Cross-Border Shopping: Evidence from Household Transaction Records*

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Abstract

Cross-border shopping allows consumers from high-price countries to obtain comparable goods at lower prices in foreign markets. 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. This paper exploits the random timing of the Swiss border closure to study heterogeneities in the willingness to travel for lower prices. To this end, I merge unique consumer-linked transaction data on 750,000 customers with administrative records. I find that domestic grocery expenditures temporarily increase by 10.9% in border regions. My results show that the effect increases in household size, and decreases in age, income, education, and the cross-border locations' price index. Furthermore, I find novel evidence that citizens working close to the border combine their commuting trips with cross-border shopping, providing evidence for strategic trip chaining.

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

JEL: R1, R2, L14

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1. Introduction

Cross-border shopping has been a growing phenomenon along national borders, where consumers from one nation can purchase goods and services at lower prices from neighboring countries. This outflow of customers puts pressure on domestic prices and increases product variety for households living close to the border, but it can also 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). Yet, while urban researchers understand the commuting behavior of workers well today, consumers' movement for shopping remains understudied, partially because suitable natural experiments are scarce.

However, numerous countries imposed rigorous travel restrictions at national borders in 2020 to contain the spread of COVID-19, providing such a natural experiment. This paper exploits the closure of the Swiss borders during the COVID-19 pandemic in order to examine patterns and heterogeneities in consumer mobility. On March 16, 2020, the Swiss government mandated the immediate closure of all national borders to neighboring countries to mitigate the spread of COVID-19. This policy was upheld until June 2020.[1] Additionally, the government 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 for two reasons. First, Switzerland is surrounded by countries with 28-39% lower grocery prices, 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.[3] Hence, the relative attractiveness of these countries for Swiss consumers depends solely on the variety and prices of their grocery products. Second, the exact timing of the border closure was random for Swiss residents, and Burstein, Lein and Vogel (2022) show that the

[1] The borders to Liechtenstein remained open while crossing between Liechtenstein 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] Imports into Switzerland are exempt from VAT for a total value below 300 Swiss francs, as long as certain limits for meat, tobacco, etc., are met. In addition, Switzerland also borders the Principality of Liechtenstein (40,000 inhabitants), which uses the Swiss franc as a currency and has almost identical grocery prices.

[3] The CHF/EUR exchange rate was stable throughout this period.

policy was stringent and effective in achieving its purpose, as cross-border shopping shares almost fell to zero during the intervention.

I identify the causal effect of the border closure on expenditures at grocery stores in Switzerland by comparing Swiss households living close to a national border to Swiss households residing further inland within a difference-in-differences framework. The estimated increase in domestic grocery expenditures measures the magnitude of cross-border shopping during open borders as customers were forced by the shock to shift these expenditures to domestic retailers. To conduct this analysis, I merge unique grocery data featuring the universe of customer-linked transactions from the largest Swiss retailer for the year 2020 with individual-level administrative records on labor market income, commuting behavior, and household characteristics for the entire Swiss population. The final data set contains 40 million weekly shopping baskets for 750,000 households that I can uniquely link to residents in the administrative data. I use this setting to calculate a distance decay function, measuring the decline in cross-border shopping with distance, and to analyze extensive heterogeneities across households' socioeconomic characteristics, cultural backgrounds, and commuting behavior.

My findings show that mobility patterns in consumption are persistent over time and vary strongly between different groups of customers. First, I find that the policy increases expenditures by 10.9% in border regions. This effect vanishes instantly and entirely once the border reopens, suggesting that behaviors in cross-border shopping are deeply rooted and resist temporary shocks. These estimated effects decay with distance, indicating that a household's probability of engaging in cross-border shopping decreases with travel time. Second, I document various heterogeneities and find larger effects among poorer, younger, and larger households in response to the policy. Third, I provide novel evidence that households combine their trips to work with cross-border shopping if they commute towards the border. Fourth, I find that cross-border shopping is more pronounced in areas with cheaper neighboring countries, suggesting a price elasticity of 0.61.

This paper relates to two strands of the literature. First, it contributes to the previous work on cross-border shopping, documenting that both consumers and retailers respond to changes in relative prices. For instance, a depreciation of the US dollar reduces the consumers' propensity to cross into Canada (Chandra, Head and Tappata, 2014) while increasing US employment and the number of establishments close to the border (Campbell and Lapham, 2004). Similarly, Asplund, Friberg and Wilander (2007) show that a cut in Danish spirits taxes reduces alcohol sales in Sweden, and Baker, Johnson and Kueng (2021) find that customers in the United States use cross-border shopping to escape local sales taxes. Finally, Friberg, Steen and Ulsaker (2022) demonstrate that the marginal customer further inland reacts stronger to foreign price changes while households close to the border shop abroad anyway. This implies that the response to relative price changes is an incomplete measure of the level of cross-border shopping. Therefore, I follow an alternative approach and use a natural experiment that restricts access to cross-border shopping completely rather than changing relative prices.

At least two other papers tackle the topic of cross-border shopping through COVID-19-related border closures, answering, however, different questions. First, Friberg, Halseth, Steen and Ulsaker (2024) investigate the effect on taxes and find that Norwegian cross-border shopping reduces national tax revenues by 3.6% nationally and 27% in border regions. Second, Burstein, Lein and Vogel (2022) study cross-border shopping in Switzerland using data from Nielsen and conclude that it lowers the cost of living by over 14% in some regions. In contrast to these papers, I focus on the customers' behaviors and the rich heterogeneities therein. My data – matching unique transaction records with administrative data – may be better suited for this analysis than the Nielsen data, whose self-recorded reporting errors are correlated with demographic variables (Einav, Leibtag and Nevo, 2008).

In a broader context, this paper also links to the research on spatial shopping in general and trip chaining, showing that customers deliberately plan and adapt their grocery expenditures and shopping trips. For example, Agarwal, Jensen and Monte (2022) suggest that consumers purchase products with a low storability within a shorter distance. Additionally, previous work on spatial trip-chaining demonstrates that customers strategically visit multiple non-tradable services along their daily travels. This travel behavior generates consumption externalities that explain one-third of the spatial concentration in non-tradable services (Oh and Seo, 2023) and Miyauchi, Nakajima and Redding (2022) show that modeling trip-chaining is crucial to understanding the decreased demand for non-traded services following the shift to remote working during the COVID-19 pandemic. Furthermore, trip-chaining can cause complex adaptations in the spatial equilibrium with potentially winning and losing stores (Relihan, 2024). My paper contributes to this literature by showing that households strategically include their cross-border shopping trips into their daily commutes to work.

The remainder of 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 checkout with their loyalty card in exchange for exclusive offers and discounts. This loyalty program captures 79% of the retailer's total sales, and 2.4 million customers regularly participate in it (meaning, 33% 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.

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, household members, 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* add education and commuting behavior for the sub-sample

[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.

