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The effect of cross-border shopping on commodity tax revenue: results from Norway's COVID-19 border closings

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Abstract

We use grocery data from Norway and COVID-19 border closings to gauge the effect of cross-border shopping on commodity tax revenue. Detailed store–category-level data identify differential treatment effects that depend on distance to Swedish stores. Economically significant effects extend to up to two hours' drive from the border, and even further for prominent cross-border shopping products, such as beer, cigarettes, and carbonated soft drinks. Across all products, cross-border shopping decreases tax revenue from VAT by 3.6 percent at the national level. National commodity tax revenue from carbonated soft drinks (subject to a sugar tax) is reduced by 8.1 percent and from cigarettes by 11.9 percent.

Keywords: Cross-border shopping; commodity taxes; excise taxes; tax competition
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1. Introduction

Excise taxes play a key role in limiting the consumption of goods where excessive consumption is seen as harmful to the consumer or to others. Such

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"sin taxes" are pervasive on, for instance, alcohol, gasoline, and tobacco. Excise taxes, and other commodity taxes such as sales tax and value added tax (VAT), are also an important source of tax revenue. Both of these roles are potentially affected by cross-border shopping, where consumers purchase the good at a lower price in a neighboring country. A rich body of literature documents the empirical relevance of such cross-border shopping across national borders. This is manifested in the number of retailers established in several border areas, in local demand, and in how the number of border crossings responds to relative price changes.[1] A related body of literature examines within-country patterns, and documents the crossing of state or municipal borders to avoid local sales and excise taxes on, for instance, cigarettes (Lovenheim, 2008) and gasoline (Manuszak and Moul, 2009).[2] A case of much recent policy interest is that of taxes on sugar-sweetened beverages, where city-level taxes have been shown to lead to substantial substitution towards out-of-town purchases.[3]

In this paper, we estimate the impact of cross-border shopping on commodity tax revenue. We use weekly store–category-level data from a Norwegian grocery retail chain and the natural experiment provided by border closures in connection with the COVID-19 pandemic to estimate the quantitative impact of cross-border shopping.

Two features of the setting and data are important in helping us to identify the impact of cross-border shopping on sales and commodity tax revenue. First, as we document at length in Section 6, Norway's geography – with large swaths of the country many hours' drive from the border – creates a clear control group against which to gauge the impact of closing the border on the stores in "treated" areas closer to the long Norwegian–Swedish border. Combined with strict border closings, this creates a natural experiment with the kind of autarky flavor often assumed in theoretical work in international economics, but rarely observed in practice; see, for example, Bernhofen and Brown (2005), who perform an empirical study of Japan's opening up to trade in the 1800s, which is a notable study in the same vein. Second, category-level sales from a large number of stores across the country allow us to measure the quantitative effect of cross-border shopping on commodity tax revenue with high precision. Identification is aided by the fact that all the grocery retail chains keep the same prices across all of Norway, which implies that we do not need to control for any endogenous price responses that would be unique to border areas.

[1] See, for instance, Campbell and Lapham (2004), Asplund et al. (2007), Chandra et al. (2014), and Baggs et al. (2018); Leal et al. (2010) provide a survey.
[2] See also Baker et al. (2021) who, in a particularly encompassing study, document an important role for cross-border shopping in how US households respond to changes in local sales taxes.
[3] For an overview, see, for example, Cawley et al. (2019) or Allcott et al. (2019b).

We establish that cross-border shopping has a large effect on sales in areas close to Swedish stores, that effects only gradually diminish with driving duration, and that, broadly speaking, effects are stronger and stretch further inland for products with larger price differences and products that are easy to transport and stockpile. We combine our store–category-level estimates of how sales are affected by cross-border shopping with information on the total number of stores in Norway, and their location, to infer the overall loss in tax revenue. In particular, we calculate losses in VAT and excise taxes that are due to cross-border shopping, and how these losses relate to travel duration to the closest Swedish stores. We take the last full year before the COVID-19 pandemic border closures as our benchmark, and use our estimated effects to calculate a counterfactual outcome with a closed border. In border areas, the effects are large: because of cross-border shopping, VAT revenue is 27 percent lower in stores that are within 30 minutes' driving distance from a Swedish store, 20.2 percent lower in stores that are between 30 and 60 minutes away, and 8.5 percent lower for locations between 60 and 90 minutes away. At the national level, we estimate a VAT loss of 3.6 percent stemming from goods sold in grocery stores a result of cross-border shopping. As explained in Section 5, this number relies on the estimated effects from the stores for which we have access to product-level sales but is then aggregated up to the national level using information on the full set of Norwegian grocery stores.

For several of the products subject to excise taxes, such as carbonated soft drinks (hereafter, "soda"), beer, cigarettes, and snus,[4] excise tax revenue more than doubles in these border areas when we move to a counterfactual situation with no cross-border shopping. Aggregating up to the national level, the effects are still substantial with an estimated loss of excise taxes on, for instance, soda of 8.1 percent and on cigarettes of 11.9 percent.

In total, we estimate that cross-border shopping from Norway to Sweden implies a tax loss of 2.3 billion NOK through lost VAT and excise taxes on products sold in grocery stores.[5] Nearly one-third of this loss comes through lost excise taxes on five top border-trade product categories: beer, cigarettes, snus, soda, and sweets.

The current research thus highlights that the concerns examined in theoretical models of commodity "tax competition" are quantitatively important. In a setting with two countries and costly cross-border shopping, Kanbur and Keen (1993) show that a smaller country will set lower commodity

[4] Snus is a smokeless tobacco product that is placed between the upper lip and gum, which is commonly used in Norway and Sweden. Statistics Norway (2022b) reports that, in 2019, 15 percent of the Norwegian population aged between 16 and 74 were daily users of snus.

[5] An important additional loss of VAT and excise taxes comes from sales of wine and other stronger alcoholic beverages that are sold by the state-owned monopoly alcohol retailer Vinmonopolet.

taxes, and they analyze the case for commodity tax harmonization. Their baseline model has been extended in a number of directions, for instance allowing for one country to decide taxes first (Wang, 1999) and allowing for lower commodity taxes in border regions (Agrawal, 2012).[6]

For VAT revenue, the key motivation for international tax agreements to avoid a "race to the bottom" is clearly tax revenue per se. For goods subject to excise taxes, the tax revenue motivation may be combined with a motivation to limit harm due to externalities (e.g., gasoline taxes to lower greenhouse gas emissions) or "internalities" (e.g., excessive consumption due to self-control problems related to consumption of products such as snus and soda) or a combination of the two (e.g., alcohol and cigarettes). For all the products that we examine and which are subject to excise taxes (alcoholic beverages, sugar-sweetened products, and tobacco), there are well-documented studies showing that they are the subject of substantial cross-border shopping (Leal et al., 2010). With store-level data on the quantity of these products, we are able to contribute to the literature by providing evidence of how volumes sold and tax revenue are affected at locations ranging from below 30 minutes up to 180 minutes away from the border.

The numbers clearly point to difficulties for smaller jurisdictions in maintaining higher excise taxes than neighboring jurisdictions. An application of particular interest may be that of soda taxes. Soda taxes (or, more broadly, excise taxes on non-alcoholic beverages) have been implemented both at a national level (e.g., Chile, France, the UK) and at a city and state level (e.g., Berkeley, Philadelphia, Washington state), and are the subject of a rapidly growing academic literature (for a survey, see, e.g., Allcott et al., 2019b). Most closely related to the current research are studies that explicitly discuss geographical constraints, such as Rojas and Wang (2021), who examine the introduction of these excise taxes in Berkeley and Washington state, and Seiler et al. (2021), who examine the Philadelphia tax. The strength of the effects also at long distances clearly indicates the important constraint that cross-border shopping imposes on excise taxes at a city level or for smaller jurisdictions. Even in large and mountainous Norway, cross-border shopping was arguably

[6]Much of the related empirical literature on tax competition has focused on characterizing the nature of the strategic interaction. Kanbur and Keen (1993) assume that taxes are strategic complements across jurisdictions, an assumption that also fits well with policy discussions aimed at avoiding a "race to the bottom". A notable aspect of some recent work on within-country tax competition is that it indicates that local taxes are strategic substitutes (downward-sloping reaction functions such that lower taxes in one jurisdiction imply higher taxes in another). Such a pattern is, for instance, found for local income taxes across Switzerland in Parchet (2019); see Agrawal et al. (2022) for a survey of tax competition and policy choices across local governments.

an important reason why the excise tax on non-alcoholic beverages was removed in 2021.

To frame the counterfactual calculations of the effects of cross-border shopping on commodity tax revenue, an analogy from the study of the effects of trade policy might be useful. We examine the difference between a current situation and a situation with no cross-border trade, akin to a move to autarky. The border closings give us an unusual opportunity to do this. Alternatively, one can examine the effects of marginal changes in taxes (akin to examining the effect of changes in tariff rates). As seen above, it is well established that cross-border shopping responds to changes in relative prices across borders. In principle, sound estimates of pass-through elasticities of VAT and excise taxes, together with elasticities of local demand in response to relative price changes, could be used to calculate the counterfactual that we examine. This would require assuming that elasticity estimates are valid for a much wider range of prices and taxes than those that underlie their estimation. An additional complication for such an exercise would be that the price elasticity of local demand differs by distance to the border and not necessarily in a linear way. This is examined theoretically and shown empirically (using similar data as in the present study) in Friberg et al. (2022): the response to a price change may be the largest some distance inland (as that is where the extensive margin decision of whether to make the cross-border trip or not bites). With the current natural experiment, we estimate the effect of cross-border shopping directly.

In relation to the literature on trade policy, it may also be useful to highlight the special nature of cross-border shopping in comparison to the welfare effects of international trade in general. In models of international trade, welfare gains are largely a result of specialization according to comparative advantage, with the efficiency gains reflected in lower prices. With cross-border shopping, a very particular form of international trade, consumers also take advantage of lower prices. To the extent that international price differences are the result of excise taxes, which are in place to correct for externalities or internalities, the welfare effects become more complex, however. In particular, individual heterogeneity is likely to play a key role – as, for instance, in work on the welfare effects of soda taxes by Allcott et al. (2019a) – and we stop short of trying to model the full welfare effects of cross-border shopping.

On a related note let us point out that some cross-border shopping would be likely to remain from Norway to Sweden even if all excise taxes were eliminated. For instance, abolishing the excise tax on cigarettes would reduce prices in Norway by around 40 percent if the tax change was fully passed through to consumer prices. However, in 2019, tobacco prices in Sweden were more than 50 percent lower than in Norway. For soda, the excise tax in 2019 constituted about 20 percent of the average price in Norway, while the Swedish prices were close to 40 percent lower than the Norwegian prices.

Removing these two excise taxes alone would have reduced tax revenue from the sample regions by five billion NOK (from only the grocery store sales) but would still come short of completely removing the incentives to buy these products in Sweden.[7] On the one hand, this example illustrates that large cross-border price differences make independent tax policy difficult because the border trade leakage will be significant. On the other hand, the example also illustrates that it is not necessarily revenue-maximizing to reduce taxes, because revenue loss inland might not make up for gains in regions closer to the border.

