% to 14.9%). This provides conclusive evidence that households combine work commutes with cross-border shopping trips in the form of trip chaining.

Table 6: Prices in neighboring countries 2015–2020

	Austria		France		Germany		Italy	
Category	PI	vs. CH	PI	vs. CH	PI	vs. CH	PI	vs. CH
Clothing and footwear	102.83	-20%	105.53	-18%	98.80	-23%	100.52	-22%
Consumer goods	106.37	-20%	107.02	-20%	103.12	-23%	105.18	-21%
Food and non-alcoholic beverages	120.47	-28%	112.38	-33%	102.52	-39%	109.30	-35%
Households appliances	95.08	-21%	105.37	-12%	101.18	-16%	101.50	-15%
Recreation and culture	113.27	-26%	107.28	-30%	104.57	-32%	100.10	-35%
Restaurants and hotels	108.67	-35%	119.73	-28%	105.88	-36%	104.02	-38%

Notes: The table shows prices in neighboring EU countries averaged over the six years before and during the first wave of the COVID-19 pandemic, 2015–2020. Prices are shown as price indices (PI) for different product categories and relative to the category's price index in Switzerland. In each year, the EU27 average is set to 100.

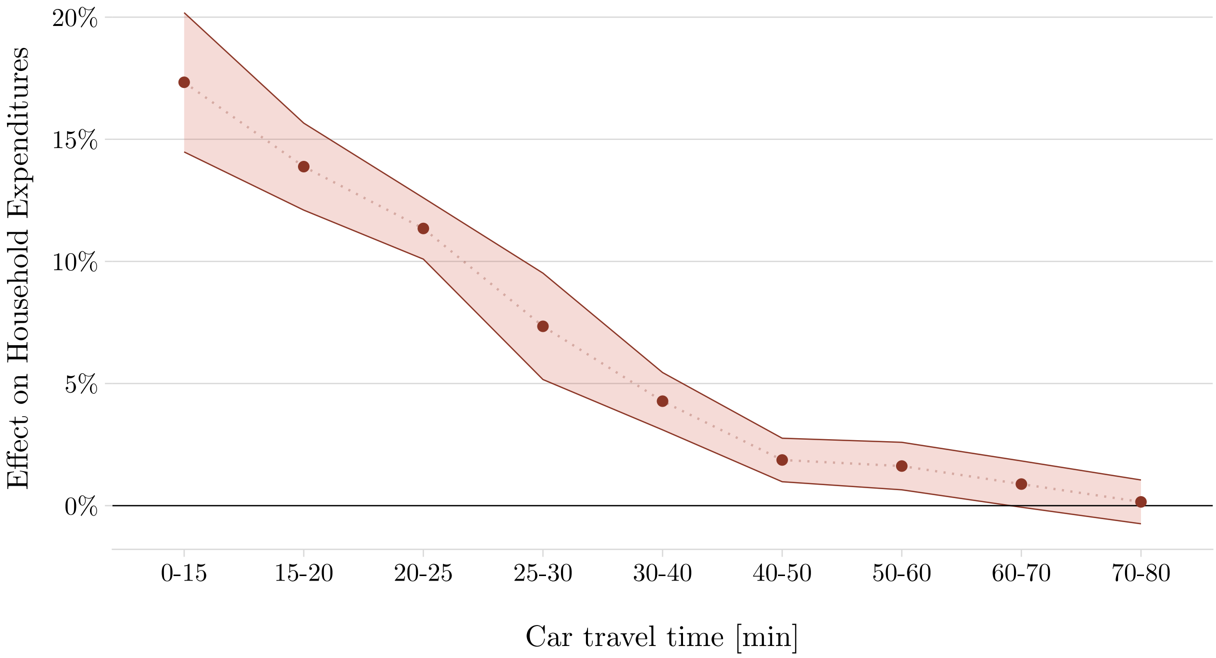
4.5. Variation across cross-border locations

Finally, I look at the role of neighboring countries and their grocery prices. Panel b) of Table 3 shows the spatial variation of the effect by estimating heterogeneous treatment effects for the four neighboring countries Austria, Germany, France, and Italy.[15] The results show a large estimate for households living closest to Italy (28%), with smaller values for France and Germany (11% and 15%, respectively) and no significant response to the shock for households living close to Austria. To assess the role of prices behind these findings, I show in Table 6 national price level indices averaged over the period of 2015 – 2020 for different major product categories and how much these products are cheaper compared to Switzerland. While each product category is in every country cheaper than in Switzerland, relative prices between these neighboring countries vary for different product categories.