Table 1: Household summary statistics

	Final Sample		Population	
Panel a)	Mean	SD	Mean	SD
Age	56.63	15.91	50.43	18.17
Income (1,000 CHF)	100.66	129.99	106.01	132.48
Income adjusted (1,000 CHF)	60.09	80.29	64.90	78.96
Time home to work (min.)	28.21	23.02	29.12	23.70
Time home to border (min.)	57.69	24.27	56.13	25.28
Time work to border (min.)	58.28	31.75	56.08	23.81
Panel b)	Pct.	N	Pct.	N
Education		505,309		4,413,173
Primary	9.8	49,747	11.3	498,292
Secondary	45.8	231,237	44.3	1,954,810
Tertiary	44.4	224,325	44.4	1,960,071
Household size		757,629		7,043,734
1	19.3	146,593	20.9	1,471,897
2	36.0	272,663	36.1	2,544,442
3-4	36.1	273,742	33.8	2,381,660
5+	8.5	64,631	9.2	645,735
Language		756,936		7,036,484
German	76.2	576,786	71.2	5,010,326
French	20.2	153,279	24.1	1,697,654
Italian	3.5	26,871	4.7	328,504
Population density		756,936		7,036,484
Urban	24.4	184,556	30.2	2,122,190
Suburban	57.6	436,372	51.9	3,649,595
Rural	18.0	136,008	18.0	1,264,699
Nationality		757,568		7,042,341
Swiss	85.6	648,380	74.0	5,210,215
European	12.5	94,605	22.0	1,551,076
African	0.5	3,507	1.1	77,266
Asian	1.0	7,255	1.9	131,883
N.American	0.1	1,025	0.3	21,530
S.American	0.4	2,796	0.7	50,371
Commuting mode		103,295		923,718
Car	59.0	60,973	55.4	511,779
Public transport	24.8	25,595	27.8	256,869
Other	16.2	16,727	16.8	155,070
Observations		757,629		7,043,734

Notes: The table shows summary statistics for the customers uniquely matched to the administrative data and compares them to the entire Swiss population above legal age. *Income* equals the total annual labor market income of a household in 1,000 Swiss Francs, and *Income Adjusted* adjusts for the square root of household size. All *Time* variables measure the uncongested car travel time in minutes to the work location or the closest cross-border location. The variables *Commuting mode* and *Education* are only available for the sub-sample participating in the *Structural Surveys*.

of individuals participating in the survey.[5] Education is categorized as either primary, secondary, or tertiary education, and the commuting behavior is characterized by travel times in minutes, means of transport, and the municipality of the work location.[6]

Both data sets measure addresses on the same spatial grid spanning 350,000 cells over the entire country with a mean population of 25 residents. 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 1.3 million customers in the grocery data uniquely to a citizen and her household in the administrative data. Hence, I can match 54% of the 2.4 million regular customers, corresponding to 20% of all adult Swiss residents. The outcome of interest throughout this analysis is a household's total grocery expenditures in a given week. I aggregate the individual shopping trips into weekly baskets and exclude customers who moved in 2020 as well as those spending less than 100 Swiss francs per capita a month before the shock (equalling 111 USD on April 4, 2024), as their baskets might not capture the overall consumption accurately. This procedure generates a final data set including 757,000 households and 40 million weekly consumption baskets.[7]

Table 1 shows summary statistics for the households and displays for how many of them I observe a given variable. The average matched household has an income of 60,000 Swiss francs (adjusted for the square root of household size), and the mean cardholder is 56.6 years old, while 44.4% have a tertiary education, and 80% live in multi-person households. Comparing these statistics to the entire administrative data shows that the matched sample represents the population well. Further, Table 2 shows summary statistics for the transactions. The average household makes 6.1 transactions and spends 92 Swiss francs (102 USD on April 4, 2024) per week. This corresponds to roughly 63% of the average household's grocery expenditures based on administrative consumption surveys. Looking at different subgroups, expenditures increase with household size and income, while they are hump-shaped for age. A comparison to the entire transaction data shows that the

[5]This representative cross-sectional survey selects 200,000 people above age 15 every year. Individuals can be selected repeatedly, and participation is mandatory. To measure education, I use the highest-reported education between 2010 and 2021 and exclude individuals younger than 30 to capture students. For commuting, I only use the surveys since 2018 as workplaces are less stable than education.

[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. Completing any degree at a university, university of applied sciences, or university of teacher education results in a tertiary degree.

[7]See Kluser and Pons (2024) and Kluser, Seidel and von Ehrlich (2022) for additional information on the two data sources, the matching procedure, and the representativeness of the matched households for the general population.

Table 2: Transactions summary statistics

Group	Mean	SD	p50	p1	p99
Weekly grocery purchases					
Expenditures in matched sample	92.5	64.1	75.5	12.9	300.7
Expenditures in full sample	88.7	62.3	72.0	12.2	293.1
Shop visits in matched sample	6.1	3.5	5.5	0.8	17.5
Shop visits in full sample	6.1	3.5	5.5	0.8	17.4
Expenditures by age group					
20–34	82.2	53.9	68.7	11.8	251.1
35–44	107.9	70.6	91.6	13.8	317.6
45–54	110.2	74.5	92.1	14.2	336.7
55–64	94.6	63.7	79.1	13.6	301.7
65–74	79.4	51.3	67.1	12.7	247.4
75+	68.3	44.4	57.4	11.2	217.4
Expenditures by income quintile					
25,000–73,000	79.3	53.1	65.7	12.7	255.5
73,001–106,000	90.7	59.7	75.5	13.4	280.5
106,001–137,000	104.0	66.6	89.6	14.2	302.4
137,001–181,000	111.9	71.0	97.6	14.3	321.4
181,001+	119.3	79.4	102.5	13.6	357.8
Expenditures by education					
Primary	69.8	47.7	57.3	11.4	232.8
Secondary	90.5	60.2	75.6	13.3	284.2
Tertiary	107.9	71.9	91.3	13.7	328.8
Expenditures by household size					
1	60.0	37.3	51.8	11.2	191.0
2	83.2	51.5	72.5	12.6	244.0
3–4	111.5	71.0	97.1	14.5	319.9
5+	125.0	84.8	105.9	14.4	373.6
Transactions in matched sampled			40,179,519		
Transactions in full sampled			95,192,993		

Notes: The table shows summary statistics for the weekly expenditures and trip frequency of customers that I can match to residents in the administrative data. I compare these statistics to the full transaction data set, including the unmatched customers, and report statistics on sub-samples for the matched data. The statistics for the *full sample* apply the same sample selection criteria used for the matched sample to the 120 million weekly baskets (600 million shop visits) in the transaction data set.

matched customers' shopping behavior matches expenditures in the full sample well.

Finally, I calculate car travel times to foreign shopping locations and workplaces. To this end, 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 and a total of 1,787 stores, of which 691 have at least 100 Google ratings. Table A1 lists the largest identified cross-border locations, showing the number of stores with at least 100 and 500 Google ratings. A municipality with a large number of stores typically also has many larger stores with numerous Google reviews, and 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. As cross-border shoppers likely focus on larger stores, I define a cross-border location as a foreign municipality with at least three stores that have more than 100 Google ratings.[8] Next, I scrape 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. Following the same approach, I calculate distances to workplaces. Table 1 shows the average car travel time to the closest cross-border location (57 minutes) and the work location (28 minutes). 59% commute to work by car, while 24.8% use public transportation.