Lastly, let us also note that our paper relates to other research that has documented large effects of COVID-19 related shocks across many outcomes. For instance, several articles use data from credit cards or bank accounts to examine individual-level changes in spending (Andersen et al., 2022; Bounie et al., 2023). Using data from Denmark, Andersen et al. (2022), for instance, document that individual-level health risks and supply restrictions, rather than income risks, were key in explaining individuals' spending cuts. As to the impact of border closings because of COVID-19, we are aware of one paper that examines how cost-of-living expenses vary with access to cross-border shopping (Burstein et al., 2023, using Swiss homescan data) and one paper that uses simulations based on a pre-pandemic cross-border study to examine effects on retailers – Baggs et al. (2022) on Canada, relying on estimates from Baggs et al. (2018).[8] In contrast, we focus on the consequences of cross-border shopping for excise taxes.

In Section 2, we look more closely at cross-border shopping and the COVID-19 pandemic. Then, in Section 3, we present our data and provide descriptive statistics, before we analyze the effects of cross-border shopping on retail activity in Section 4, and discuss the economic impact in Section 5. In Section 6, we provide a number of robustness tests, and we conclude in Section 7.

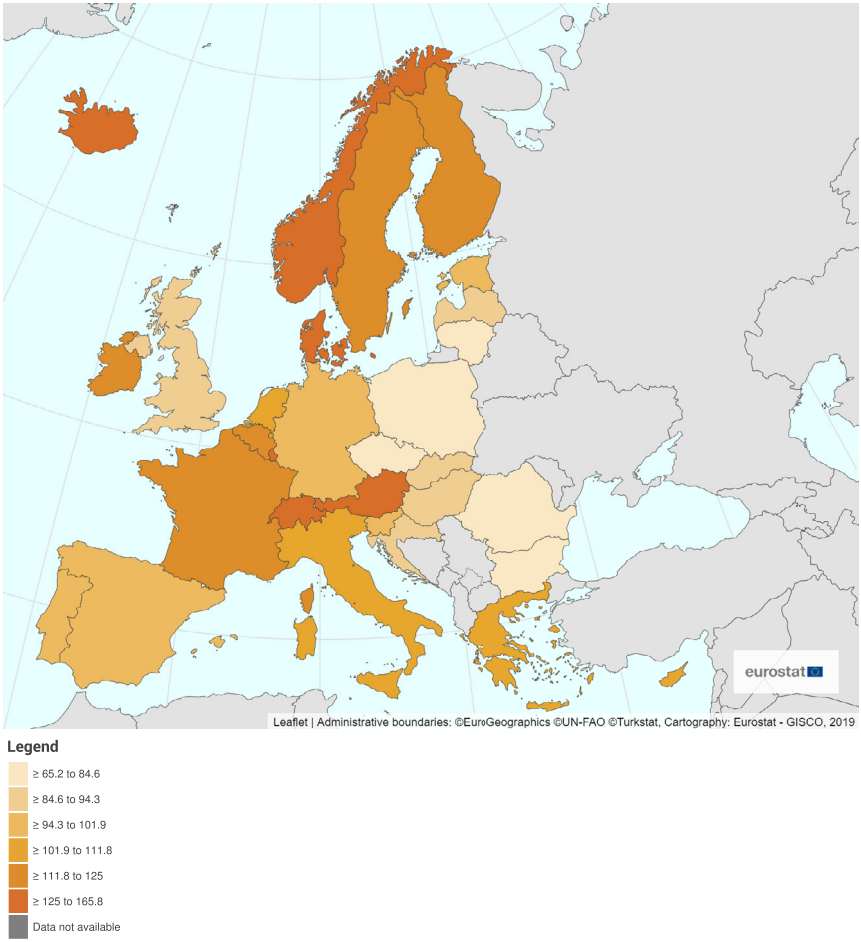
2. Cross-border shopping and the COVID-19 pandemic

Cross-border shopping from Norway into neighboring Sweden features prominently in policy discussions in Norway, and has been the subject of many policy reports (see, e.g., NHO Mat og Drikke, 2020; Skogli et al., 2020; Abel et al., 2021; Statistics Norway, 2022a). Norway is not a member of the European Union (EU) and maintains tariffs on many food products, which,

[7]The loss of five billion NOK is based on actual tax revenue as reported in the last column of Table 9.

[8]See also Leifman et al. (2022) who use national-level data from 14 European countries to establish that COVID-19 border closures were associated with increasing domestic alcohol sales in inflow countries, such as Norway, and falling sales in outflow countries.

Figure 1. Price-level index for food (EU27=100) in European countries in 2019



Notes: This figure shows the purchasing power parities (PPPs), price-level indices, and real expenditures for ESA 2010 aggregates. Geopolitical entity (reporting)/time: 2019. Time frequency: annual. National accounts indicator (ESA 2010): price-level indices (EU27_2020 = 100). Analytical categories for PPPs calculation: food. The level of index for some particularly noteworthy countries is given in bold.

together with a high general price level and high excise taxes, combine to create substantial price differences relative to neighboring Sweden. One way to observe the incentives for cross-border shopping is to examine price-level indices reported by Eurostat at a yearly level for many European countries (Eurostat, 2022). The average across all the 27 EU countries (excluding the UK) is set to 100 in each year. Figure 1 presents the food price index for a set of European countries. For food, the index for Norway is 155.2, thus implying

Table 1. Price-level index for selected product categories (EU27 = 100) for Norway and Sweden in 2019

Product category	Norway	Sweden
Food	155.2	114.8
Alcohol	260.4	155.9
Meat	149.1	115.1
Milk, cheese, and eggs	168.7	109.2
Non-alcoholic beverages	177.8	108.3
Tobacco	234.9	112.3

that food prices in Norway are about 50 percent higher than the average in the EU. Sweden's corresponding price index is 114.8 in the same year. Clearly, a difference of 40.4 percentage points (155.2 − 114.8) suggests a substantial incentive to cross-border shop for food.[9] While the price difference is large, it is notable that it is not unique. We note that, for instance, Switzerland's corresponding price index was 165.8 with a difference of 64.4 percentage points to neighboring Germany, and of 49.9 percentage points to neighboring France. Similarly, Austria's food price index was 41.9 percentage points above that of Czechia.

Table 1 reports the indices for selected categories in 2019 for Norway and Sweden. Price differences are especially marked on products that are protected by agricultural policy (meat, milk, cheese, and eggs) and products subject to excise taxes (alcohol, tobacco, and a "sugar tax"). The corresponding excise tax rates in Sweden were at the time lower (beer, tobacco) or zero (no "sugar tax" in Sweden).[10] VAT rates for groceries are similar in the two countries, 15 percent in Norway and 12 percent in Sweden.

A notable feature of the Norwegian grocery market is that all the chains have national pricing, and prices are thus not endogenously lower closer to the border (Friberg et al., 2022). This means that the relevant price comparison is crucially affected by driving duration to the closest Swedish store. Uniform nationwide pricing may at first seem surprising to many

[9]A set of legal restrictions on cross-border shopping is in place (Norwegian Customs, 2023). A person can bring into the country up to 10 kilos of meat, meat products, and cheese, 2 liters of strong beer (ABV above 2.5 percent), 100 cigarettes, and 125 grams of snus. While random checks are possible, border stations are often unmanned and it is unclear how binding these quotas are for a typical cross-border shopper.

[10]For instance, in 2019, a liter of beer with ABV of 4.6 percent was taxed at 22.4 NOK per liter in Norway (8.65 NOK per liter in Sweden), cigarettes were taxed at 2.63 NOK per cigarette in Norway (1.47 NOK plus 1 percent of retail price in Sweden), and snus was taxed at 1070 NOK per kilo in Norway (420 NOK per kilo in Sweden). In these comparisons, we use average exchange rate for 2019, NOK/SEK = 0.9306 (Norges Bank, 2022), to express Swedish excise taxes in NOK.

economists but is a common feature of grocery retailing; see, for instance, Seaton and Waterson (2013) for UK grocery retailing, or DellaVigna and Gentzkow (2019) for US grocery retailing.

Let us now turn to an overview of the aspects of the COVID-19 pandemic of relevance to the current paper. As in many countries, the development of the pandemic from March 2020 was very rapid, and we have little reason to assume that there were any important anticipatory measures on the part of consumers with regards to cross-border shopping. On 12 March, the Norwegian government implemented a string of severe infection control measures: schools were closed, cultural and sporting events cancelled, and travel restrictions imposed.[11] The measures effectively closed down much of the economic activity and led to the biggest recorded fall in GDP for mainland Norway, and to a rise in the unemployment rate from 3.8 to 15.3 percent (NAV, 2021). On 20 April 2020, a partial reopening of Norway was announced, but the pandemic continued to affect everyday life and economic activity throughout 2020, as restrictions were eased and tightened in response to changes in the infection rates.[12]

The pandemic and the control measures affected the grocery sector in several ways. In March 2020, stockpiling led to sharp spikes in sales but also to temporary stockouts of goods such as toilet paper and flour. Throughout the pandemic, the dramatic increase in remote working, together with closed bars and restaurants, meant that more meals were eaten at home. Especially relevant for our purpose are the restrictions on international travel. Under regulations imposed in March 2020, any international travelers were required to undergo ten days of quarantine, and for the rest of 2020, visits to countries and areas with high infection rates triggered quarantine upon re-entering Norway (Norwegian Directorate of Health, 2020b,a). Consequently, cross-border shopping was infeasible for most of 2020, and practically all food and alcohol had to be bought in Norwegian stores. In the next section, we describe in more detail how different counties in Sweden were effectively opened and closed for cross-border shopping during 2020, as travel restrictions were imposed and lifted.

The estimation of the effects of cross-border shopping on local sales in Norway is made simpler by the fact that the relevant restrictions on

[11] See the article "Norway to take 'strongest-ever peacetime measures' as coronavirus spreads", Reuters, 12 March 2020, https://www.reuters.com/article/us-health-coronavirus-norway-idUSKBN20Z1R8.

[12] For most of 2020, the restrictions were national in scope. Towards the end of the year, there was some geographical variation in the severity of the restrictions in response to local outbreaks. The regulation *Covid-19-forskriften* contained the valid national legislation at any time. All versions of the regulation are available (in Norwegian) at https://lovdata.no/dokument/SF/forskrift/2020-03-27-470.

cross-border shopping were essentially all driven by policy variation in Norway, rather than by policy in neighboring Sweden. Throughout 2020, Sweden largely relied on voluntary measures to limit contagion and enforced light-handed restrictions compared with other European countries (see, e.g., Ludvigsson, 2020; Andersson et al., 2022). Grocery stores enforced caps on the number of customers who were allowed in store, but these were rarely binding.