Using the price level index for consumer goods, the heterogenous coefficients are negatively correlated with the price index of the neighboring countries, meaning that higher foreign prices correspond to less Swiss cross-border shopping. Based on a back-of-the-envelope calculation using the price indices for food and non-alcoholic beverages, a 1% increase in the price index of a neighboring country is associated with a 0.78% decline in cross-border shopping expenditures. Note that any interpretation of this as a price elasticity assumes that all households assigned to a given neighboring country face the same price difference at home and abroad, which seems plausible as our retailer

[15] For this spatial heterogeneity, I use week fixed effects compared to the week-group fixed effects in the case of socioeconomic variables.

Figure 4: Decay of the treatment effect



Notes: The figure shows the border closure's average treatment effect on household expenditures for households living within a certain distance bin. I compare these treated units to households living further away than 80 minutes from the closest cross-border location. Standard errors are clustered at the zip code level. The regression estimates model (3) and uses 23 million observations.

charges the same prices throughout the country. Additionally, this calculation assumes that residential location choice does not depend on the households' cross-border shopping preferences and that customers buy the same products at home and abroad. Also, not all foreign retailers charge the same prices across the entire country, and prices may be higher close to the Swiss border.

4.6. The distance decay function

Focusing on the role of distance behind these findings, I quantify the decay of cross-border shopping with distance by analyzing the effect for different distance bins from model (3). Figure 4 displays the distance decay function, plotting the average treatment effect for each distance bin separately.

Households living within a short distance of 15 minutes from a cross-border destination increase their expenditures by 17% during the border closure. The effect first declines linearly up to a distance of 40 minutes before flattening out and becoming negligible, although remaining significant for up to 70 minutes. This suggests an extensive margin effect due to the high fixed costs of the trip. Hence, most individuals having to drive more than 40 minutes avoid the trip altogether, leading to a strong kink in the decay function. Before hitting this threshold, the variable costs appear highly linear in travel time. Note that these distances are potentially lower bounds of the actual travel distance as customers might prefer to shop at other foreign stores further away. These findings are broadly in line with Burstein et al. (2022), who estimate that Swiss households close to the border spend roughly 8% of their expenditures abroad.

In addition, Figure A.1 to Figure A.4 display the distance decay for each socioeconomic characteristic separately. The general picture is consistent with the estimates in Table 2, suggesting that the variable costs of traveling longer do not depend on any of these variables. Larger households respond more to the shock across all distance bins, while rather old and young households engage in less cross-border shopping.

4.7. Robustness

Can also do robustness with road distance! and with other minimum values for expenditures.

This section adds robustness checks. In the main results of the dynamic treatment effect, I compare treated households living within 30 minutes from the closest cross-border location to control households living further than 80 minutes away. Figure A.5 displays the distribution of car travel times to the closest cross-border location for all households in the final data. Built on that, Figure A.7 reproduces the same results but uses a control group that lives at least 90 or 100 minutes from the closest cross-border location (resulting in a control group of 6% and 2.5% of the sample, respectively). In both cases, the average treatment effect remains between 10% and 15% percent, even as the comparison groups become small for these more restrictive doughnut bins. I also use another definition of cross-border locations where I only consider very large foreign stores that may be more attractive to travel to (Figure A.8). The changes in the coefficients are minimal. Finally, I report in Figure A.6 the dynamic estimates for the full sample of transaction data rather than focusing on the sub-sample of customers matched to residents in the administrative data. Also, in this case, the observed changes are negligible.

5. Conclusion

Overall, price differences between neighboring countries induce households to shop abroad and generate welfare gains for them. I analyze the COVID-19-induced border closure in Switzerland as a natural experiment and show that cross-border shopping is a widespread and persistent phenomenon in Switzerland, and diverse socioeconomic groups are willing to drive up to 40 minutes to take part in it. My findings indicate further that particularly larger households engage in more cross-border shopping, that the response is larger if the neighboring country has relatively low grocery prices, and that households commuting towards the border combine their trip to work with shopping abroad.