3. Empirical Strategy

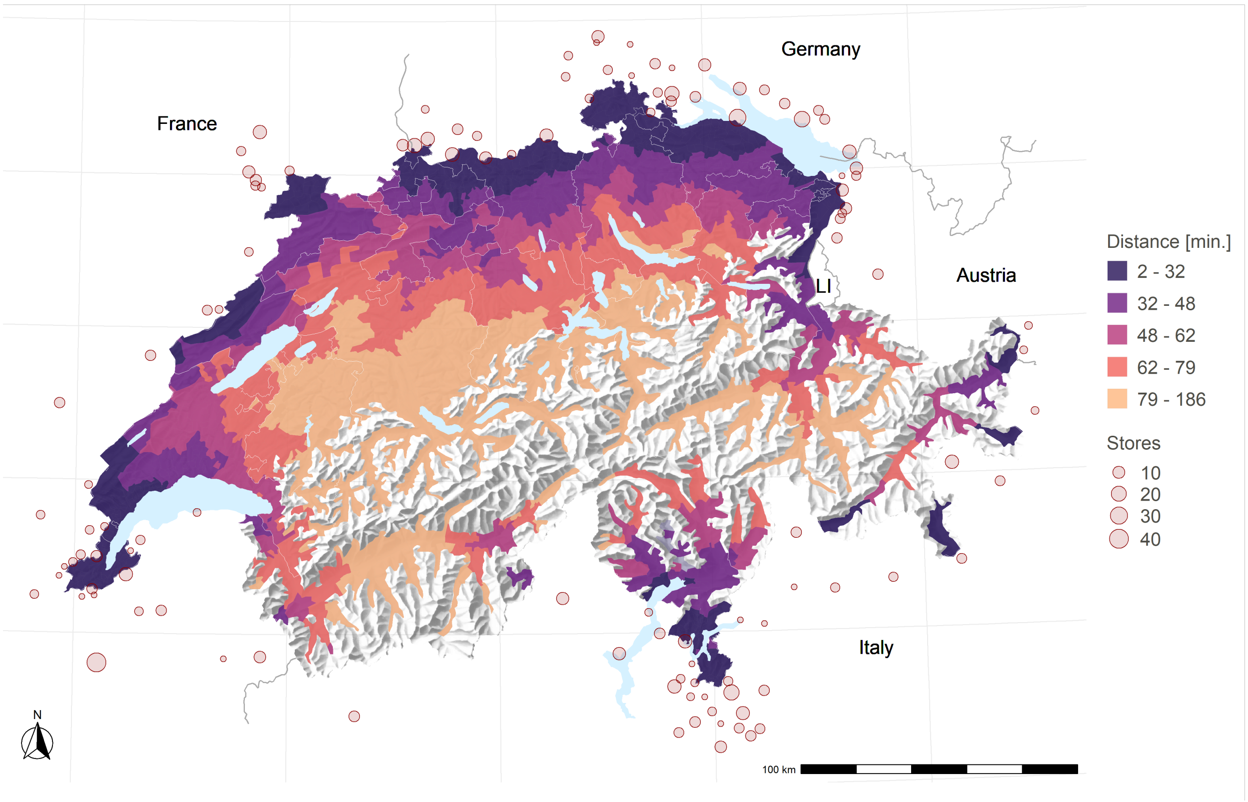
I study the impact of the border closure on household expenditures by comparing households living within a half-hour car drive from a cross-border location (the first quintile) to those living far enough inland such that they typically do not shop abroad. Hence, I define the control group as households living more than 80 car minutes away (the fifth quintile) and drop all individuals living within the doughnut area to ensure a clean control group. This results in a sample of roughly 150,000 treated and control households.[9] Figure 1 shows these travel distance bins to the closest foreign location across Switzerland. 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 morphology of the landscape.

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.

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

[9] If a fraction of control units still reacted to the border closure, my estimates should be regarded as a lower bound. I will address this further in subsection 4.2, showing that my results are robust if I use alternative comparison distances of 90 or 100 minutes.

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 of the Swiss border, and the dots' size indicates the number of supermarkets at this location.

As some households record zero expenditures in a given week, I follow Chen and Roth (2024) and Wooldridge (2023) and estimate the following QMLE-Poisson model:[10]

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

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, the dummy variables T_j indicate the weeks of the year 2020, and β_j are the associated pre- and post-treatment coefficients for each period j. Finally, z_{it} measures the time-varying cantonally reported cases of COVID-19.

[10] Chen and Roth (2024) show that using a linear model with $log(Y + 1)$ as a dependent variable does not allow interpreting the coefficients as percentage changes. Instead, estimating a QMLE-Poisson model and reporting the transformed coefficients $\hat{\beta}_{ATT\%} = exp(\hat{\beta} - 1)$ leads to the desired result.

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. Treatment starts in week twelve, and I normalize coefficients to the average in the pre-treatment period. I cluster standard errors in the QMLE Poisson regressions on the zip-code level and report the transformed coefficients $\hat{\beta}_{ATT\%} = exp(\hat{\beta} - 1)$, which gives the average proportional treatment effects and allows me to interpret the coefficients as percentage changes. I calculate the corresponding standard errors using the delta method.[11]

To analyze heterogeneities in the treatment effect, I use a static model and interact the treatment indicator with a categorical variable x_i:

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

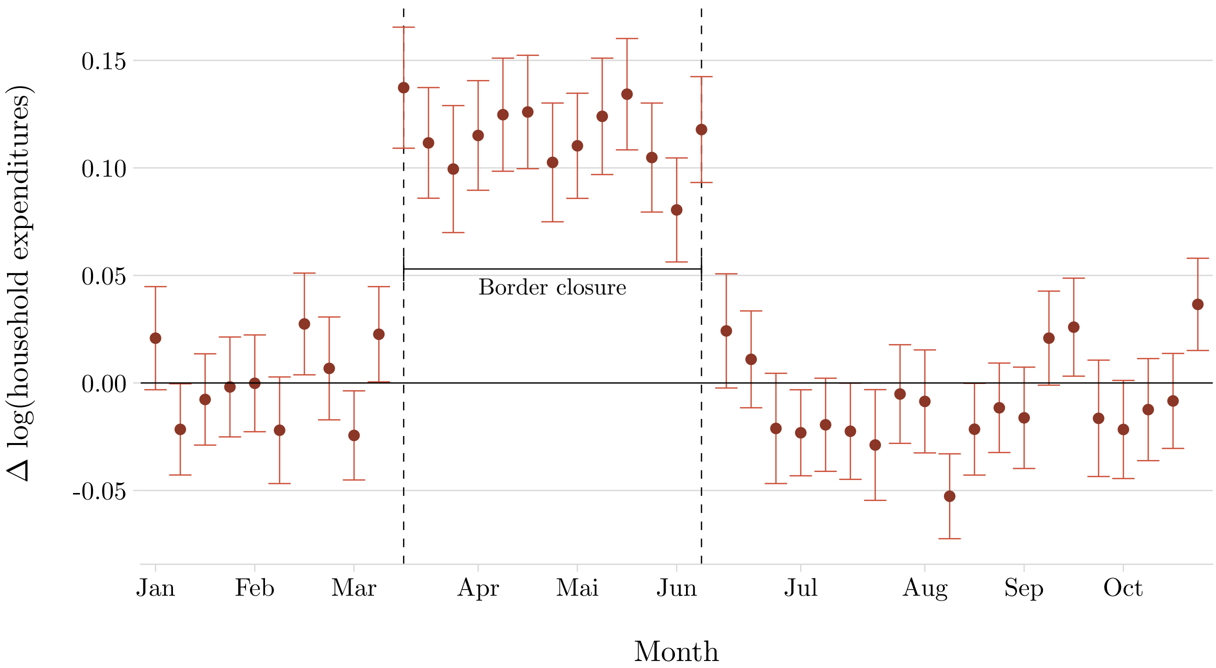
where $Post_t = 1$ if $t \geq 12$, $k \in \mathcal{K}$ indexes the individual categories of x_i, $x_{ik} = \mathbb{1}(x_i = k)$, and β_k is the average treatment effect for each group k. In this specification, the time dimension of the treatment effect collapses to a single post-treatment coefficient. I allow the time fixed 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.