3. Data and descriptive statistics

The main dataset used for estimation of the effects of cross-border shopping consists of weekly sales and volumes at the store–category level for 2019 and 2020 from a random sample of grocery stores belonging to the largest Norwegian grocery umbrella chain, Norgesgruppen. Norgesgruppen had a market share of 43.7 percent in 2019 and has retail stores across all market segments and across the entire country (Nielsen, 2020).[13] In addition to the sales data, we have information about the number of distinct store visits per week at the store level. Because we are interested in cross-border shopping across the Swedish border, we over-sampled stores located close to the border when selecting our sample of stores. Specifically, 200 stores were drawn randomly from the whole of Norway, and an additional 200 stores were drawn randomly from three counties bordering Sweden (Viken, Innlandet, and Trøndelag). The dataset used for estimation has been created by the authors aggregating up from store–barcode-level data for the full assortment, following product categorizations used by Norgesgruppen. Because we are ultimately interested in overall grocery sales, and not just sales in the sampled stores, we also use information about the location and estimated yearly sales amounts for all Norwegian grocery stores in 2019, obtained from the grocery store database, Geodata.[14]

The Norwegian store data contain the locations of the Norwegian stores. Data on the locations of all grocery stores in Swedish counties that border on Norway were obtained from Delfi Marknadspartner.[15] Using map data from OpenStreetMap, we calculate driving duration in minutes from each Norwegian store to each Swedish store, as outlined in the Online Appendix.

In our analysis, we explore how changes in the accessibility of cross-border shopping affect grocery store sales in Norway. The source of this variation is

[13] There are no hard discounters in Norway, and very few hypermarkets. Discount stores, a segment in which Norgesgruppen also has a presence, constitutes the largest market segment with a market share of about 60 percent in 2019 (Nielsen, 2020).
[14] See https://www.geodata.no.
[15] See https://www.delfi.se.

Table 2. Stores and population by duration category

	<30	[30, 60)	[60, 90)	[90, 120)	[120, 150)	[150, 180)	>180	Total
Stores								
Number of stores	14	33	55	103	41	27	127	400
Proportion	0.04	0.08	0.14	0.26	0.10	0.07	0.32	1.00
Population								
Population	112,040	324,070	1,003,407	1,030,645	266,896	212,793	1,434,226	4,384,077
Proportion	0.03	0.07	0.23	0.24	0.06	0.05	0.33	1.00

Notes: This table shows the number of stores and the population within each duration category in the week starting on Monday 9 March 2020 (i.e., the week before travel to Sweden became restricted). For each duration category, the population is the population of the municipalities with stores in the store sample. Because there are not stores from all municipalities in the store sample, the total population is less than the total population of Norway in 2020.

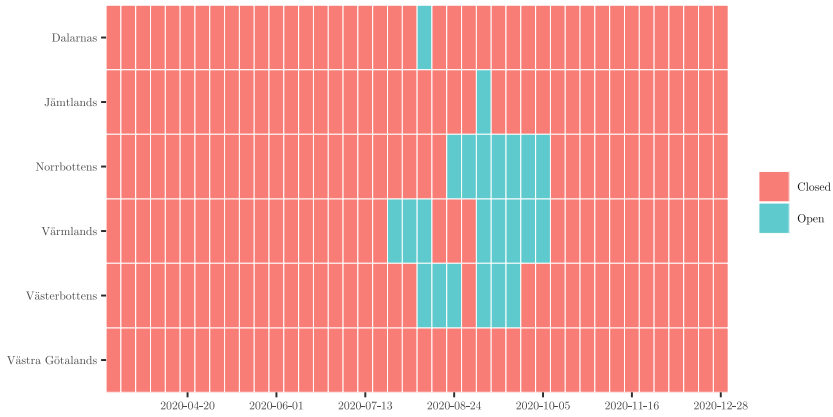
regulations that restrict travel to Sweden. Information about which Swedish counties could be visited without triggering quarantine upon re-entering Norway on given dates during 2020 was obtained from the Norwegian Institute of Public Health.[16] We refer to a Swedish county as open for cross-border shopping whenever it can be visited without triggering quarantine, and closed for cross-border shopping otherwise. Figure IA.2 in the Online Appendix plots traffic flows at the main border crossings into Sweden and clearly indicates the impact of the restrictions on travel.

Our main measure of the availability of cross-border shopping is the driving duration to the closest Swedish store, which is located in a border county and is open for cross-border shopping. From the start of our sample period in January 2019 until 17 March 2020, all Swedish border counties were open for border shopping. In the following, we refer to these driving durations as pre-COVID driving durations. In Table 2, we report the distribution of pre-COVID driving durations, where we use 30-minute bins (up to 180 minutes) to categorize the stores.

From Tuesday 17 March until 25 July 2020, Norwegians had no access to cross-border shopping in Sweden and the border was effectively closed to any private travel (unless one was willing to undergo ten days of quarantine on the return to Norway). As discussed below, in relation to our examination of the robustness of our empirical estimates, a recent empirical literature has highlighted concerns with difference-in-difference estimates when treatment is heterogeneous in terms of timing (see, e.g., De Chaisemartin and d'Haultfoeuille, 2020). A treatment that puts a stop to cross-border shopping

[16]Earlier versions of maps – entry quarantine, accessed on 12 August 2021, at https://web.archive.org/web/20210726041830/https://www.fhi.no/en/id/infectious-diseases/coronavirus/daily-reports/earlier-versions-of-maps/.

Figure 2. Accessibility of Swedish border counties



Notes: This figure indicates which weeks the Swedish border counties were closed to cross-border shopping in 2020. We define a county as closed in a given week if in the majority of the days of the week, visiting the county triggered quarantine upon returning to Norway. The first week shown is the week starting on Monday 16 March 2020. Travels to Sweden were restricted from Tuesday 17 March 2020.

across the whole country on the same day clearly avoids such concerns. Towards the end of the sample period, there is, however, some variation across locations in border closings. Between 25 July and 9 October 2020, cross-border travel to some Swedish counties was allowed without triggering quarantine, based on local infection rates. For the period 9 October 2020, until the end of the sample period, the border was then again closed for all cross-border shoppers unless one was willing to undergo quarantine.[17] Figure 2 details which Swedish counties were open for cross-border shopping during which weeks. For the summer and early autumn period, when some Swedish locations were open for cross-border shopping, we keep track of the driving duration to the closest accessible Swedish store as it varies due to border closures. In Figure IA.4 in the Online Appendix, we use maps to illustrate how driving durations to the closest available Swedish store varied during the late summer and early autumn of 2020.

In our empirical analyses, we include a number of municipality-level control variables that may both affect grocery store activity and be affected by the COVID-19 pandemic. The variables we consider are COVID-19

[17]COVID-19 restrictions also implied that cross-border commuting was affected by quarantine rules. Because the wage level is higher in Norway than in Sweden, this mainly affected Swedes working in Norway and we do not expect any particularly important effect of this on cross-border shopping, as such commuters were likely to shop in Sweden throughout. See Statistics Norway (2021b) for a further analysis of cross-border work into Norway.

infection rates,[18] unemployment rates (Norwegian Labour and Welfare Administration, 2021), population (Statistics Norway, 2021c), and the proportion of residential dwellings in the municipality that are holiday homes (Statistics Norway, 2021a). In Table 3, we present mean values for these variables, along with the outcome variables we consider in our empirical analysis. We break down the descriptive statistics by year, and by the pre-COVID travel duration category.

Some interesting patterns emerge from Table 3. First, we see that average weekly sales at the store level are significantly higher in 2020 than in 2019, regardless of the driving duration to Sweden. This indicates that the pandemic had a positive impact on grocery store sales also in regions where cross-border shopping is unlikely to be an important factor. As discussed above, this can be explained by factors such as closed restaurants, remote working, and restrictions on international travel. However, the relative growth is significantly larger for stores located close to Sweden than for stores located further away. We also see that average number of visits per store and per week is higher in 2020 than in 2019 for the <30 minutes and 30–60 minutes categories, but that the reverse is true for the other duration categories. However, basket size is increasing from 2019 to 2020 in all duration categories. There is substantial variation in the average number of new COVID-19 cases in the municipality in which a store is located. Infection rates are highest in the 60–90 minutes and 90–120 minutes categories. The mean unemployment rate is growing from 2019 to 2020 across the duration groups, with no clear indications that some duration categories are affected more than others. Finally, mean population in the municipality in which a store is located varies quite a bit between the duration groups, but changes between 2019 and 2020 are relatively small.

Table 3 already gives an indication that the COVID-19 pandemic affected grocery store activity in regions close to Sweden differently than in regions further from the border. Our hypothesis is that this difference is driven by travel restrictions that made cross-border shopping more difficult (indeed mostly impossible) during the pandemic. Figure 3 provides some initial informal support for our hypothesis. The top panels of Figure 3 illustrate how, prior to the pandemic, product categories that are well suited for cross-border shopping (beer, cheese, meat, soda, sweets, and tobacco) account for an increasing share of store-level sales as we move away from the border. Conversely, product categories that are less suitable for cross-border shopping (freshly baked products, ice cream, milk, and ready-made food) account for a decreasing share. However, as can be seen from the bottom panels of Figure 3, these

[18]Taken from the regularly updated Norwegian COVID-19 data of *Folkehelseinstituttet* (the Norwegian Institute of Public Health, https://github.com/folkehelseinstituttet.

