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The Impact of COVID-19 on Economic Activity: Evidence from Administrative Tax Registers

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The Impact of Covid-19 on Economic Activity: Evidence from Administrative Tax Registers*

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Abstract

We use tax-register data on all firms in Sweden to document the impact of the Covid-19 pandemic on firm sales, tax payments, and sick pay. The pandemic impact is identified using within-year, between-year, and geographical variation, and we also run placebo tests. Our findings confirm large negative economic effects of the pandemic but shed new light on their magnitudes and sensitivity to Covid-19 morbidity rates. Specifically, we find that VAT and firm sales dropped more than other indicators of corporate activity such as industrial electricity usage or aggregate industrial production. Short-term sick pay increased during the pandemic, but, unlike tax payments, it was insensitive to local infection rates, which indicates behavioral responses to more generous sickness insurance rules during the pandemic.

Keywords: Covid-19 impact, VAT, Excise taxes, Sick pay.
JEL codes: H24, H25, J22, J24

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1 Introduction

The Covid-19 pandemic caused large economic downturns in many countries and governments have launched ambitious policies to mitigate its consequences on society. For these policy measures to function as intended, it is important to properly assess the pandemic's economic impact. Not only are updated and encompassing data needed, but methods to identify the impact of the crisis are also needed. A recent literature has emerged to address these needs, proposing novel approaches to analyzing the economic impact of the Covid-19 pandemic in different countries. For example, Andersen et al. (2020) examine private bank transaction data to gauge the impact of lockdown restrictions on economic activity. Chetty et al. (2021) collect high-frequency data from a wide array of private-sector sources in the United States, mapping details in consumer behavior and business revenues during the pandemic. Using a similar approach, Chen et al. (2021) track Chinese consumption responses using data on bank card purchases and mobile phone usage. Some studies have analyzed the impact on tax revenues by combining simulation approaches and different evidence-based outcomes (for example, Clemens and Veuger 2020 and Green and Loualiche 2021).[1]

In this study, we analyze the effects of the Corona pandemic on private sector real activity using high-frequency tax register data collected from the Swedish tax authorities. We make two specific contributions. The first is to use administrative tax registers on firm-level outcomes to analyze the effect on firm turnover and tax payments. Outcomes are based on taxes paid at a monthly frequency, which are reported by firms continuously and thus allows an assessment almost in real-time. While more traditional sources show economic outcomes in the macroeconomy that are based on survey records obtained from more infrequently sampled populations, our full-population tax data offer a faster and often more precisely measured picture of private sector activities. Specifically, our analysis uses data from the Swedish corporation tax registers kept by the Swedish National Tax Agency covering all monthly firm-level records in Sweden on value-added tax transactions, employment taxes (payroll taxes, income taxes), short-term sick-leave remuneration and registered payments of excise taxes on air travel, gasoline, tax deductions for industrial electricity usage, and commercial advertising. Studying the case of Sweden is relevant since the country experienced similar patterns in Coronavirus contagion during 2020 as in other developed countries despite Sweden's initially less restrictive prevention policies.[2] However, our analysis of high-frequency firm-level tax data could serve as a useful benchmark for other countries for which such data are also collected continuously by tax authorities and, in principle, available to researchers and analysts wanting to analyze the real economy with high precision and minimal time duration.

[1]Needless to say, there are numerous reports by government ministries, statistical bureaus and international organizations examining the economic consequences of the Covid-19 pandemic.

[2]See, for example, SOU 2022.