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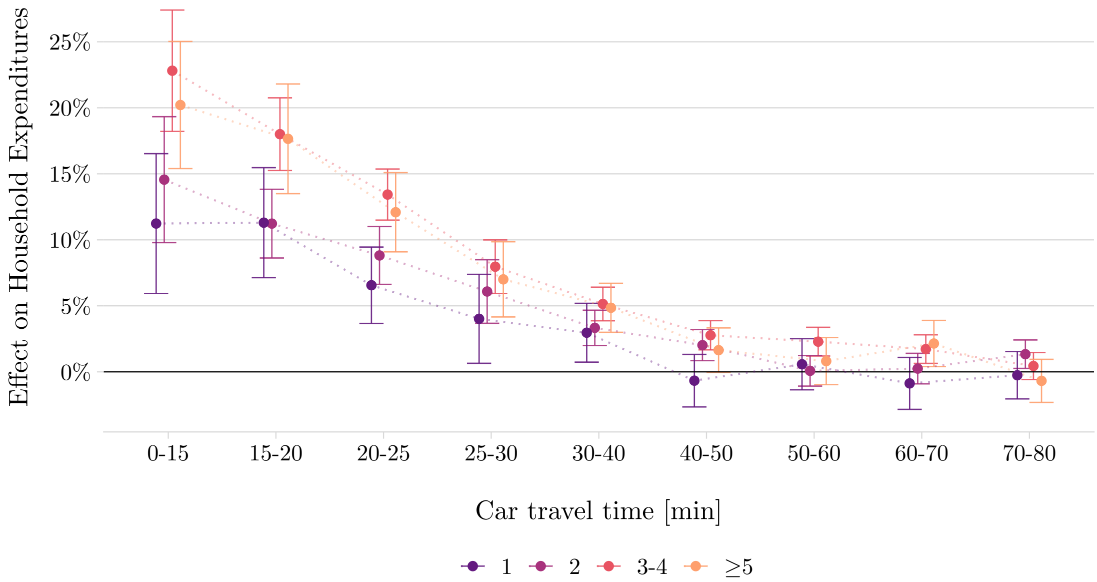
Appendix A. Supplementary Material

Table A.1: Cross-border locations

	Location	Country	Pop	Number of Stores			Rank		
				Google Reviews			*Google Reviews*		
				-	100	500	-	100	500
1	Annecy	FR	131'766	79	29	11	1	1	3
2	Como	IT	84'808	76	21	14	2	4	1
3	Konstanz	GER	84'446	71	29	14	3	1	1
4	Singen	GER	48'033	50	18	10	4	5	4
5	Annemasse	FR	36'582	49	13	5	5	13	15
6	Aosta	IT	34'052	47	7	3	6	30	34
7	Livigno	IT	6'363	47	14	5	6	12	15
8	Varese	IT	80'588	46	15	7	8	8	8
9	Friedrichshafen	GER	61'561	45	23	10	9	3	4
10	Sondrio	IT	21'457	40	3	1	10	67	67
11	Cantù	IT	40'031	39	12	6	11	16	10
12	Belfort	FR	45'458	37	15	4	12	8	22
13	Lindau	GER	25'547	36	15	9	13	8	6
14	Domodossola	IT	17'930	35	11	4	14	18	22
15	Lörrach	GER	49'295	33	15	7	15	8	8
16	Weil am Rhein	GER	30'009	31	18	9	16	5	6
17	Saronno	IT	39'332	30	9	6	17	24	10
18	Waldshut-Tiengen	GER	24'067	30	13	6	17	13	10
19	Stockach	GER	17'118	29	11	5	19	18	15
20	Radolfzell	GER	31'582	28	7	4	20	30	22
21	Überlingen	GER	22'684	27	13	4	21	13	22
22	Rheinfelden	GER	32'919	26	16	5	22	7	15
23	Bad Säckingen	GER	17'510	25	11	4	23	18	22
24	Bregenz	AT	29'806	25	12	5	23	16	15
25	Montbéliard	FR	25'806	25	10	3	23	22	34
			...						
Overall									
117			1'980'614	1'787	691	304			

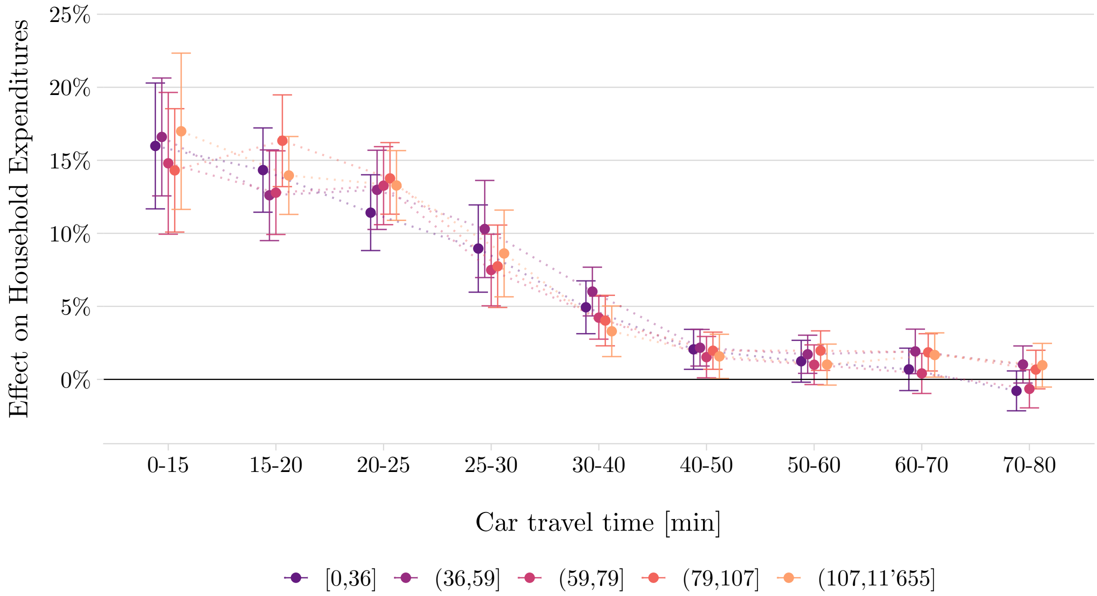
Notes: The table shows the 25 largest cross-border locations for grocery shopping. *Number of Stores* counts the municipality's stores for a given minimum of Google reviews, while *Rank* ranks the locations according to the number of stores. All store locations are scraped from Google Maps.