Table 3. Descriptive statistics

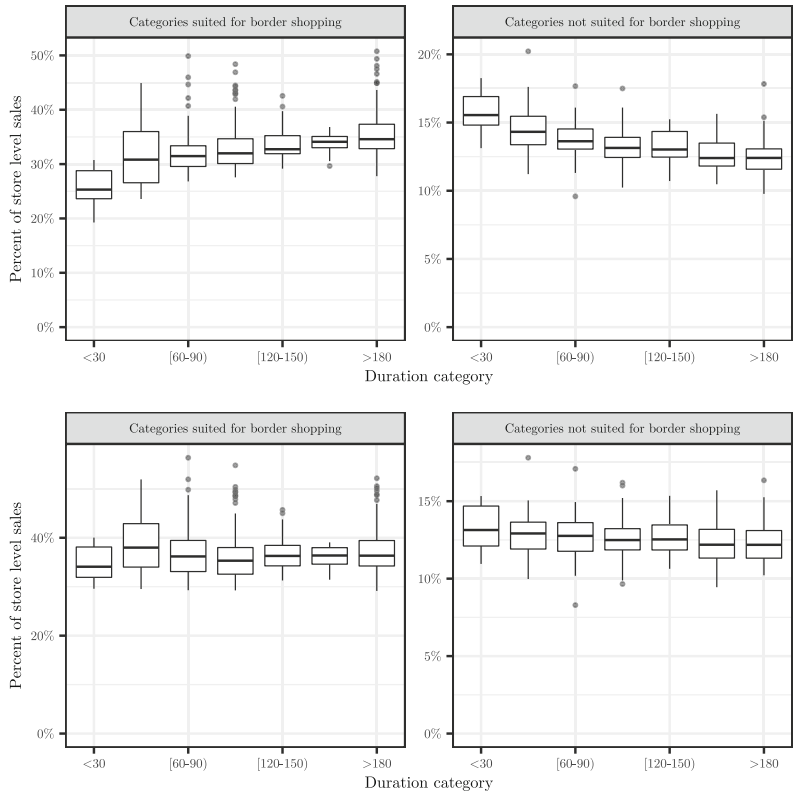
	<30		[30, 60)		[60, 90)		[90, 120)		[120, 150)		[150, 180)		>180	
	2019	2020	2019	2020	2019	2020	2019	2020	2019	2020	2019	2020	2019	2020
Outcome variables														
Store level														
Store sales (millions NOK)	0.97	1.38	0.92	1.20	1.40	1.67	1.60	1.88	1.17	1.31	1.09	1.22	0.84	0.94
Store visits	4,591	5,071	4,179	4,479	5,891	5,870	6,703	6,627	5,104	4,927	4,350	4,189	3,734	3,603
Basket size (NOK)	206.7	263.8	206.6	253.1	226.1	269.8	230.9	273.7	227.1	266.2	247.4	291.7	229.7	269.0
VAT (thousands NOK)	136	197	131	174	198	240	226	269	167	190	157	178	121	137
Category level														
Ice cream sales (thousands NOK)	15.9	22.4	14.6	19.8	20.0	25.8	21.3	27.6	14.1	17.4	12.7	16.1	11.0	13.8
Milk sales (thousands NOK)	31.0	39.0	28.3	32.5	43.1	47.1	45.3	49.6	35.0	36.2	35.1	36.2	26.5	27.8
Fresh bake sales (thousands NOK)	47.1	53.4	43.2	47.7	59.4	62.9	63.4	67.7	48.4	49.9	40.6	41.2	31.2	32.3
Ready-made sales (thousands NOK)	45.5	54.2	38.3	44.6	56.1	61.7	64.5	71.7	44.2	47.7	35.4	38.6	26.6	29.0
Cheese sales (thousands NOK)	42.3	58.9	39.2	50.3	61.9	73.1	75.2	88.8	50.9	55.7	48.6	53.3	34.9	38.7
Meat sales (thousands NOK)	43.6	81.2	46.6	70.7	81.2	104.8	99.7	124.7	66.0	80.8	56.6	68.8	42.0	49.9
Sweets sales (thousands NOK)	39.2	57.9	37.5	49.9	57.8	69.6	66.1	78.5	52.0	58.3	49.9	56.4	38.3	43.6
Sweets sales (kilograms)	167.2	238.3	153.8	193.9	236.2	270.5	273.8	310.4	215.2	228.3	216.2	229.2	163.9	174.6
Soda sales (thousands NOK)	32.3	61.8	35.4	53.3	54.8	67.6	61.1	72.7	52.0	59.0	49.4	56.0	37.8	41.1
Soda sales (liters)	1,435	3,227	1,520	2,535	2,408	3,139	2,672	3,278	2,397	2,810	2,318	2,692	1,686	1,866
Beer sales (thousands NOK)	46.0	82.2	51.8	79.6	75.5	100.1	85.2	110.9	65.7	81.5	65.9	80.2	53.3	63.4
Beer sales (liters)	782	1,368	845	1,267	1,226	1,587	1,325	1,688	1,046	1,268	1,065	1,268	868	987
Cigarette sales (thousands NOK)	17.5	51.7	26.8	58.0	40.9	67.6	42.0	61.4	41.4	57.1	41.9	56.1	32.5	41.2
Cigarette sales (items)	2,897	8,205	4,338	9,005	6,569	10,434	6,760	9,491	6,713	8,877	6,917	8,904	5,308	6,459
Snus sales (thousands NOK)	7.5	23.6	12.1	28.2	23.1	38.0	30.0	44.1	23.6	33.7	24.1	33.2	23.0	29.2
Snus sales (kilograms)	1.8	5.5	2.7	6.3	5.1	8.5	6.5	9.5	5.2	7.4	5.5	7.6	5.0	6.3

Table 3. Continued

	<30		[30, 60)		[60, 90)		[90, 120)		[120, 150)		[150, 180)		>180	
	2019	2020	2019	2020	2019	2020	2019	2020	2019	2020	2019	2020	2019	2020
Control variables														
New COVID-19 cases	0.0	25.8	0.0	23.8	0.0	88.0	0.0	81.3	0.0	9.5	0.0	2.8	0.0	15.4
Holiday home proportion	0.11	0.11	0.14	0.13	0.10	0.10	0.07	0.07	0.17	0.17	0.28	0.28	0.27	0.27
Unemployment rate	0.03	0.06	0.03	0.05	0.02	0.05	0.02	0.05	0.02	0.04	0.02	0.04	0.02	0.04
Population (in thousands)	45.5	45.9	42.5	42.8	134.3	136.1	147.4	149.0	25.8	25.9	11.9	12.2	44.2	43.9

Notes: This table shows the mean value of the outcome and control variables. All variables are measured at the week-store level. The mean values are calculated by year and duration groups, where the duration group is defined by the driving duration to the closest Swedish store when all Swedish counties were open for cross-border shopping.

Figure 3. Category sales shares before (top panels) and during (bottom panels) the COVID-19 pandemic



Notes: The panels on the left-hand side show boxplots of the share of total store-level sales that are accounted for by the categories sweets, soda, cheese, meat, beer, and tobacco. The panels on the right-hand side show boxplots of the share of total store-level sales that are accounted for by the categories ice cream, milk, freshly baked, and ready-made. The period in the top panels is all weeks between the week starting on Monday 31 December 2018 and the week starting on Monday 2 March 2020; in the bottom panels, it is all weeks between the week starting on Monday 16 March 2020 and the week starting on Monday 13 July 2020. During these weeks, all Swedish border counties were closed for cross-border shopping. The lower and upper hinges correspond to the first and third quartiles. The upper whisker extends from the hinge to the largest value no further than 1.5 × IQR from the hinge. The lower whisker extends from the hinge to the smallest value at most 1.5 × IQR of the hinge.

trends are not prominent in the period from March to July 2020 when all Swedish border regions were closed for cross-border shopping.[19]

[19] As reported in Table IA.1 in the Online Appendix, the differences between the sales shares in the >180 minute category and the other duration categories are mostly highly statistically significant in the pre-COVID period, and mostly statistically insignificant in the period during the pandemic.

4. The effect on cross-border shopping retail activity

We are interested in estimating the effect of the availability of cross-border shopping on grocery sales in Norway. Our empirical strategy exploits the fact that travel restrictions due to the COVID-19 pandemic introduced within-store variability with regards to the accessibility of border shopping in Sweden. In addition, because buying groceries in Sweden is not an equally viable alternative to buying groceries locally across Norway, the travel restrictions do not affect all Norwegian stores in the same way. This enables us to estimate the effect of cross-border shopping on retail activity by including both store fixed effects (capturing unobservable time-invariant differences between the stores) and time fixed effects (capturing country-wide shocks that affect all the stores equally).

We follow Friberg et al. (2022) in considering the effect of cross-border shopping on retail activity in Norwegian stores that are located within a 180-minute drive from the closest Swedish grocery store. The variable B_{st} takes the value one if there is an accessible Swedish store less than a 180-minute drive from store s in period t, and takes the value zero otherwise. A Swedish store will be considered accessible if it can be visited without triggering a quarantine stay upon re-entering Norway. In periods where no Swedish stores are accessible, B_{st} will be zero for all stores in the dataset. We assume that cross-border shopping is not a viable alternative for stores located 180 minutes or more from the closest Swedish grocery store. This group of stores will therefore be assumed to be unaffected by the travel restrictions, effectively functioning as a control group in our analysis, by providing a reference point from which we can estimate the effect of the accessibility of cross-border shopping for stores closer to the border.[20] The key identifying assumption that enables a causal interpretation of our results is that the underlying trend in grocery sales is not dependent on the distance to Sweden. In Section 6, we provide evidence in support of this assumption.

It seems unlikely that all stores within 180 minutes from a Swedish store are equally affected by cross-border shopping. We therefore interact the binary variable B_{st} with a set of indicator variables D^j_{st}, which capture the driving duration from store s to the closest Swedish store that is accessible in period t. We measure duration in 30-minute bins, indexed by j, between 0 and 180, with a final category capturing cases where the closest available store is more than 180 minutes away or where there are no Swedish stores available because

[20]One could of course assume that stores more than 180 minutes from the closest Swedish grocery store are also affected by the availability of cross-border shopping. In Section 6, we re-estimate our main model using only stores located at least 300 minutes from Sweden as the control group.

Table 4. Effect of cross-border shopping on grocery store activity: main specification

	Sales (1)	Store visits (2)	Basket size (3)
Duration $< 30 \times B$	−0.294***	−0.174***	−0.121***
	(0.017)	(0.015)	(0.009)
$30 \leq$ Duration $< 60 \times B$	−0.207***	−0.135***	−0.073***
	(0.016)	(0.012)	(0.011)
$60 \leq$ Duration $< 90 \times B$	−0.081***	−0.050*	−0.030***
	(0.023)	(0.020)	(0.007)
$90 \leq$ Duration $< 120 \times B$	−0.030	−0.009	−0.021***
	(0.015)	(0.014)	(0.006)
$120 \leq$ Duration $< 150 \times B$	0.004	0.016	−0.011*
	(0.014)	(0.013)	(0.005)
$150 \leq$ Duration $< 180 \times B$	−0.007	0.005	−0.013
	(0.020)	(0.019)	(0.007)
Observations	42,000	42,000	42,000
Stores	400	400	400

Notes: This table reports results from estimation of equation (1). In Column 1, the dependent variable is the natural logarithm of weekly sales. In Column 2, the dependent variable is the natural logarithm of the number of weekly store visits. In Column 3, the dependent variable is the natural logarithm of the average weekly basket size, defined as weekly sales divided by weekly store visits. The standard errors reported in parentheses are clustered at the municipality level. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

all border counties are closed for cross-border shopping. The equation we estimate is

$$Y_{st} = \gamma_s + \lambda_t + \sum_j B_{st} D_{st}^j \delta_j + \epsilon_{st}. \quad (1)$$

In equation (1), Y_{st} is the outcome of interest (e.g., store-level sales). The variables of interest are δ_j, which estimate the effect of cross-border shopping on sales for the different duration categories. γ_s and λ_t are store and time fixed effects, respectively. The treatment has a geographical dimension and we cluster standard errors on the municipal level.

Table 4 reports the results for three different store-level outcome variables: the natural logarithm of total weekly sales, the natural logarithm of the number of weekly customers, and the natural logarithm of the average basket size.