The second contribution of the paper is to present an approach to identify the impact of the pandemic. This is a difficult task in the absence of a reasonable counterfactual, and simple before-after comparisons may be misleading in the presence of both long-term trends and short-term variability. Our identification strategy combines within-year variation and across-year variation and also geographical differences. The Coronavirus started spreading in Sweden a few months into 2020, and we use this within-year variation to control for similar seasonal variations across pre-pandemic and pandemic years in addition to the yearly changes. To our knowledge, this is the first time the effect of the pandemic on economic outcomes has been estimated in this manner.[3]

We obtain the following results. The tax data confirm the profound impact of the pandemic on private-sector activity and on tax revenues. Firm sales, calculated from VAT payments, fell more than six percent as a result of the pandemic. Electricity usage, estimated from tax deductions for electricity usage for industrial purposes, dropped almost six percent. Compared to commonly used monthly indexes for industrial production and service sector value-added, our estimated effects on turnover are at the same level or slightly higher. The estimates also show that the impact of the pandemic varies with the intensity of the Coronavirus infection rates. In municipalities with less infections, the downturn in VAT and sales was significantly smaller.

As for the tax receipts, we find that VAT revenues fell somewhat less than actual turnover, which is explained by the fact that Sweden uses differentiated VAT rates and that the biggest downturn occurred in low-VAT sectors. Labor income taxes responded less to the downturn, falling around three percent. This milder impact reflects the government support programs that have channeled wage support directly to firms and employees in proportion to their pandemic-related losses.[4] Notice that none of these tax rates were altered during the pandemic and neither did reporting standards change in the tax administration during the pandemic.[5]

The impact of the pandemic on sickness absence, arguably one of the most direct outcomes of a pandemic, is estimated using firms' sick-pay reports in the tax registers.[6] We find that sick pay soared during the pandemic; the average impact is 71 percent during all of 2020, but it was much higher levels in the months directly after the pan-

[3]We are inspired by a similar identification approach by Johansson and Palme (2005), who used it to measure the impact of a policy reform in the sickness insurance system on worker sick-leave.

[4]Angelov and Waldenström (2023) analyze the distributional impact of the Swedish government's Covid-19 support policies across firms and individuals.

[5]However, the payroll tax rate was temporarily lowered from April 2020, which accounts for a part of the drop in payroll tax revenues related to the pandemic, as we discuss below in section 4.3. See also the public investigation of the Swedish Covid-19 policies SOU 2022

[6]In Sweden, firms are responsible for employees' sick pay during the first 14 days (for longer sickness absence than that, government sickness insurance takes over). Sick pay is capped at 80 percent of the salary up to a ceiling. Before the pandemic, the first two days of absence were qualifying days, during which no sick pay is paid out. During the Covid-19 pandemic in 2020, the government temporarily abolished the two qualifying days and also overtook responsibility for firm's sick pay costs, in full during March-June and partially thereafter.

demic's outbreak. Interestingly, when we examine the association between the effect of the pandemic on firms' sick pay and the level of registered Covid-infection rates in the municipality where each firm is registered, we find no effect variation. This contrasts the VAT payments and firm turnover, for which the effect clearly varies with local virus infection status. Whether the lack of a traceable link between morbidity and sick pay reflects measurement errors or the presence of incentive effects, generated by the generous government support to compensate firms for their sick pay costs and removing the qualifying days for absent employees, is an intriguing question worthy of further analysis.

The remainder of the paper is structured as follows. Section 2 presents the identification strategy used to estimate the impact of the Covid-19 pandemic on economic outcomes. Section 3 outlines the Swedish tax-register data. Section 4 presents the results of estimating the impact of the pandemic, first on VAT payments and firm sales, then on excise taxes, and lastly on labor income taxes and sick pay expenses. Section 5 concludes.

2 Estimation approach

Estimating the Covid-19-impact on any chosen measure of economic activity is challenging because of the apparent lack of reasonable counterfactual. It is always possible to provide a crude before-after comparison, but inspecting the outcomes in the following section shows that such an estimate would not be credible. The reason is that there is enough within- and between-year variation to obstruct any strong conclusions from a raw mean comparison over calendar time. Furthermore, no sector in the economy can be assumed to be unaffected by the pandemic, so a standard difference-in-differences-type (DD) group comparison over calendar time would also not be credible.