Figure A.1: Decay of the treatment effect: by household size



Notes: The figure shows the border closure's effect on household expenditures for different distance bins and household size quintiles compared to households living further away than 80 minutes. Household size is measured by the number of people living in this household according to administrative data. Standard errors are clustered at the zip code level. The regression estimates model (3) and uses 4.9 million matched observations.

Figure A.2: Decay of the treatment effect: by income



Notes: The figure shows the border closure's effect on household expenditures for different distance bins and income quintiles compared to households living further away than 80 minutes. Income is measured in 1,000 CHF. Standard errors are clustered at the zip code level. The regression estimates model (3) and uses 4.9 million observations.

Table A.2: Treatment effect by socioeconomic subgroups (jointly)

Dependent Variable:	log(HH Expenditures)
Treat	0.054***
	(0.011)
Education (reference = 1)	
Treat × Education = 2	0.037***
	(0.006)
Treat × Education = 3	0.084***
	(0.006)
Income Quintile (reference = Q1)	
Treat × Income Quintile = Q2	0.006
	(0.005)
Treat × Income Quintile = Q3	0.010**
	(0.005)
Treat × Income Quintile = Q4	0.025***
	(0.006)
Treat × Income Quintile = Q5	0.048***
	(0.006)
Age Quintile (reference = 18–48)	
Treat × Alter Quintile = (38,48]	-0.002
	(0.005)
Treat × Alter Quintile = (48,55]	-0.034***
	(0.005)
Treat × Alter Quintile = (55,63]	-0.064***
	(0.005)
Treat × Alter Quintile = (63,103]	-0.097***
	(0.006)
No. of HH members (reference = 1)	
Treat × No. of HH members = 2	0.016***
	(0.005)
Treat × No. of HH members = 3-4	0.041***
	(0.006)
Treat × No. of HH members = 5+	0.019**
	(0.007)
Observations	3,817,128

Notes: The table shows the border closure's average treatment effect on household expenditures within a 30-minute car ride from a cross-border location compared to households living further away than 80 minutes, interacting the treatment effect with all the socioeconomic household characteristics. These characteristics include the *household size*, *age* of the registered cardholder, household *income* adjusted by the square root of household size, and the highest *education* in the household. The regression estimates model (2), where x_i now is a matrix. Standard errors are clustered at the zip code level. The coefficient for the number of COVID-19 cases is omitted.

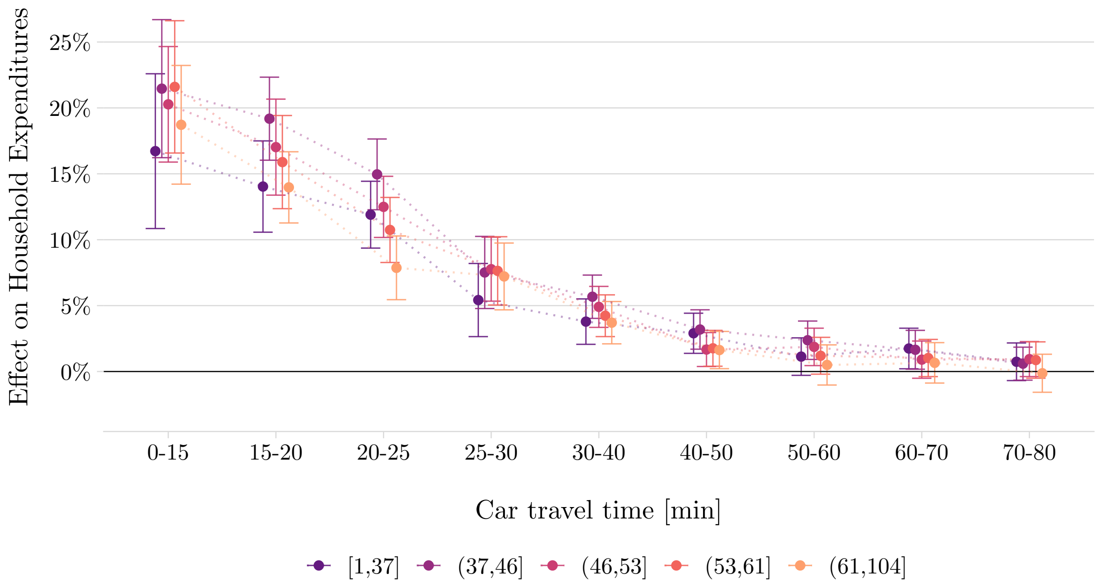
Table A.3: Treatment effect by socioeconomic subgroups (jointly with nationality)