From Column 1 of Table 4, we see that the availability of cross-border shopping is estimated to lead to a 25 percent $((\exp(-0.294) - 1) * 100)$ reduction of grocery store sales for stores located within 30 minutes of the closest Swedish store.[21] As one would expect, the size of the effect is reduced

[21] Because the effects we estimate often are large in magnitude, the coefficients in our log-level regression models can be poor approximations to the percentage effect of the dependent variable

as we move further away from the border, eventually becoming insignificant for stores located between 90 and 120 minutes from the closest Swedish store.

Inspecting the relative contributions of store visits and basket size, we see that the contribution of fewer store visits is large close to the border but diminishes as we move away from the border. Conversely, the relative contribution of basket size is larger further away from the border, and the effect is statistically significant at the 1 percent level even at travel durations between 90 and 120 minutes. A possible explanation for this pattern could be that households living close to the border can cross the border regularly and cover much of their daily grocery needs in Sweden. These households can therefore, to a large extent, replace store visits in Norway with cross-border shopping, whenever the border is open. Households located further away from the border, however, will find it more difficult to substitute store visits in Norway with store visits in Sweden. At these distances, the average basket size bought in Norway could still be sensitive to cross-border shopping, as even infrequent cross-border shopping could cover the needs in categories well suited for cross-border shopping, thus reducing the average basket size in the Norwegian stores.[22]

This explanation can be investigated by estimating the effect of cross-border shopping on the sales in categories that are more or less suitable for (long-distance) cross-border shopping. In Table 5, we consider the effect of cross-border shopping in four product categories that are difficult to transport over distance. We see that the estimated effects are relatively large close to the border, but that, as expected, the effects taper off quickly and are statistically insignificant for travel durations above 60 minutes.

In Table 6, we consider seven product categories that should be more suitable for cross-border shopping also at relatively large distances.[23] In all seven categories, we see large effects that are also apparent relatively far from the border. For example, cross-border shopping is estimated to reduce the sales of soda by about 7 percent as far away as 90–120 minutes from the closest Swedish store. We find the largest effects for beer and tobacco products, which is not surprising given that prices are substantially

on the outcome. Whenever we refer to a percentage effect in our discussions of the regression results, we therefore carry out the exact calculation in the same manner as we do here.

[22] It may be tempting to think of this decomposition as reflecting the extensive margin (shop in Sweden versus shop in Norway) and the intensive margin (how much to purchase). Note, however, that they do not rely on traveling during COVID-19 but on how patterns change when households that could travel before now cannot. Our decomposition of sales into store visits and basket size is similar to the decomposition of sales growth in US grocery stores performed by Einav et al. (2021).

[23] We consider cheese, meat, sweets, and soda (the same four categories as in Friberg et al. (2022)), in addition to beer, cigarettes, and snus.

Table 5. Effect of cross-border shopping on sales in categories not suitable for cross-border shopping

	Ice cream (1)	Milk (2)	Freshly baked (3)	Ready-made (4)
Duration $< 30 \times B$	-0.123^{***}	-0.220^{***}	-0.151^{***}	-0.142^{***}
	(0.021)	(0.022)	(0.026)	(0.027)
$30 \leq$ Duration $< 60 \times B$	-0.095^{***}	-0.113^{***}	-0.101^{***}	-0.114^{***}
	(0.014)	(0.016)	(0.019)	(0.016)
$60 \leq$ Duration $< 90 \times B$	−0.036	−0.029	−0.042	−0.007
	(0.025)	(0.022)	(0.025)	(0.025)
$90 \leq$ Duration $< 120 \times B$	0.019	−0.003	−0.018	0.021
	(0.017)	(0.015)	(0.015)	(0.015)
$120 \leq$ Duration $< 150 \times B$	0.035	0.029	0.005	0.033^{*}
	(0.019)	(0.016)	(0.018)	(0.017)
$150 \leq$ Duration $< 180 \times B$	−0.024	0.014	0.006	0.000
	(0.024)	(0.020)	(0.020)	(0.022)
Observations	41,995	41,993	41,994	41,993
Stores	400	400	400	400

Notes: This table reports results from estimation of equation (1). Each column shows the result of an estimation where the dependent variable is the weekly sales at store level (in NOK) of the given product category. The standard errors reported in parentheses are clustered at the municipality level. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

higher for these categories in Norway than in Sweden, and given that both beer and tobacco are easy to store and transport. Closest to the border, cigarette sales are reduced by about 67 percent, while beer sales are reduced by about 40 percent, due to the cross-border shopping. We also see that the effects are substantial even more than 120 minutes from the closest Swedish store.

5. The effect of cross-border shopping on sales and tax revenue

5.1. Tax revenue and cross-border shopping: an overview of approaches

To frame the exercise that we perform in the following, and to be able to compare with other possible approaches, let us present a stylized view of how demand, prices, and tax revenue are related to cross-border shopping. We can then express the local demand in Norway for good g by household h at location l as depending on the price of the good in Norway (p_N), the price in Sweden translated into NOK at the current exchange rate ($e \times p_S$), and the cost of cross-border shopping (cbc) that varies by distance to Sweden

Table 6. Effect of cross-border shopping on sales in categories well suited for cross-border shopping

	Cheese (1)	Meat (2)	Sweets (3)	Soda (4)	Beer (5)	Cigarettes (6)	Snus (7)
Duration < 30 ×*B*	−0.281***	−0.519***	−0.349***	−0.660***	−0.510***	−1.102***	−1.096***
	(0.031)	(0.021)	(0.020)	(0.036)	(0.046)	(0.109)	(0.054)
30 ≤ Duration < 60 ×*B*	−0.178***	−0.324***	−0.212***	−0.375***	−0.310***	−0.681***	−0.769***
	(0.031)	(0.023)	(0.021)	(0.035)	(0.041)	(0.045)	(0.052)
60 ≤ Duration < 90 ×*B*	−0.074**	−0.103***	−0.084**	−0.150***	−0.144***	−0.310***	−0.304***
	(0.024)	(0.027)	(0.028)	(0.025)	(0.026)	(0.038)	(0.042)
90 ≤ Duration < 120 ×*B*	−0.038	−0.036	−0.029	−0.076***	−0.091***	−0.141***	−0.138***
	(0.020)	(0.022)	(0.021)	(0.022)	(0.026)	(0.025)	(0.027)
120 ≤ Duration < 150 ×*B*	0.004	−0.017	0.006	−0.048*	−0.053**	−0.070**	−0.086***
	(0.019)	(0.020)	(0.017)	(0.019)	(0.019)	(0.022)	(0.025)
150 ≤ Duration < 180 ×*B*	−0.014	−0.033	0.003	−0.042	−0.031	−0.045*	−0.048
	(0.023)	(0.022)	(0.025)	(0.025)	(0.026)	(0.020)	(0.030)
Observations	41,994	41,993	41,998	41,997	41,996	41,997	41,927
Stores	400	400	400	400	400	400	400

Notes: This table reports results from estimation of equation (1). Each column shows the result of an estimation where the dependent variable is the weekly sales at store level (in NOK) of the given product category. The standard errors reported in parentheses are clustered at the municipality level. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

and by household characteristics.[24] The latter includes both direct outlays on transport and opportunity costs, and such costs of cross-border shopping imply that the law of one price need not hold.[25] Thus, formally,

$$q_{ghl} = f(p_N, e \times p_S, cbc_{hl}). \tag{2}$$

In any standard model of pricing in imperfectly competitive markets, price can be expressed as a markup (that depends on the elasticity of demand, ϵ) times marginal costs (mc_N) whilst recognizing that excise tax (t_N) and VAT will also affect price. We can thus express price as

$$p_N = (1 + VAT_N)\left(\frac{\epsilon}{\epsilon + 1}\right)(t_N + mc_N). \tag{3}$$

The outlined framework can be used to describe several questions of potential interest. One set of questions regards how local demand depends on marginal changes in the relative prices. This is essentially the exercise that is performed in Friberg et al. (2022) where we rely on the shifts in relative price for some product categories (cheese, meat, sweets, soda) generated by exchange rate changes.

A second set of questions pertains to how markups respond to the costs of cross-border shopping. If prices were set at the store or regional level, one would have expected that the non-availability of a set of competitors would lead to higher markups. As discussed, however, all the Norwegian grocery retail chains keep the same prices across all of Norway, which implies that such incentives are balanced by areas far from the border. The COVID-19 pandemic clearly affected demand and costs in many ways, and we do not attempt to tease out the relative contribution of costs and markups to any price changes that we observe.

A third set of possible questions concerns how changes in VAT and excise taxes would affect local demand and the extent of cross-border shopping. Such an analysis hinges on good estimates of the extent to which tax changes are passed through into retail prices; for example, see Weyl and Fabinger (2013) for theoretical considerations, and for empirical studies, see Ardalan and Kessing (2021) for European beer markets, DeCicca et al. (2022) for tobacco, and Allcott et al. (2019a) for sugar taxes. Whether such effects are desirable or not is not only a matter of tax consequences, but the motivation for excise taxes is also important. A lower excise tax will not only decrease

[24] To simplify this discussion, we disregard that the decision to shop across the border will typically depend on several prices. For instance, a user of snus may shop for cheese in Sweden even if the saving on cheese in itself would not have motivated a cross-border trip.

[25] See, for example, Gopinath et al. (2011) for an influential study of reasons for price differences in grocery retailing along the Canada–US border.

cross-border shopping and have consequences for tax revenue, it will also raise domestic consumption with complex welfare effects that interact with consumer heterogeneity; for example, see Griffith et al. (2019) for an analysis of heterogeneity and alcohol taxation, and Allcott et al. (2019a) for taxes on sugar-sweetened beverages.

In principle, the above kinds of questions could be addressed with the dataset on hand. Note, on the one hand, that there are no important changes in VAT or excise taxes during the period of study that could be used to identify the effects of such changes. On the other hand, the dataset is uniquely suited to examine the overall effect of border closings – not studying the effect of marginal changes but studying the overall effect of cross-border shopping being unavailable on commodity tax revenue, an estimation that we turn to in the following. It deserves to be emphasized, however, that many important questions intersect. For instance, the effect of closing cross-border shopping depends on the extent of price differences when borders are open. In addition, it can be pointed out that a number of factors are kept constant in the experiment that may be endogenous in the longer run – for instance, the level of excise taxes and the locations of retail stores.[26]

5.2. Tax revenue and cross-border shopping: an estimation of the effect of closed borders

In this subsection, we seek to quantify the effect of cross-border shopping on store sales and tax revenue. The detailed data allow for an examination of cross-border shopping of sales at the category level as well as the effect on tax revenue from excise taxes and VAT. As discussed in Section 3, several of the product categories most frequently bought in Sweden by Norwegian customers are subject to excise taxes in Norway. In 2019, there were excise taxes for beer, cigarettes, snus, soda, and sweets. These excise taxes are non-trivial. For instance, cigarettes faced an excise tax of 2.63 NOK per cigarette in 2019, equivalent to around 0.25 euros using the average EUR/NOK exchange rate for 2019 (the average consumer price was 6.17 NOK or about 0.63 euros per cigarette).[27] Of particular interest may be the "sugar tax". In 2019, this excise tax for chocolates and sweets amounted to 20.82 NOK per kilogram (representing 8.7 percent of the average consumer price); for beverages, the

[26] In the long run, we expect the absence of cross-border shopping to lead to the establishment of new stores in Norwegian border areas, an effect documented in response to large swings in exchange rates in Canada by Campbell and Lapham (2004).