4. Results and Discussion

This section presents three sets of results. First, I study the treatment effects of the border-closing policy on grocery expenditures over time. Second, I examine the effect's decay with distance, assessing how far customers are willing to travel for lower prices. Third, I show diverse heterogeneities of the average treatment effect, including socioeconomic household characteristics, culture, and commuting behavior, as well as foreign grocery prices. This provides rich insights into the varying patterns of consumer mobility in space in response to price differences.

[11] Alternatively, I calculate standard errors from 1,000 clustered bootstrap replications for the main results. The bootstrapped standard errors give similar results.

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 12 million observations. Coefficients are normalized to the pre-treatment periods' average, and standard errors are clustered at the zip code level.

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. Additionally, Table 3 reports the corresponding average treatment effects, grouping the periods during the border closure and after the reopening together. I find that the border closure temporarily increases domestic grocery expenditures by 10.9% at the border in comparison to households residing further inland, with week-specific effects ranging from 8% to 14%. These findings are in line with Burstein, Lein and Vogel (2022), who estimate that Swiss households close to the border spend roughly 8% of their expenditures abroad. Further, this expenditure 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 border closure and completely switched back to their old behavior as soon as

Table 3: Average treatment effects

Dep. Var.: log(Household Expenditures)	
Treat * border closed	0.109***
	(0.006)
Treat * border open	-0.012***
	(0.004)
n	12,030,579

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. The regression follows model (1) but groups the periods during and after the border closure together (*border closed* and *border open*, respectively). Standard errors are clustered at the zip code level.

possible. This result suggests that cross-border shopping follows deeply rooted routines that withstand major temporary shocks. Furthermore, Figure 2 shows that most coefficients in the initial weeks after the reopening are below zero with an average of -1.2%. This increase in cross-border consumption after the reopening is most likely due to a temporary catch-up or stockpiling effect.

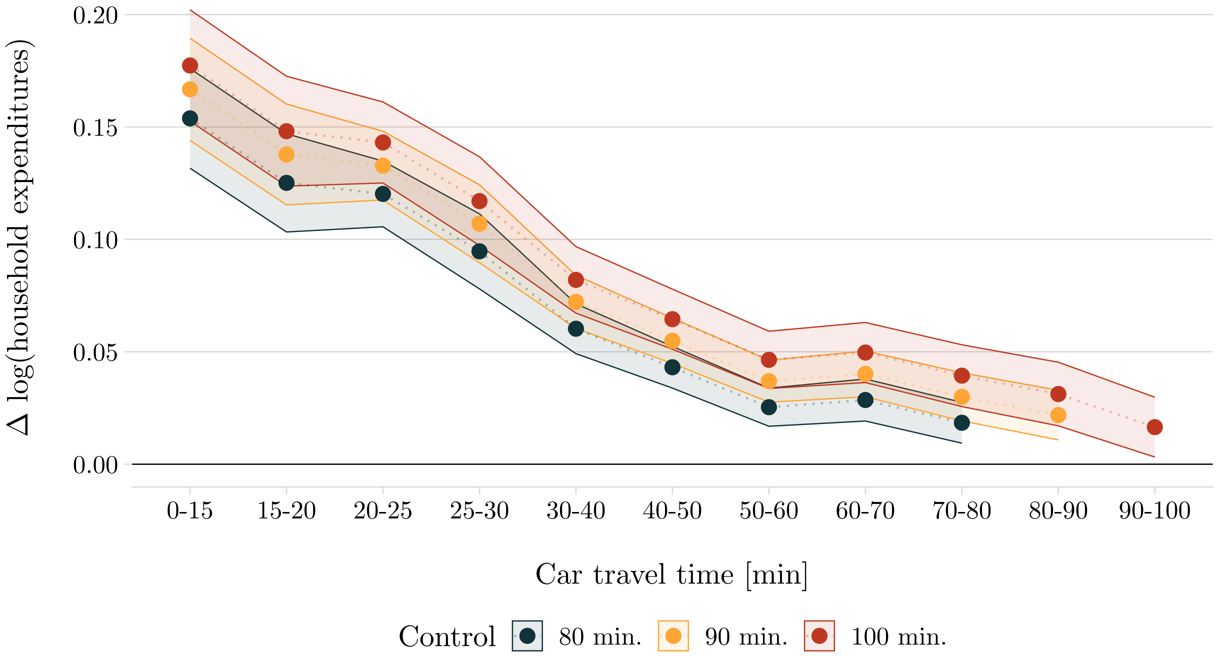
One concern might be that consumers adapted their shopping behavior before the actual introduction of pandemic restrictions, especially in strongly affected areas (for example, in the form of stockpiling or by avoiding larger crowds). Yet, the insignificant pre-treatment coefficients in Figure 2 do not indicate a potential 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. The distance decay function

Throughout this paper, I choose a doughnut–specification with control households living at least an 80-minute car drive from the closest cross-border shopping location. Yet, choosing the radius of the inner doughnut defines the households left out in my analysis and features a trade-off between (i) ensuring that the control units are not contaminated by the treatment and (ii) having a large and representative enough control group. If households living 80 minutes from a cross-border location are still affected, my results should be regarded as lower bounds.

To investigate this, I now consider larger doughnut areas. Figure 3 compares the distance decay

Figure 3: 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, 90, and 100 minutes from the closest cross-border location, respectively. Standard errors are clustered at the zip code level. The regressions estimate model (2) and use 17.4 million observations in all three cases.

function for my preferred specification to two alternative approaches based on control households with at least a 90-minute and 100-minute trip to the closest cross-border location. The results indicate that some control units in my baseline results are possibly still affected by the border closure, as the coefficient for the last distance bin is significant. As the alternative approaches consistently report higher point estimates, I likely underestimate the true effect. On the other hand, the size of the control group shrinks significantly from 150,000 to 68,000 and 28,000 households for the stricter definitions of control units. To balance this trade-off, I select the most conservative approach and present in the paper all estimates with a control group consisting of households living 80 minutes from the border. Figure A2 and Table A2 to Table A6 in the Appendix replicate all results for a control distance of 100 minutes and show that all conclusions remain the same.

Focusing on the preferred specification of 80 minutes in Figure 3, I find that households living within a short distance of 15 minutes from the closest cross-border destination increase their expenditures by 16% during the border closure. This effect first declines linearly up to a distance of 50 minutes

Table 4: Treatment effects by socioeconomic subgroups

Dep. Variable: log(Household Expenditures)							
a) Household size		b) Age		c) Income		d) Education	
Group	Coeff	Group	Coeff	Group	Coeff	Group	Coeff
1	0.068***	20–34	0.138***	Q1	0.150***	Primary	0.137***
	(0.006)		(0.010)		(0.008)		(0.010)
2	0.103***	35–44	0.142***	Q2	0.144***	Secondary	0.108***
	(0.007)		(0.009)		(0.008)		(0.006)
3-4	0.136***	45–54	0.134***	Q3	0.128***	Tertiary	0.108***
	(0.008)		(0.008)		(0.008)		(0.007)
≥5	0.145***	55–64	0.122***	Q4	0.117***		
	(0.009)		(0.008)		(0.007)		
		65–74	0.130***	Q5	0.099***		
			(0.009)		(0.009)		
		75+	0.114***				
			(0.010)				
p-value	0.000	p-value	0.014	p-value	0.000	p-value	0.007
n	6,434,950	n	6,433,731	n	5,148,635	n	4,199,790

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.

before flattening out, although remaining significant for at least 80 minutes. 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 rather than at the closest location.