Dependent Variable:	log(HH Expenditures)
Model:	(1)
Variables	
Treat	0.050***
	(0.010)
Treat × Education = 2	0.040***
	(0.006)
Treat × Education = 3	0.086***
	(0.006)
Treat × Income Quintile = Q2	0.006
	(0.005)
Treat × Income Quintile = Q3	0.010**
	(0.005)
Treat × Income Quintile = Q4	0.025***
	(0.006)
Treat × Income Quintile = Q5	0.048***
	(0.006)
Treat × Nationality = African	0.027
	(0.025)
Treat × Nationality = Asian	-0.039***
	(0.015)
Treat × Nationality = European	0.012**
	(0.006)
Treat × Nationality = N.American	0.073*
	(0.037)
Treat × Nationality = S.American	-0.016
	(0.019)
Treat × Alter Quintile = (38,48]	-0.002
	(0.005)
Treat × Alter Quintile = (48,55]	-0.034***
	(0.005)
Treat × Alter Quintile = (55,63]	-0.063***
	(0.005)
Treat × Alter Quintile = (63,103]	-0.096***
	(0.006)
Treat × No. of HH members = 2	0.015***
	(0.005)
Treat × No. of HH members = 3-4	0.041***
	(0.006)
Treat × No. of HH members = 5+	0.018**
	(0.007)
Fit statistics	
Observations	3,816,914

Clustered (PLZ) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

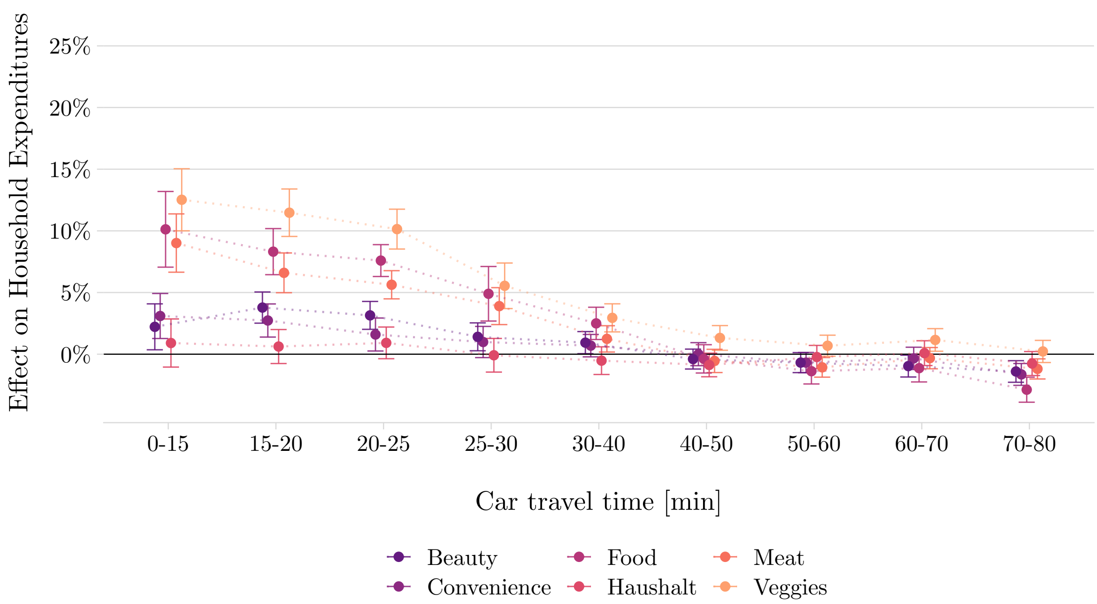
Notes: The table shows the border closure's average treatment effect on household expenditures within a 30-minute car ride from a cross-border location compared to households living further away than 80 minutes, interacting the treatment effect with all the socioeconomic household characteristics. These characteristics include the *household size*, *age* of the registered cardholder, household *income* adjusted by the square root of household size, the highest *education* in the household, and *nationality*. The regression estimates model (2), where x_i now is a matrix. Standard errors are clustered at the zip code level. The coefficient for the number of COVID-19 cases is omitted.