[27] The average exchange rate was 9.8527. The exchange rates are available through Norges Bank (2022).

Table 7. Effect of cross-border shopping on VAT and category volumes

	VAT (1)	Sweets (2)	Soda (3)	Beer (4)	Cigarettes (5)	Snus (6)
Duration $< 30 \times B$	-0.314^{***}	-0.373^{***}	-0.789^{***}	-0.521^{***}	-1.099^{***}	-1.100^{***}
	(0.016)	(0.022)	(0.047)	(0.048)	(0.108)	(0.055)
$30 \leq$ Duration $< 60 \times B$	-0.226^{***}	-0.217^{***}	-0.433^{***}	-0.322^{***}	-0.679^{***}	-0.780^{***}
	(0.016)	(0.022)	(0.049)	(0.044)	(0.046)	(0.054)
$60 \leq$ Duration $< 90 \times B$	-0.089^{***}	-0.090^{**}	-0.174^{***}	-0.153^{***}	-0.308^{***}	-0.313^{***}
	(0.023)	(0.028)	(0.024)	(0.027)	(0.038)	(0.042)
$90 \leq$ Duration $< 120 \times B$	-0.034^{*}	-0.034	-0.086^{***}	-0.103^{***}	-0.139^{***}	-0.146^{***}
	(0.015)	(0.021)	(0.021)	(0.028)	(0.025)	(0.028)
$120 \leq$ Duration $< 150 \times B$	0.001	0.008	-0.063^{**}	-0.062^{**}	-0.066^{**}	-0.091^{***}
	(0.014)	(0.018)	(0.021)	(0.021)	(0.022)	(0.025)
$150 \leq$ Duration $< 180 \times B$	-0.008	0.008	-0.052^{*}	-0.046	-0.045^{*}	-0.054
	(0.020)	(0.026)	(0.023)	(0.026)	(0.020)	(0.028)
Observations	41,995	41,998	41,997	41,996	41,997	41,927
Stores	400	400	400	400	400	400

Notes: This table reports results from estimation of equation (1). Columns 2–6 show the result of an estimation where the dependent variable is the weekly sales (in volume) at store level of the given product category. The standard errors reported in parentheses are clustered at the municipality level. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

tax was 4.82 NOK per liter (representing 21.4 percent of the average consumer price).[28]

Because the excise taxes were calculated on the basis of units sold, we first estimate the effect of cross-border shopping on the volume sold in the different categories, and then use these estimates to calculate the estimated loss in excise taxes resulting from cross-border shopping. In addition to category specific excise taxes, all products sold in grocery stores are subject to VAT. Because our sales data include information about VAT, we can estimate the effect of cross-border shopping on VAT revenue directly.[29]

In Table 7, we report the estimated effect of cross-border shopping on VAT and on the volume sold in the categories subject to excise taxes. The models are defined by equation (1), and the outcome variables are log-transformed.

As expected, the effect of cross-border shopping on VAT is similar to the effect of cross-border shopping on store sales (as reported in Column 1

[28] The "sugar tax" has been the subject of considerable discussion in Norway. It was subject to a sharp and unexpected increase in 2018, followed by a partial lowering in 2019. In 2021, the tax was abolished, arguably partly in response to concerns about cross-border shopping as borders reopened. See Table IA.2 in the Online Appendix for average consumer prices (per unit) and excise tax rates.

[29] There are two relevant VAT rates for Norwegian grocery stores, 15 percent for food items and 25 percent for non-food items; our dataset provides exact values for VAT for each product.

Table 8. Estimated effect of border trade on stores sales and VAT

	<30	[30, 60)	[60, 90)	[90, 120)	[120, 150)	[150, 180)	>180	Total
Store distribution								
Number of stores	62	188	371	762	279	176	1,985	3,823
Proportion of stores	0.02	0.05	0.10	0.20	0.07	0.05	0.52	1.00
Sales (million NOK)								
Counterfactual	4,203	11,091	29,265	65,292	16,904	10,031	86,381	223,166
Actual	3,132	9,015	26,999	63,369	16,974	9,959	8,6381	215,828
Loss	1,071.6	2,076.2	2,266.2	1,922.7	−70.4	71.5	0.0	7,337.8
VAT (million NOK)								
Counterfactual	602	1,602	4,185	9,283	2,426	1,452	12,531	32,081
Actual	440	1,278	3,828	8,971	2,428	1,440	12,531	30,916
Loss	162.3	324.0	356.6	311.5	−2.3	12.1	0.0	1,164.3

Notes: The duration groups are defined by driving duration to the closest Swedish store when all Swedish counties were open for cross-border shopping.

of Table 4), and the effect of cross-border shopping on the category sales by volume is similar to the effect on category sales in NOK (as reported in Table 6), although the estimated effect on soda volume is slightly higher than the estimated effect on sales in NOK. This suggests that lower-priced sodas are more affected by cross-border shopping than premium products, consistent with the notion that it is the more price-sensitive consumers that stock up sufficient volumes of soda from cross-border trips to affect local sales.[30]

Table 8 contains our estimates of the effect of cross-border shopping on total sales and VAT, while the estimated effect on category-level sales and excise tax is reported in Table 9. To obtain the reported estimates, we proceed as follows. For each duration category, we calculate the mean value of total store sales, category sales volumes, and VAT in our estimation sample in 2019. We then use the estimated effects reported in Table 4 (for store-level sales) and Table 7 (for category volumes and VAT) to calculate the counterfactual closed-border outcome the last full normal year before COVID-19 closures. To get an estimate of the economic impact of cross-border shopping, we then multiply these estimated effects by the total number of grocery stores in each duration category (i.e., not only the stores in our estimation sample).[31]

[30] This is in some contrast to Wang (2015) who, using US household-level data for 2002–2004, finds that low-income households are less likely to stockpile soda.

[31] An underlying assumption of the results reported in Tables 8 and 9 is that, within each duration category, the stores in our estimation sample are representative of the grocery stores. Because the umbrella chain to which our sample stores belong has stores in all market segments, we believe that this is a reasonable assumption. Furthermore, in Figure IA.1 in the Online Appendix, we compare the yearly sales amounts of the estimation stores and the population of stores (within each duration category). We find that the distributions are very similar.

Table 9. Estimated effect of border trade on sales and tax income

	<30	[30, 60)	[60, 90)	[90, 120)	[120, 150)	[150, 180)	>180	Total
Sweets								
Sales (metric tonnes)								
Counterfactual	783	1,869	4,987	11,221	3,098	1,963	16,913	40,834
Actual	539	1,504	4,557	10,846	3,122	1,978	16,913	39,459
Loss	244	365	429	375	−24	−15	0	1,375
Tax (millions NOK)								
Counterfactual	11.0	26.2	69.9	157.2	43.4	27.5	237.0	572.2
Actual	7.6	21.1	63.9	152.0	43.8	27.7	237.0	553.0
Loss	3.4	5.1	6.0	5.3	−0.3	−0.2	0.0	19.3
Soda								
Sales (thousand liters)								
Counterfactual	10,184	22,910	55,269	115,302	37,028	22,354	174,023	437,069
Actual	4,626	14,862	46,449	105,844	34,777	21,212	174,023	401,792
Loss	5,558	8,048	8,820	9,458	2,252	1,142	0	35,277
Tax (million NOK)								
Counterfactual	36.1	81.3	196.0	408.9	131.3	79.3	617.2	1,550.1
Actual	16.4	52.7	164.7	375.4	123.3	75.2	617.2	1,425.0
Loss	19.7	28.5	31.3	33.5	8.0	4.0	0.0	125.1
Beer								
Sales (thousand liters)								
Counterfactual	4,245	11,399	27,561	58,211	16,131	10,205	89,595	217,347
Actual	2,522	8,265	23,651	52,489	15,168	9,748	89,595	201,438
Sales loss (thousand liters)	1,723	3,135	3,910	5,721	963	458	0	15,909

Table 9. Continued

	<30	[30, 60)	[60, 90)	[90, 120)	[120, 150)	[150, 180)	>180	Total
Tax (million NOK)								
Counterfactual	94.1	252.7	610.9	1,290.3	357.6	226.2	1,986.0	4,817.9
Actual	55.9	183.2	524.3	1,163.5	336.2	216.1	1,986.0	4,465.2
Loss	38.2	69.5	86.7	126.8	21.3	10.1	0.0	352.7
Cigarettes								
Sales (thousand items)								
Counterfactual	28,046	83,625	172,488	307,781	104,075	66,194	547,895	1,310,104
Actual	9,341	42,405	126,734	267,821	97,387	63,308	547,895	1,154,891
Loss	18,705	41,220	45,754	39,961	6,688	2,885	0	155,213
Tax (million NOK)								
Counterfactual	75.5	225.2	464.6	829.0	280.3	178.3	1,475.7	3,528.5
Actual	25.2	114.2	341.3	721.3	262.3	170.5	1,475.7	3,110.5
Loss	50.4	111.0	123.2	107.6	18.0	7.8	0.0	418.0
Snus								
Sales (metric tonnes)								
Counterfactual	15.76	57.64	135.59	298.57	83.08	53.22	518.77	1,162.63
Actual	5.25	26.42	99.19	258.08	75.85	50.44	518.77	1,033.99
Loss	10.51	31.22	36.40	40.49	7.23	2.79	0.00	128.65
Tax (million NOK)								
Counterfactual	15.8	57.8	136.0	299.6	83.4	53.4	520.5	1,166.5
Actual	5.3	26.5	99.5	258.9	76.1	50.6	520.5	1,037.4
Loss	10.5	31.3	36.5	40.6	7.3	2.8	0.0	129.1

Notes: The duration groups are defined by driving duration to the closest Swedish store when all Swedish counties were open for cross-border shopping.

For instance, stores in the estimation sample within a 30-minute drive from the closest Swedish border sold on average 8.69 metric tonnes of sweets in 2019 (see Table IA.3 in the Online Appendix). For these stores, the estimated effect of cross-border shopping on the log of sweet sales (in metric tonnes) is −0.373 (see Column 2 of Table 7). The counterfactual mean level of sales is therefore given by $8.69/\exp(-0.373) = 12.62$, which gives an estimated mean loss due to cross-border shopping equal to $12.62 - 8.69 = 3.93$. Multiplying the actual and counterfactual mean sales volume and the estimated mean sales volume loss by the total number of grocery stores in this duration category (62) gives (subject to rounding) numbers reported in Column 1 of Table 9.