4.3. Variation across socioeconomic characteristics

Consumers' preferences for cross-border shopping may vary based on their socioeconomic background. Hence, I analyze treatment effect heterogeneities for different household characteristics and Table 4 reports the estimation results of model (2) for the variables household size, age, income, and education in the panels a) to d). The table also reports p-values, testing the treatment effects' equality over the different groups (meaning, the null hypothesis is $\beta_k = \beta \ \forall k$).

First, I find that the effect increases in household size. While a one-person household increases their expenditures by 6.8% in response to the border closure, I document an increase by 10.3% for two-person households, and by 14% 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 2 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 heterogeneous effects over age in the response to the border closure. The estimated effect lies around 14% for young households between age 20 and 44 and decreases slowly as households become older. Yet, even retired households after age 65 show a relatively high response of roughly 12%, while their total expenditures are markedly lower (see Table 2). This result is likely driven by their sharp decline in income after retirement induces them to still shop abroad at lower prices, and their opportunity costs are lower. Note that this heterogeneity can either be due to age or cohort effects, as the short sample period does not allow for disentangling them.

Third, I look at income. On the one hand, one should 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. For instance, high-income households in my data (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). On the other hand, lower car ownership might constrain the mobility of less affluent households. 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), according to the Federal Statistical Office. Similarly, lower-income households travel, on average, shorter distances on a given day (30.2 kilometers vs. 40.8 kilometers).
The results in panel c) show that the first argument dominates the narrative: the treatment effect decreases from 15.0% for the lowest-earning quintile to 9.9% for the highest-earning households. Hence, although traveling costs are relatively high for many of them, lower-income households still

Table 5: Treatment effects by cultural and spatial subgroups

Dep. Variable: log(Household Expenditures)			
a) Nationality		b) Country	
Group	Coeff	Group	Coeff
African	0.197***	AT	0.074***
	(0.038)		(0.013)
Asian	0.163***	GER	0.110***
	(0.025)		(0.008)
European	0.155***	FR	0.120***
	(0.012)		(0.009)
N.American	0.166**	IT	0.350***
	(0.062)		(0.040)
S.American	0.120**		
	(0.041)		
Swiss	0.105***		
	(0.006)		
p-value	0.000	p-value	0.000
n	6,434,398	n	6,235,192

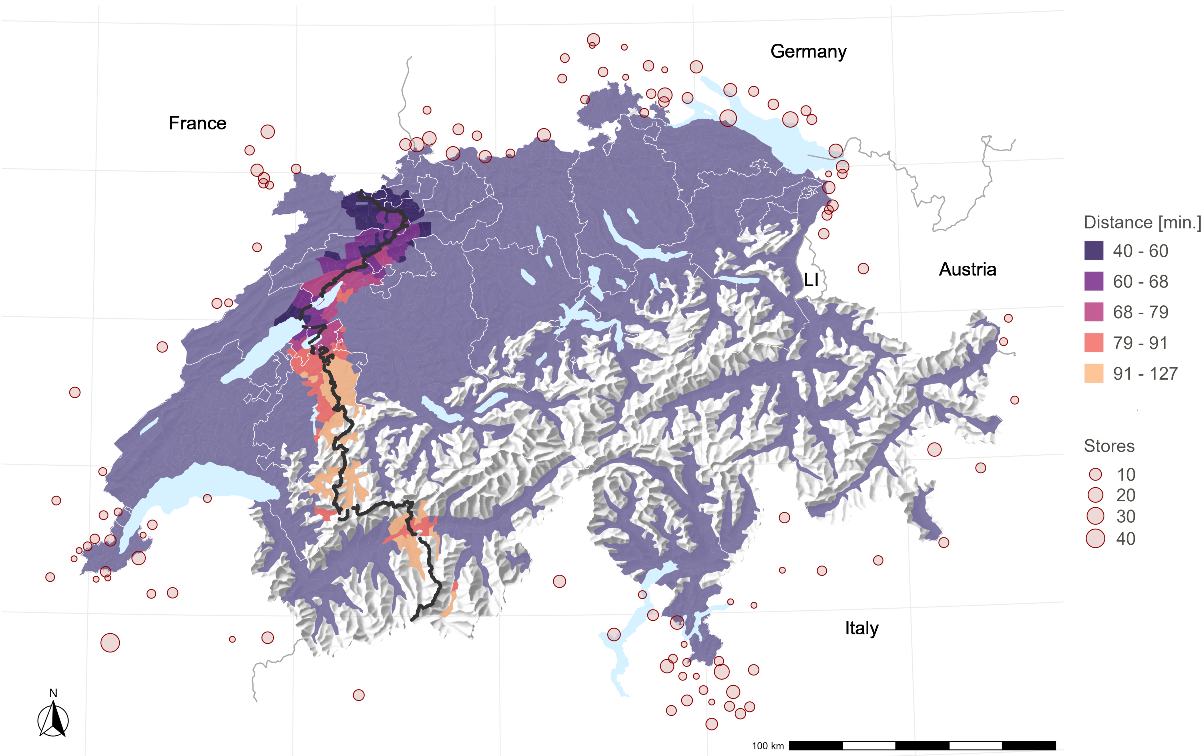
Notes: The table shows the border closure's average treatment effect on household expenditures within a 30-minute car ride from a cross-border location compared to households living further away than 80 minutes, separately for different household characteristics. These characteristics include the cardholders' *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.

engage in more cross-border shopping activity.

Fourth, higher-educated individuals may have broader knowledge and access to more information to strategically optimize their consumption behavior while being less budget-constrained. I find that households with at least one member holding a tertiary education react less to the border closure than comparable households further inland. While high-educated households increase their expenditures by 10.8%, I estimate a higher effect of 13.7% for low-educated households.

Overall, these socioeconomic heterogeneities suggest that many households engage in cross-border shopping either (i) because of large potential savings relative to their low income or (ii) because they have high overall grocery expenditures and can, therefore, save more money in absolute terms.

Figure 4: German-French language border



Notes: The figure shows the quintiles of car driving times to the closest cross-border shopping location in a 20-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.

4.4. Culture

Beyond the socioeconomic background of households, I address the role of cultural differences, as citizens from various cultural origins may have different shopping preferences. 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 within Switzerland.

To begin with, Panel a) in Table 5 shows the heterogeneous response of individuals from different 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% more in response to the border closure, while other Europeans, North Americans, and

Table 6: Cultural differences: effect at language border

	Dep. Var: log(HH Expenditures)		
Dist. to ntl. border	German	French	p-value
Treat × 30-45 min.	0.101***	0.006	0.000
	(0.012)	(0.015)	
Treat × 45-55 min.	0.055***	0.025	0.175
	(0.016)	(0.017)	
Treat × 55-65 min.	0.041***	0.044***	0.842
	(0.011)	(0.011)	
n	1,158,263		

Notes: The table shows the border closure's average treatment effect on household expenditures for households living within 10 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 (2), and standard errors are clustered at the zip code level. The reported p-values test the equality of the two coefficients in the same distance bin.