Figure A.3: Decay of the treatment effect: by age



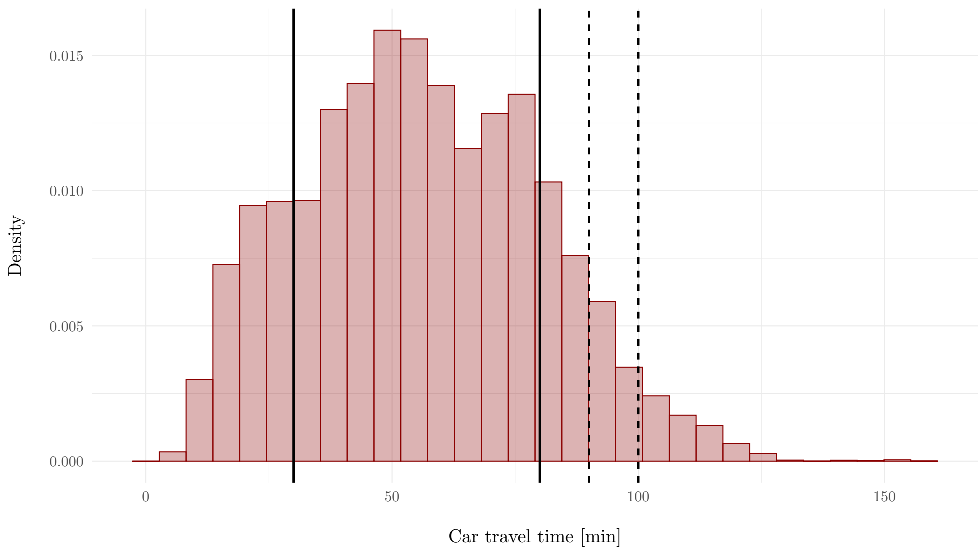
Notes: The figure shows the border closure's effect on household expenditures for different distance bins and age quintiles compared to households living further away than 80 minutes. Standard errors are clustered at the zip code level. The regression estimates model (3) and uses 4.9 million observations.

Figure A.4: Decay of the treatment effect: by product groups



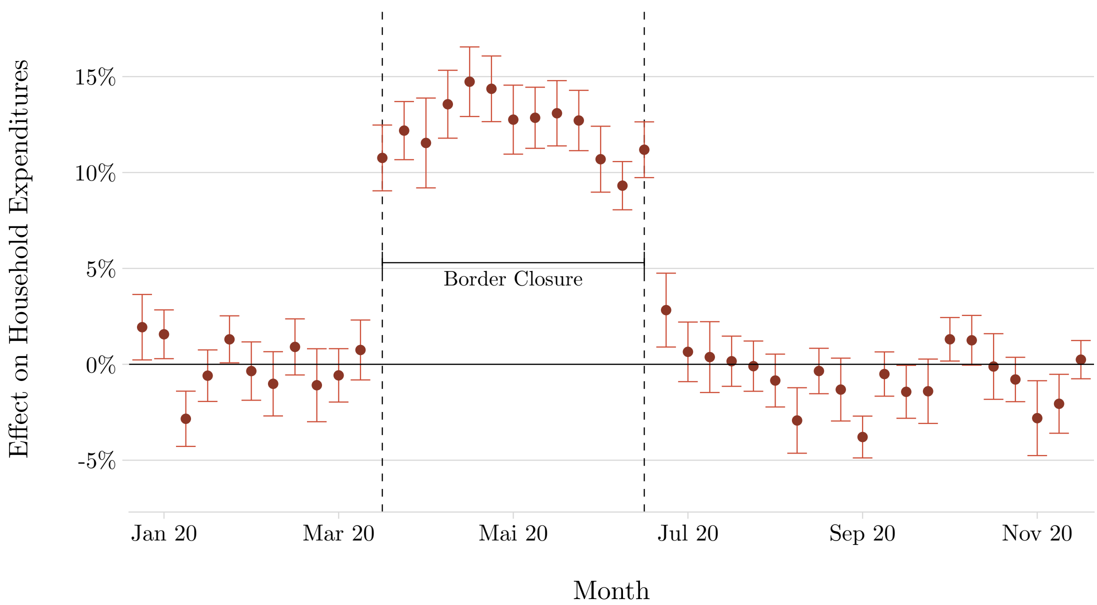
Notes: The figure shows the border closure's effect on household expenditures for different distance bins and product groups compared to households living further away than 80 minutes. Standard errors are clustered at the zip code level. The regression estimates model (3) and uses 67.6 million observations, where the transactions are aggregated to product categories.