We see that the economic significance of cross-border shopping is substantial. We commence with the effect on store sales and VAT as reported in Table 8. As expected, the effects are strongest close to the border and then gradually diminish with travel duration to the closest Swedish stores. Sales are reduced by 25.5 percent (1071.6/4203) in stores that are within 30 minutes' driving distance from a Swedish store due to cross-border shopping, and VAT revenue from stores in these locations is reduced by 27 percent (162.3/602). For locations between 30 and 60 minutes away, the corresponding VAT loss is 20.2 percent, and for locations between 60 and 90 minutes from Sweden the corresponding VAT loss is 8.5 percent. These are clearly substantial numbers and, as discussed in the introduction, numbers of this magnitude serve to motivate minimum levels of VAT within the European single market. Even in a large and mountainous country such as Norway, the VAT loss is non-trivial, also at the national level, with an estimated VAT loss of 3.6 percent due to cross-border shopping.

We now turn to an estimation of the loss of sales and excise taxes for products sold in grocery stores due to cross-border shopping. Table 9 reports the estimated effects on volumes sold and on commodity tax revenue for different duration intervals from the border.[32]

Again, effects are strongest close to the border and then gradually diminish. In several cases, the estimated foregone tax revenue is very large for stores within a 30-minute drive from the closest Swedish store, and for cigarettes, soda, and snus, tax revenue more than doubles in these border areas in a situation with no cross-border shopping. However, only 2 percent of the grocery stores in Norway are located this close to Sweden, whereas more

[32]In the analysis above, we also examined the effect of cross-border shopping on sales of meat and cheese that are not subject to excise taxes but to import tariffs. The motivation for such tariffs is to protect domestic production rather than a combination of externalities and internalities directly linked to consumption. Evaluating the impact of those tariffs could be done in similar ways but would require keeping track of the origin of different products, and is arguably of less interest outside Norway than the examination of how cross-border shopping interacts with excise taxes at different distances (travel duration) from the border.

than a third of grocery stores are located within 120 minutes from the closest Swedish store, and we see substantial effects also at this distance. The effects are largest for beer, soda, and tobacco products. For areas located between 90 and 120 minutes from the closest Swedish store, the estimated loss of tax revenue in these categories is close to, or above, 10 percent of the actual tax revenue. Aggregating up to the national level, effects are still substantial with a loss of excise taxes on, for instance, soda of 8.1 percent and on cigarettes of 11.9 percent. Total estimated loss of tax revenue (VAT and excise taxes) from cross-border shopping at the national level is about 2.3 billion NOK.

Still, whether numbers are seen as large or small at the national level is partly in the eye of the beholder. The impact on some regions and for some product groups is very large but in terms of the overall direct impact of cross-border shopping on tax revenue the consequences for Norway are limited, as our estimate of 2.3 billion NOK corresponds to approximately 0.2 percent of taxes and levies from non-oil-related income.[33]

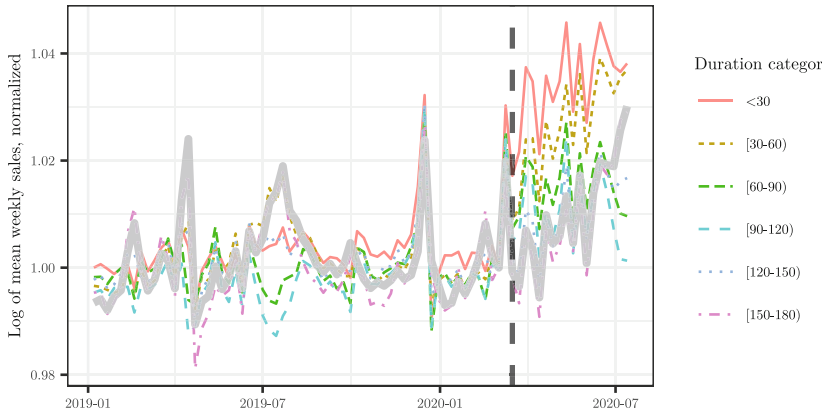
6. Discussion of the identification assumption

The key identification assumption that we make is that the underlying trend in grocery store activity does not vary with the travel duration to Sweden. In this section, we provide evidence in support of this underlying assumption, and show that the results found in Section 4 are robust to a number of alternative specifications.

Figure 4 plots the natural logarithm of average weekly sales for stores in the different pre-COVID duration categories. The time series are normalized by dividing by the values in the week before the Norwegian lockdown was announced (i.e., the week starting on 2 March 2020). The thick gray line represents the stores in the control group, that is, stores located further than 180 minutes from the closest accessible Swedish store before the travel restrictions were implemented. We plot the average weekly sales in the period when all Swedish counties were open to border trade (from 7 January 2019 until 17 March 2020) and in the period when all Swedish counties were closed to cross-border trade (that is, between 17 March and 25 July 2020). While there is some variation in the levels between the groups, they follow each other quite closely over time, and there is no indication that the trends are different. We also see a clear increase in sales after travels to Sweden became restricted for the duration groups closest to the border.

[33] According to the 2019 government budget (Finansdepartmentet, 2019, p. 52), revenue from taxes and levies excluding oil-related revenue amounted to 1031.3 billion NOK. As is well known, Norway is a major oil exporter, and taxes and levies from the oil sector contributed a further 312.8 billion NOK.

Figure 4. Pre-trends in store sales



Notes: The figure shows the natural logarithm of the average weekly sales at the store level (in NOK), for stores in different duration groups. The plotted values have been normalized to the last week before the lockdown was announced (the week starting on 2 March 2020). The first week included in the figure is the week starting on Monday 7 January 2019. The last week included in the figure is the week starting on Monday 13 July 2020. The dashed vertical line indicates the week travels to Sweden became restricted. The thick gray line represents stores that were located at least 180 minutes from the closest Swedish store.

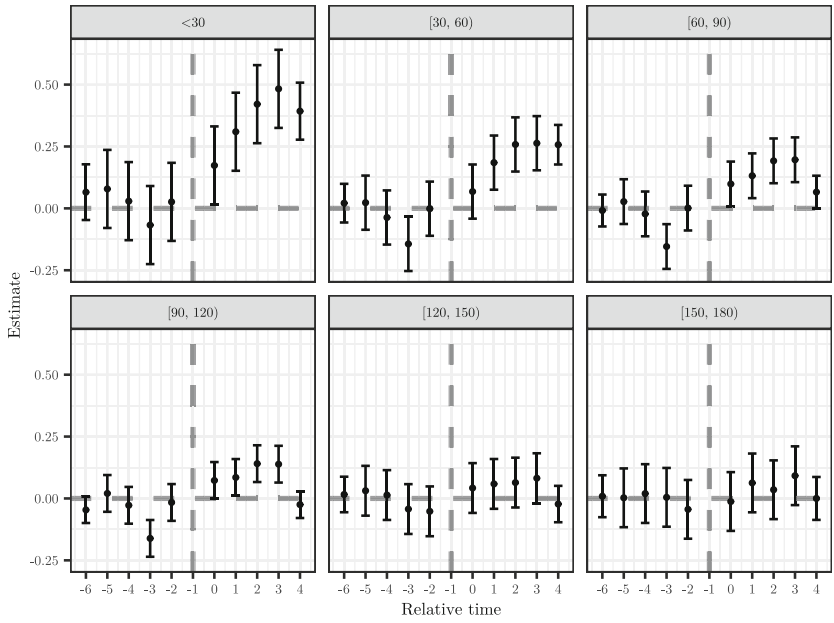
As a more formal test for common trends, we have estimated the following event study model, where $\tilde{D}_s^j$ indicates the pre-COVID duration category of store s and where we let k be the period the lockdown was announced:

$$\begin{aligned} Y_{st} = \gamma_s + \lambda_t + \sum_j 1(t \leq k-6)\tilde{D}_s^j \delta_{-6,j} \\ + \sum_j \sum_{\tau=-5}^{-2} 1(t = k+\tau)\tilde{D}_s^j \delta_{\tau,j} + \sum_j \sum_{\tau=0}^{3} 1(t = k+\tau)\tilde{D}_s^j \delta_{\tau,j} \\ + \sum_j 1(t \geq k+4)\tilde{D}_s^j \delta_{4,j} + \epsilon_{st}. \end{aligned} \tag{4}$$

The parameters of interest are the treatment effects $\delta_{\tau,j}$, which vary according to both pre-COVID duration group and time. We again restrict attention to the period up to 25 July 2020. We normalize the effects relative to the week prior to the treatment ($\tau = -1$), and bin the periods at least six weeks before the treatment and the periods at least four weeks after. In Figure 5, we plot for each duration category the estimated treatment effects.

From Figure 5, we see that the leads are generally close to zero and not statistically significant. An exception is the coefficient for $\tau = -3$, which is negative in several of the duration bins. This can be explained by the fact that

Figure 5. Event study plot



Notes: The figure shows the estimated treatment effects and 95 percent confidence intervals from estimation of equation (4). The x-axis shows the time relative to when the lockdown was imposed (i.e., the week starting on Monday 9 March 2020).

this week was a school holiday in parts of Norway. The effect of the border closing is clearly visible through the large positive lags for the duration groups closest to Sweden.

We can also investigate the underlying common trends assumption by adding separate linear time trends for each of the pre-COVID duration categories. If this produces significantly different results from the results reported in Table 4, it could reflect diverging underlying trends in sales in different regions rather than the effect of cross-border shopping (Angrist and Pischke, 2008, p. 238). Reassuringly, we see that the results reported in Column 1 of Table 10 are very similar to the results in the main specification.