Asians increased their expenditures by 15-16%, South Americans by 12%, and Africans by 19%, suggesting cultural differences in the preferences for foreign goods.

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 4 displays the language border crossing the entire country from North to South.[12] I use again model (2) to estimate the treatment effect separately for French- and German-speaking households living within 10 kilometers of the language border compared to households further inland speaking the same language. I estimate treatment effects separately for households living between 30-45, 45-55, and 55-65 minutes from the national border compared to households farther away than 80 minutes.[13] This empirical strategy relies on the testable assumption that households within this 20-kilometer band are comparable. Table 6 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. I find a stronger response for German-speaking households in the first distance bin but no significant difference for the other two

[12] 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.

[13] I cannot report results for households living closer to the next cross-border location, as no household living along the language border can reach a cross-border location in less than 30 minutes.

Table 7: Treatment effect for different commuting behaviors

	Dep. Var: log(HH Expenditures)		
Δ Border Access	Commute towards border	Commute away f. border	p-value
Treat × 5-15 min.	0.145**	0.088***	0.439
	(0.017)	(0.017)	
Treat × 15-25 min.	0.148***	0.107***	0.008
	(0.051)	(0.024)	
n	357,492		

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 and 15-25 minutes, 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 (2) and standard errors are clustered at the zip code level.

bins further inland. One potential explanation for the difference in the first distance bin might be that Germany has lower grocery prices than France (see Table 8). An alternative reason could be that households prefer to shop in the country speaking their own language, and the German stores may be more attractive than the French ones.

4.5. Commuting and trip chaining

A key determinant of a household's shopping behavior may be her daily commute to work (see, for example, Miyauchi, Nakajima and Redding, 2022). 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 distance and traveling costs and influence her likelihood of traveling abroad, even if her workplace lies far away from the border. Hence, I use model (2) 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 live 20 to 35 minutes from the border and report commuting by car.

Table 7 shows the estimation results. On the one hand, households with a commute taking them 5 to 15 minutes closer to the border increase their cross-border shopping by 14.5% in response to the

Table 8: 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.

border closure. For households whose workplace is 15-25 minutes closer to a cross-border location, I estimate an effect of 14.8%. On the other hand, I observe for households commuting away from the border lower effects of 8.8% and 10.7%, respectively. Therefore, these two observations provide conclusive evidence that households combine work commutes with cross-border shopping trips in the form of trip chaining.

4.6. Variation across cross-border locations

Finally, I look at the role of neighboring countries and their grocery prices. Panel b) of Table 5 shows the spatial variation of the effect by estimating heterogeneous treatment effects for the four neighboring countries Austria, Germany, France, and Italy.[14] The results show a large estimate for households living closest to Italy (35%), with smaller values for households living close to Germany, France, and Austria (12%, 11%, and 7.4%, respectively). To assess the role of prices behind these findings, I show in Table 8 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 cor-

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

respond to less Swiss cross-border shopping. Based on a back-of-the-envelope calculation, a 1% increase in the price index of a neighboring country is associated with a 0.61% 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 charges the same prices throughout the country. Yet, not all foreign retailers charge the same prices across the entire country, and foreign prices may be higher close to the Swiss border. 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.

4.7. Robustness

Complementing the previous discussion of the doughnut design in Section 4.2, I discuss two additional robustness checks. First, I report in Figure A3 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. The observed changes are negligible. Second, 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 A4). The changes in the coefficients are minimal, further supporting my fi

5. Conclusion

Cross-border shopping provides researchers with a useful setting to analyze the households' heterogeneous willingness to travel for lower prices. While Friberg, Steen and Ulsaker (2022) show that the traditional study of cross-border shopping through changes in relative prices does not measure cross-border shopping one-to-one, the Swiss COVID-19-related border closure (among others) provides a unique natural experiment that I exploit.

I find that cross-border shopping is a widespread and persistent phenomenon in Switzerland and that domestic sales would be 10.9% higher in border regions without it. I then investigate heterogeneities, indicating that larger, poorer, less-educated, and younger households engage in more cross-border shopping, and that the response is larger if the neighboring country has relatively low grocery price indices. In addition, I provide novel evidence that households commuting towards the border combine their trip to work with shopping abroad. Namely, commuting trips taking a household closer to the border correspond to an expenditure increase, while commuting to a workplace further inland has no effect.

These results have important implications for urban research. First, the uncovered heterogeneities may enhance normative analyses of the optimal spatial supermarket allocation, giving additional weight to households with a lower willingness to travel. Second, my findings might improve policies targeting the negative externalities of cross-border shopping on employment, consumption, sales, and tax collection (see again Leal, López-Laborda and Rodrigo, 2010, Knight and Schiff, 2012, or Baggs, Fung and Lapham, 2018). Ultimately, while numerous spatial models in economics incorporate trips to the agents' workplaces and a broad empirical literature uncovers patterns in commuting behavior, household mobility for shopping remains largely understudied and insufficiently understood. One notable exception is Miyauchi, Nakajima and Redding (2022), who incorporate commuting and shopping trips jointly in a quantitative spatial model. Yet, as they cannot observe expenditures and focus on modeling the trips, they provide an incomplete picture, missing the intensive margin of spatial shopping. Future work could aim to bridge this gap, incorporating the empirical findings on shopping in this and other papers into theoretical models. This would result in a more encompassing picture of the spatial equilibrium and allow for more credible counterfactual analyses.

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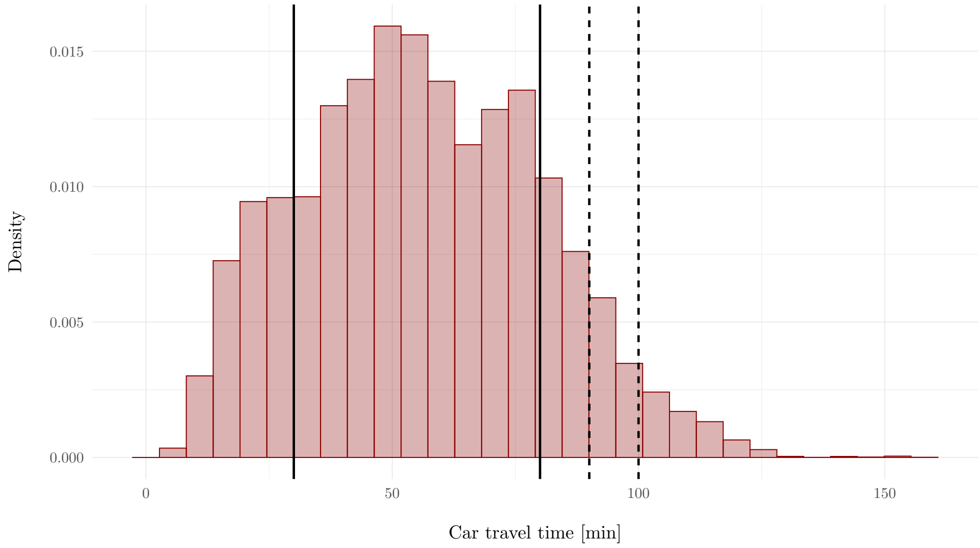
Table A1: Cross-border locations

	Location	Country	Population	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 A1: 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.