Figure A.5: Distribution of travel times



Notes: The figure shows the distribution of car travel times from a household's home to the closest cross-border shopping location. The subsamples of control units used in the different robustness checks of the dynamic results are marked by vertical dashed lines.

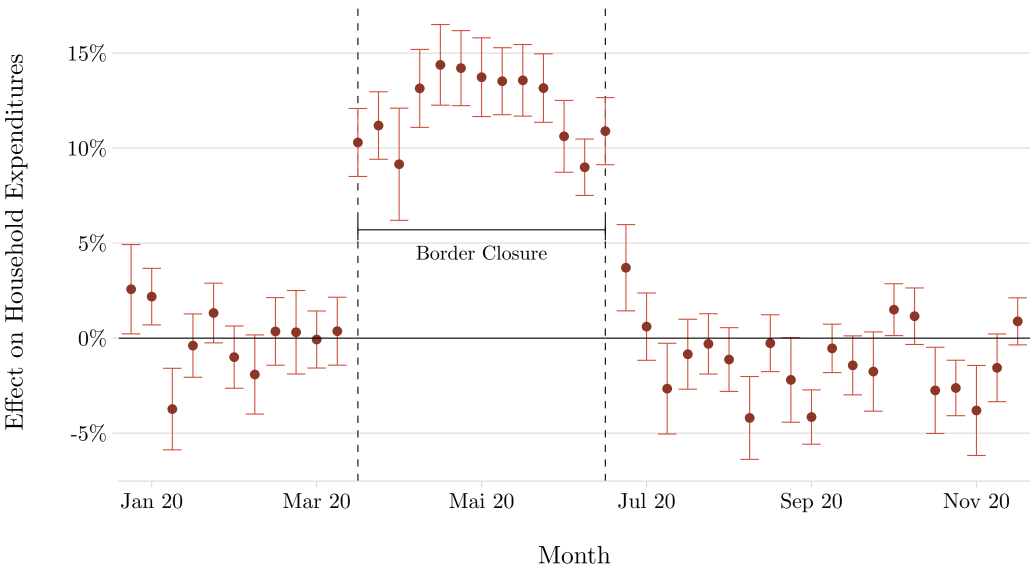
Figure A.6: Robustness of the dynamic treatment effects: the full grocery transaction data



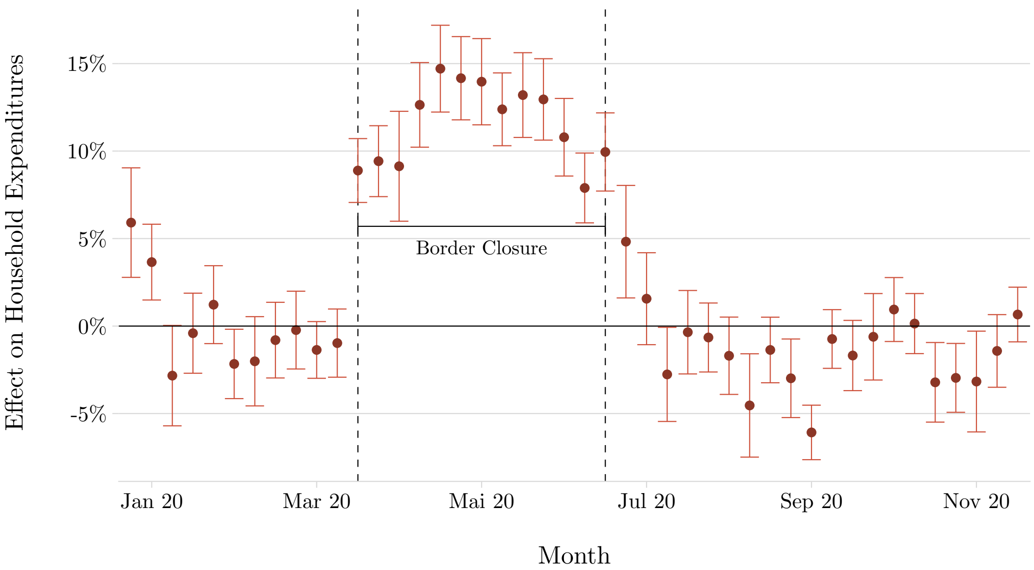
Notes: The figure shows the border closure's effect on household expenditures within a 30-minute car ride from a cross-border location compared to households living further away than 80 minutes. Standard errors are clustered at the zip code level. The regression estimates model (1) and uses 16.6 million observations.