Even though both visual and statistical tests provide support for the common trends assumption, one could still imagine that different regions experienced different shocks that coincided with the closing of the border. Any shocks unrelated to the closing of the border that affected stores differently depending on their distance from the border would bias our results. However, travel restrictions affected stores within pre-COVID duration groups differently at different times, because different Swedish counties were open to cross-border shopping at different times during 2020. This variation

Table 10. Effect of border shopping on store sales: robustness checks

	(1)	(2)	(3)	(4)	(5)
Duration < 30 × B	−0.309***	−0.328***	−0.309***	−0.301***	−0.311***
	(0.020)	(0.074)	(0.017)	(0.016)	(0.018)
30 ≤ Duration < 60 × B	−0.225***	−0.280***	−0.221***	−0.206***	−0.229***
	(0.027)	(0.060)	(0.016)	(0.016)	(0.017)
60 ≤ Duration < 90 × B	−0.101***	−0.199**	−0.090***	−0.077**	−0.109***
	(0.022)	(0.061)	(0.023)	(0.027)	(0.022)
90 ≤ Duration < 120 × B	−0.033	−0.002	−0.042**	−0.034*	−0.059***
	(0.019)	(0.026)	(0.016)	(0.017)	(0.016)
120 ≤ Duration < 150 × B	−0.011	0.022	−0.006	0.010	−0.005
	(0.018)	(0.024)	(0.014)	(0.016)	(0.015)
150 ≤ Duration < 180 × B	−0.018	0.002	−0.017	−0.006	−0.003
	(0.024)	(0.022)	(0.021)	(0.024)	(0.020)
New COVID-19 cases					0.014**
					(0.005)
C × Holiday home proportion					0.135***
					(0.040)
Unemployment rate					−1.854***
					(0.458)
Population					0.499***
					(0.146)
Observations	42,000	42,000	35,490	37,600	41,016
Stores	400	400	338	400	400
Control group	>180 min	>180 min	>300 min	>180 min	>180 min
Linear time trends	Yes	No	No	No	No
Duration group × week FEs	No	Yes	No	No	No
Time window	Full	Full	Full	Restricted	Full

Notes: This table reports results from estimation of equation (1) with the natural logarithm of sales as the dependent variable. In Column 1, separate linear time trends for duration groups based on the pre-COVID duration to the closest Swedish store are included. The frequency distribution of the groups is reported in Table 2. In Column 2, week × pre-COVID duration group fixed effects are included. In Column 3, we drop observations from stores that, before the COVID-19 restrictions, were located between 180 and 300 minutes from the closest Swedish store. In Column 4, we drop observations from the weeks between and including the week starting on Monday 27 July 2020 and the week starting on Monday 5 October 2020. In these weeks, some, but not all, Swedish border counties were open for cross-border shopping. The variable "New COVID-19 cases" included in Column 5 is the sum of the registered COVID-19 cases in week t and $t-1$ divided by the population of the municipality. C is an indicator variable taking the value one in the weeks after 17 March 2020 (and zero in the weeks before). "Holiday home proportion" is the proportion of residential dwellings that are holiday homes. The unemployment rate is measured at the municipality–month level. Population is measured at the municipality–quarter level. The standard errors reported in parentheses are clustered at the municipality level. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

allows us to include week × pre-COVID duration group fixed effects in the model. The estimated effects of cross-border shopping now no longer rely on stores from different pre-COVID duration groups having identical underlying developments in sales, but rather that stores in the same pre-COVID duration groups share a common underlying trend. The results from this specification are reported in Column 2 of Table 10. Compared to the main specification, the estimated effects are somewhat larger in magnitude up to until the 60–90 minute duration category, but the general impression is very similar. We also see that the standard errors are slightly larger, which is not surprising given that we are now using only within pre-COVID duration group variation in the accessibility of cross-border shopping to identify the effects.

In our analysis so far, we have considered the effect of cross-border shopping on stores located less than 180 minutes from the closest Swedish store. All stores located 180 minutes or more away have been assumed to be unaffected by the availability of cross-border shopping in Sweden, effectively functioning as a control group. If sales in some of the stores in the control group are in fact negatively affected by the availability of cross-border shopping, the results reported in Table 4 would be misleading, with the estimated coefficients biased towards zero. As a robustness check on our results, we therefore estimate our model using only observations where the pre-COVID driving duration to the closest Swedish store is below 180 minutes or above 300 minutes. The control group now consists of stores that are at least 300 minutes away from the closest Swedish store, a distance that should reduce the relevance of cross-border shopping to a minimum. The results are reported in Column 3 of Table 10. The results are very similar to the ones in the main specification.

The results in our main specification are based on an assumption that the effect on grocery store sales in Norway of having an accessible Swedish store a certain distance away is the same before and during the pandemic. That is, we assume that having a Swedish store a 45-minute drive away is the same in September 2019 as in September 2020. However, there could be reasons to believe that this is not the case. For instance, infection rates were typically higher in Sweden than in Norway even in periods where some Swedish counties were open for cross-border shopping, something that could deter Norwegians from cross-border shopping even in periods where Swedish border counties were open. To see whether this implicit assumption affects our results, we re-estimate our model excluding data from the period between the week starting on Monday 27 July 2020, and the week starting on Monday 5 October 2020, a period during which some but not all Swedish border counties were open. We are then left with a pre-period before 17 March 2020 where cross-border shopping was possible and the COVID-19 infection rates were non-existent or very low in both counties, and a post-period where cross-border shopping was not possible. The results are reported in Column 4

of Table 10. The point estimates are more or less unchanged from the main specification.

The results in Column 4 are also useful for another reason: several recent methodological papers have shown that the two-way fixed effects regressions we use in this paper can give biased results if treatment effects are heterogeneous across time or groups, and there is variation in treatment timing (see, e.g., De Chaisemartin and d'Haultfoeuille, 2020; Borusyak et al., 2021; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021).[34] The results reported in Column 4 of Table 10 are based on a restricted time period where there is no variation in treatment timing. This means that we avoid the forbidden comparison of units going into treatment with already treated units (Borusyak et al., 2021). The fact that the results in this specification are similar to the results in the main specification indicates that they are not biased by heterogeneous treatment effects.[35]

In our view, the main analysis and robustness checks performed so far provide clear evidence that Norwegian stores close to the border experienced increases in store activity during the COVID-19 pandemic. Our hypothesis is that this increase in store activity was the result of cross-border shopping effectively becoming impossible. An alternative explanation could be that the grocery trade in some or all border regions was affected differently by the COVID-19 pandemic than grocery trade in the rest of Norway. To provide some additional support for our explanation, we estimate a specification where we include several control variables that may both affect grocery demand, and be affected by the pandemic. First, we include a measure of COVID-19 infection rates at the municipality level, specifically, new infections per 1,000 inhabitants during the last two weeks.[36] The COVID-19 pandemic also

[34]Using the `twowayfeweights` STATA package of De Chaisemartin and d'Haultfoeuille (2020), none of the weights attached to our main regression were negative, satisfying the no sign reversal property.

[35]Several alternative estimators that are robust to heterogeneous treatment effects have been proposed, but most of these estimators do not apply to our natural experiment because treatment is switching. One exception is the estimator of Borusyak et al. (2021), which yields almost identical point estimates to our main specification. Note that one might argue that the specification in Column 4 of Table 10 should be the baseline specification as, in a simple way, it avoids all concerns about treatment heterogeneity across time. We opted for keeping the longer time period, as cross-border shopping can be partly seasonally dependent and we believe it valuable to cover large parts of the year – including both the full summer, autumn, and early winter. In the end, it does not matter much as the effects are virtually identical for the short and longer treatment periods. One explanation for the limited differences is that, as seen in Figure 2, there are relatively few post-COVID weeks when borders were open, also in summer. For instance, cross-border shopping with Västra Götaland, the region to the south of the capital Oslo, and home to several top cross-border shopping stores, is closed during the entire post-COVID period.

[36]That is, the sum of infected individuals in week t and $t-1$ divided by the population of the municipality.

affected the labor market and led to sharp increases in unemployment rates. We therefore include monthly unemployment rates at the municipality level as a control variable. The population dynamics in many Norwegian municipalities also changed during the pandemic. We therefore include population at the municipality–quarterly level as a control variable. Finally, local markets with a large number of holiday homes may have been affected differently by the pandemic than other local markets. First, in the period between 19 March 2020 and 20 April 2020, there was a ban in Norway on staying in holiday homes located outside one's home municipality. Secondly, after this first period, staying at holiday homes was permitted, while traveling abroad was severely restricted, which could lead to unusually high retail activity in municipalities with a high proportion of holiday homes. To account for these factors, we include an interaction between the proportion of holiday homes in the municipality and an indicator variable, C, which is one in the period after 12 March (when national restrictions were put in place) and zero in the period before 12 March. The results are reported in Column 5 of Table 10. Many of these variables affect grocery store sales, but the estimated effects of the interaction terms of interest are little changed compared to the main specification. We do note, however, that the estimated effect of cross-border shopping in the 90–120 minutes duration category is higher and highly statistically significant.

For yet additional robustness exercises, see the Online Appendix where we present pre-treatment plots and specification checks for the other dependent variables considered in Section 4: store visits and basket size from Table 4, sales in the categories from Tables 6 and 5, as well as VAT and category volumes from Table 7. The results reinforce the impression from this section that our results are very robust to alternative specifications and explanations.

Section IA.6 in the Online Appendix also compares our estimates of lost sales to the surveys of cross-border shopping produced by Norway's national statistical agency (Statistics Norway, 2022a), where a random sample of 2,000 Norwegians are surveyed about cross-border shopping in each quarter every year. The numbers match up remarkably well, which provides a validation for our results. Importantly, our results also provide a validation the other way around – a well-conducted survey with 2,000 respondents sampled nationally paints a picture that is remarkably well in line with observed patterns in this natural experiment. As such, the results should be encouraging for statistical agencies in both Norway and other countries that use surveys to track cross-border shopping.

7. Conclusion

Travel restrictions due to the COVID-19 pandemic made cross-border shopping in neighboring Sweden infeasible for Norwegians for most of

2020. In this paper, we exploit this natural experiment to estimate the effects of cross-border shopping on the grocery retail sector in Norway. The natural experiment is unique in the sense that it mimics the autarky regime often taken as a benchmark in theoretical work on cross-border shopping but rarely observed in practice. The natural experiment allows us to estimate the total effect of cross-border shopping directly, something that is difficult when – as is usually the case in the empirical literature on cross-border shopping – one has to rely only on changes in the relative prices.

Our estimates suggest that cross-border shopping reduces sales in the grocery stores closest to the border by more than a quarter compared to a counterfactual situation with closed borders. For typical cross-border shopping categories such as tobacco, more than half of the sales are lost. By not only estimating the effects of cross-border shopping in the regions closest to the border but also tracking how they gradually diminish as we move away from the border, we are able to quantify the effects at the national level. We estimate that cross-border shopping reduces total grocery sales in Norway by more than 3 percent, with a corresponding reduction in VAT income.

Let us now comment on the external validity of these results in two dimensions. One dimension concerns the extension to non-COVID periods. COVID-19 was a major shock that affected behavior in many areas. That said, our exploration of the identification assumption supports the notion that, apart from cross-border shopping, the pandemic affected the treated and control regions in remarkably similar ways. It is also worth noting that Norway's COVID-19 measures were much less restrictive than in most European countries; there was no hard lockdown, for instance. Thus, while necessarily somewhat speculative, we venture to believe that the results are also broadly applicable to more normal times.

Another dimension of external validity regards applicability to other countries. Here we note that border shopping is a significant phenomenon across several borders. Focusing only on Europe, for instance, Switzerland loses sales to several of its neighboring countries, as does Austria, and as do France and Spain for areas close to Andorra. Our results suggest that tax losses can be non-trivial, and clearly illustrates the challenges of independent tax policies when shopping in a neighboring jurisdiction is possible. Excise taxes on products, such as tobacco, alcohol, and soda, can be motivated by both fiscal and public health reasons. Our results demonstrate that a substantial part of the potential tax revenue can disappear through cross-border shopping. For tobacco and soda, we estimate that about 10 percent of the counterfactual tax income under closed borders is lost due to border shopping. In addition, our results indicate that Norwegians living in the border areas cover a large part of their consumption of products such as tobacco and alcohol through cross-border shopping, dampening the effectiveness of the excise taxes in reducing consumption.

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Supporting information

Additional supporting information can be found online in the supporting information section at the end of the article.

Online appendix
Replication package

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