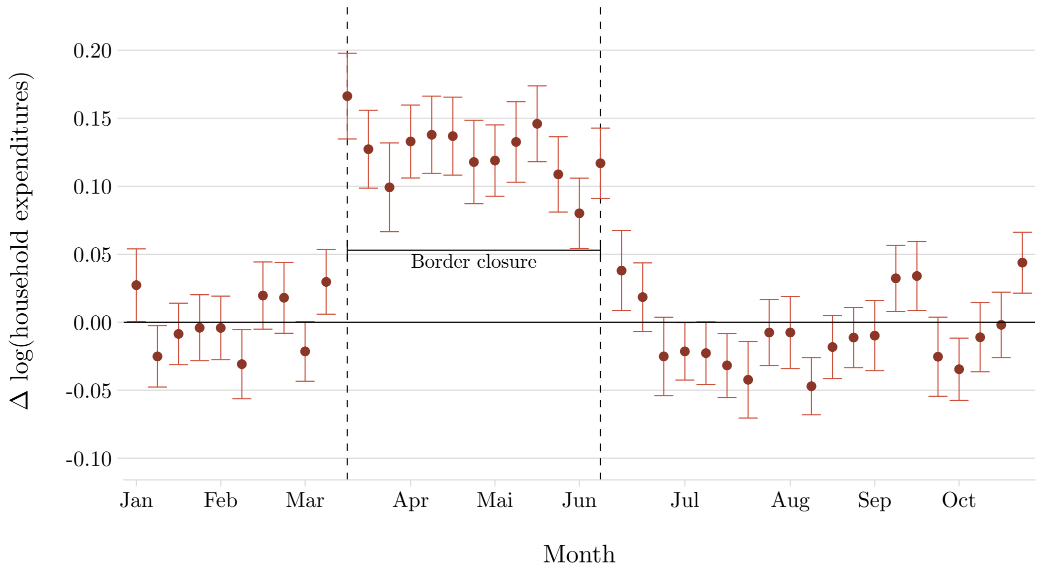
Table A2: Average treatment effects (with a 100-minute control group)

Dep. Var.: log(Household Expenditures)	
Treat * border closed	0.126***
	(0.008)
Treat * border open	-0.008
	(0.005)
n	7,051,422

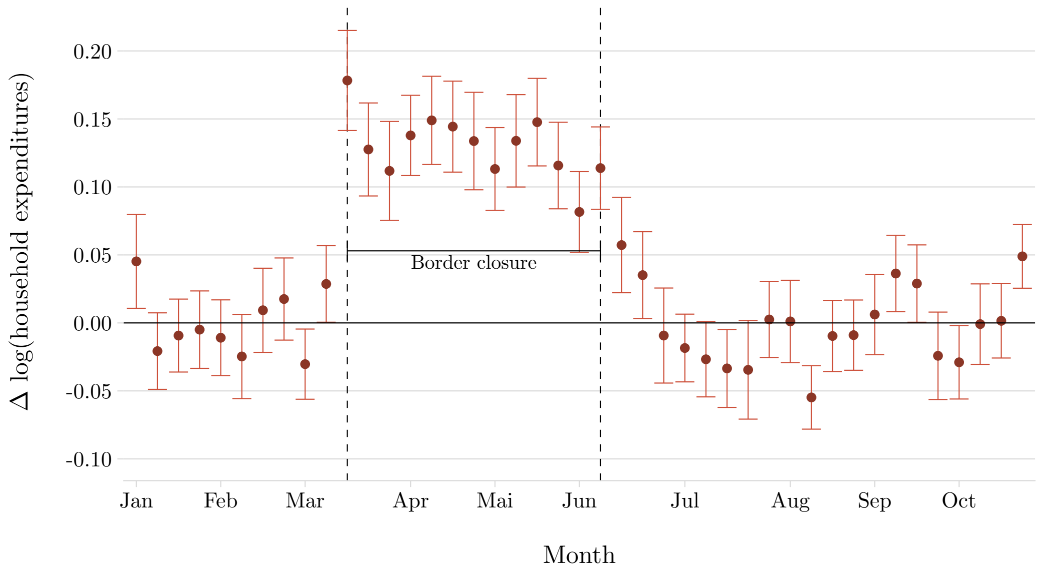
Notes: The table shows the border closure's average treatment effect on household expenditures within a 30-minute car ride from a cross-border location compared to households living further away than 100 minutes. The regression follows model (1) but groups the periods during and after the border closure together (*border closed* and *border open*, respectively). Standard errors are clustered at the zip code level.

Figure A2: 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 A2a 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. I indicate the period of border closure by vertical dashed lines. The regression estimates model (1) and uses 8.8 million observations. Figure A2b also estimates model (1) for a distance of 100 minutes using 7.1 million observations. Coefficients are normalized to the pre-treatment periods' average, and standard errors are clustered at the zip code level.

Table A3: Treatment effects by socioeconomic subgroups (with a 100-minute control group)

Dep. Variable: log(Household Expenditures)							
a) Household size		b) Age		b) Income		b) Education	
Group	Coeff	Group	Coeff	Group	Coeff	Group	Coeff
1	0.095***	20–34	0.152***	Q1	0.155***	Primary	0.134***
	(0.011)		(0.016)		(0.010)		(0.016)
2	0.117***	35–44	0.164***	Q2	0.145***	Secondary	0.111***
	(0.008)		(0.012)		(0.011)		(0.009)
3-4	0.152***	45–54	0.153***	Q3	0.133***	Tertiary	0.130***
	(0.010)		(0.011)		(0.011)		(0.011)
$\geq$5	0.162***	55–64	0.140***	Q4	0.132***		
	(0.014)		(0.011)		(0.011)		
		65–74	0.147***	Q5	0.132***		
			(0.011)		(0.014)		
		75+	0.131***				
			(0.013)				
p-value	0.000	p-value	0.220	p-value	0.199	p-value	0.062
n	3,771,701	n	3,770,827	n	2,979,910	n	2,509,512

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 100 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.

Table A4: Treatment effects by cultural and spatial subgroups (with a 100-minute control group)

Dep. Variable: log(Household Expenditures)			
a) Nationality		b) Country	
Group	Coeff	Group	Coeff
African	0.169**	AT	0.097**
	(0.059)		(0.034)
Asian	0.174***	GER	0.129***
	(0.044)		(0.010)
European	0.168***	FR	0.131***
	(0.017)		(0.015)
N.American	0.159*	IT	0.412***
	(0.083)		(0.042)
S.American	0.132*		
	(0.065)		
Swiss	0.124***		
	(0.008)		
p-value	0.071	p-value	0.000
n	3,771,425	n	3,573,599

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 100 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.