Figure A.7: Robustness of the dynamic treatment effects: different control distance

(a) Control group: more than 90 minutes distance



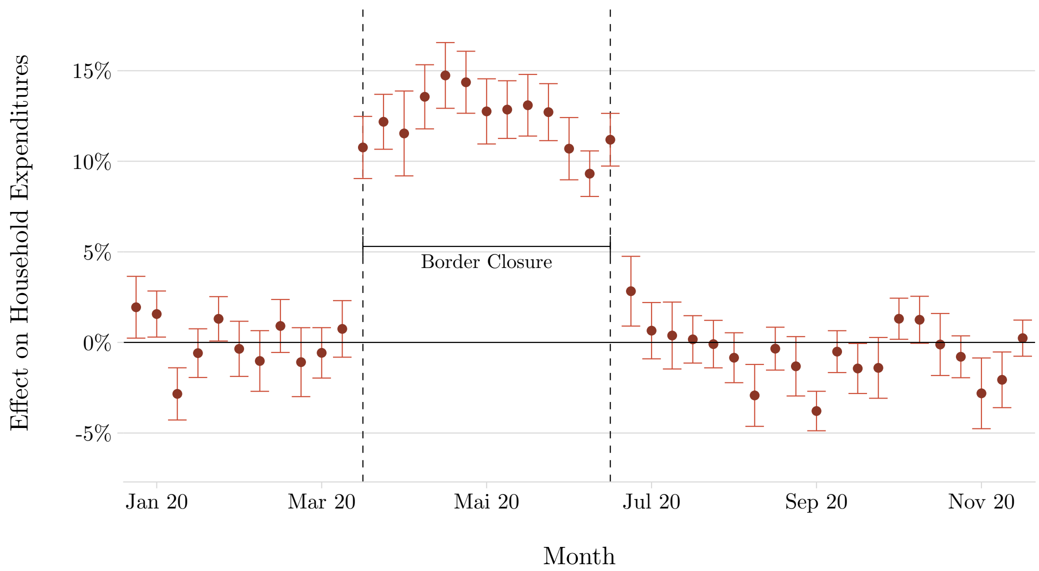
(b) Control group: more than 100 minutes distance



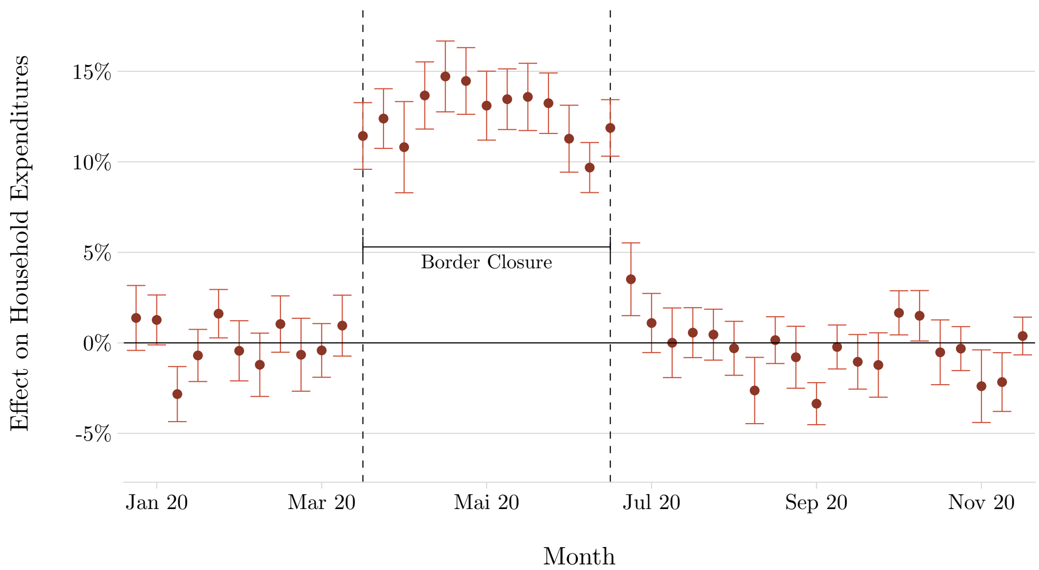
Notes: Figure A.7a shows the border closure's effect on household expenditures within a 30-minute car ride from a cross-border location compared to households living further away than 90 minutes. The regression estimates model (1) and uses 13.3 million observations. Figure A.7b also estimates model (1) for a distance of 100 minutes using 11.2 million observations. Standard errors are clustered at the zip code level.

Figure A.8: Robustness of the dynamic treatment effects: different definitions of cross-border locations

(a) At least three stores with more than 100 Google reviews



(b) At least three stores with more than 500 Google reviews



Notes: Figure A.8a shows the border closure's effect on household expenditures within a 30-minute car ride from a cross-border location compared to households living further away than 80 minutes. I consider all cross-border locations with at least three stores with more than 100 Google reviews. In comparison, Figure A.8b shows the same results but considers locations with at least three stores with more than 500 Google reviews. Both regressions estimate model (1) and use 16.6 million observations. Standard errors are clustered at the zip code level.