Table A5: Cultural differences: effect at language border (with a 100-minute control group)

	Dep. Var: log(HH Expenditures)		
Dist. to ntl. border	German	French	p-value
Treat × 30-45 min.	0.111***	0.014	0.000
	(0.015)	(0.017)	
Treat × 45-55 min.	0.064***	0.034	0.184
	(0.018)	(0.019)	
Treat × 55-65 min.	0.049***	0.053***	0.812
	(0.014)	(0.014)	
n	695,593		

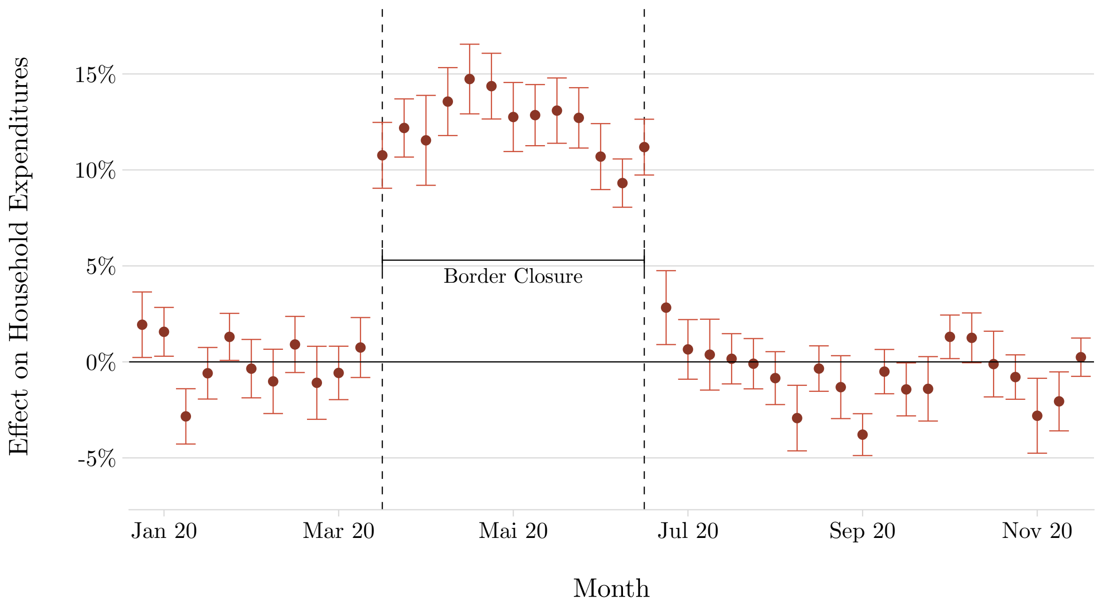
Notes: The table shows the border closure's average treatment effect on household expenditures for households living within 10 kilometers of the German-French language border. I compare these treated units to same-language households living further away than 100 minutes from the closest cross-border location. The regression estimates model (2) and standard errors are clustered at the zip code level.

Table A6: Treatment effect for different commuting behaviors (with a 100-minute control group)

	Dep. Var: log(HH Expenditures)		
Δ Border Access	Commute towards border	Commute away f. border	p-value
Treat × 5-15 min.	0.157**	0.099***	0.459
	(0.020)	(0.020)	
Treat × 15-25 min.	0.158***	0.118***	0.008
	(0.052)	(0.027)	
n	174,180		

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 100 minutes for different household commuting trips. These trips include commutes by car for 5-15 minutes and 15-25 minutes, 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 (2) and standard errors are clustered at the zip code level.

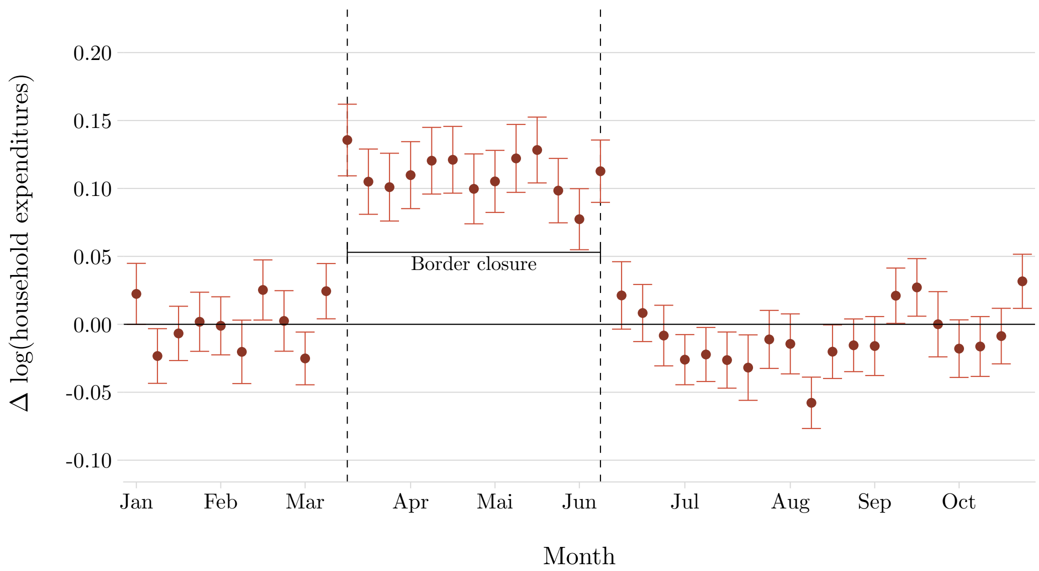
Figure A3: Robustness of the dynamic treatment effects: the full grocery transaction data



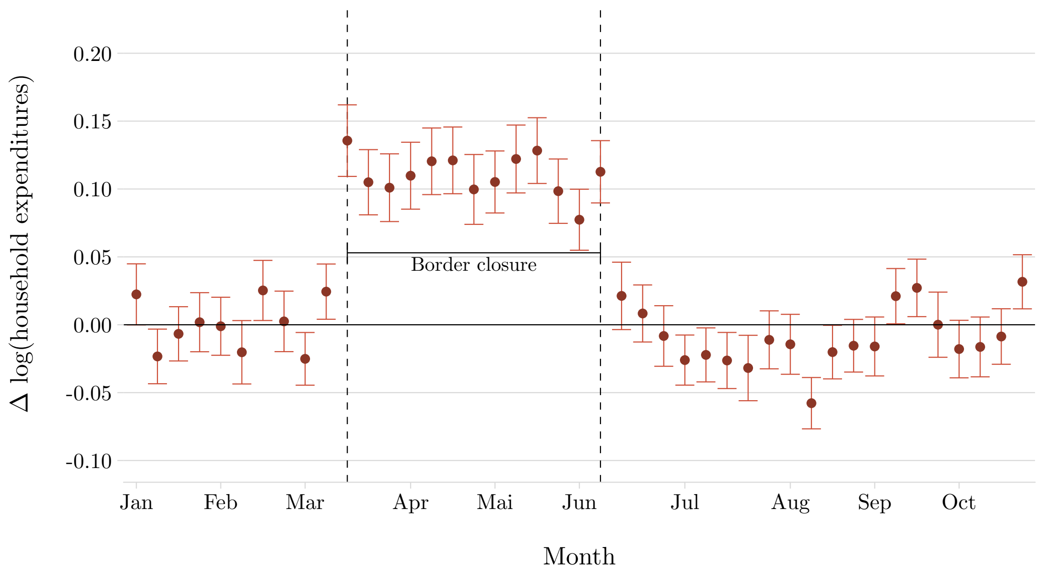
Notes: The figure shows the border closure's effect on household expenditures within a 30-minute car ride from a cross-border location compared to households living further away than 80 minutes. Standard errors are clustered at the zip code level. The regression estimates model (1) and uses 16.6 million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

Figure A4: Robustness of the dynamic treatment effects: different definitions of cross-border locations

(a) At least one store with more than 500 Google reviews



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



Notes: Figure A4a 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 one store with more than 500 Google reviews. In comparison, Figure A4b 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 12 million observations. Standard errors are clustered at the zip code level.