In the present paper, we go beyond the before-after comparison and utilize within-year variation over two different calendar years. We use the fact that the first two months of 2020 were pandemic-free (see our discussion below about the exact timing of the Covid-19 outbreak in Sweden) and then compare the change in a particular outcome variable between the last ten months and the first two months of 2020 with the corresponding change in 2019 to assess the impact of the pandemic.

We believe that our chosen estimator is less problematic than a before-after estimator measuring changes over time (such as comparing post-pandemic values in 2020 to the same period in 2019, or post- with pre-pandemic months in 2020). The main reason is that our estimator controls for nominal changes over calendar years and within-year variation that would have occurred even in the absence of the pandemic. As will be seen in the descriptive figures over various outcomes in this paper, such within-year variation appears to occur in a systematic manner across years. However, although we argue that our chosen estimator has advantages compared to a before-after estimator, we do not argue that we are able to estimate the impact of the pandemic without any bias. The main reason is that we have no way of controlling for potential business-cycle effects that would have occurred in absence of the pandemic. Below, we provide a more formal discussion of the chosen estimator and of the identification challenges.

To fix ideas, let Y_{ipt} denote the outcome variable of interest (for example, VAT payments) for firm i during the period January-February ($p = 1$) or March-December ($p = 2$) measured in year $t = 2019$ or 2020. Furthermore, let $D_t = \mathbf{1}[t = 2020]$ where $\mathbf{1}[\cdot]$ is the indicator function taking the value one if the expression within brackets is true and zero otherwise, and $S_p = \mathbf{1}[p = 2]$. In the empirical analysis to follow, we will use monthly data. In order to convey the main ideas, it is useful to disregard the monthly dimension for now and take $p = 1, 2$ as our within-year observation frequency. Keeping that in mind, consider the following model:

$$Y_{ipt} = \delta + \theta_1 D_t + \theta_2 S_p + \theta_3 D_t S_p + u_{ipt}, \tag{1}$$

where u_{ipt} is an error term. Let $\bar{Y}_{pt} = 1/N_{pt} \sum_i Y_{ipt}$ (N_{pt} being the number of observations during period p and calendar year t) and finally, $\Delta Y_t = \bar{Y}_{2t} - \bar{Y}_{1t}$. Using a random sample of the population (recall that we have the complete population), the parameter of interest θ_3 identifies

$$E(\Delta Y_{2020} - \Delta Y_{2019}). \tag{2}$$

Technically, this is a DD-estimator but the group assignment is somewhat unorthodox.[7] Whereas in an ordinary setting D would denote treated units, in this case it denotes (to a large extent) the same units measured during two separate calendar years. Moreover, whereas S in an ordinary application would denote post-treatment with respect to calendar time, in our case it measures within-year period.

Whether θ_3 identifies the impact of the pandemic hinges upon two critical identifying assumptions. First, there is the choice of within-year periods defined by p. If the pandemic had an impact on the economy before March 2020, the chosen definition of p means that we underestimate the effect. If, on the other hand, there was in fact no effect before April or even May, we might lose some precision. Weighting the different alternatives, although the choice of p is somewhat *ad hoc*, we feel reasonably safe choosing March as the first potential effect-month. In Sweden, the Coronavirus was not a big deal before the end of March. During the first months of the pandemic, measures were very

[7]Using within-year variation in the way we do is not entirely new to the literature. Using Swedish data, Johansson and Palme (2005) study the effect of a reform in the national sickness insurance on absence behavior, whereby the replacement rate was reduced. The reform came into force on 1 March, 1991 and to measure the overall change in the prevalence of work absence Johansson and Palme (2005) used a differences-in-differences estimator whereby the change between January/February and March/April 1991 was compared with the corresponding change in 1990.

lax in an international context, so government-mandated measures almost by definition cannot have had an effect on the economy in January or February. Of course, it cannot be excluded that the economy was affected by changed behavior among the general population or among other economic agents, but even this seems unlikely to have happened in Sweden during the first two months of 2020. In fact, as late as February 25, the Swedish Public Health Agency deemed the risk of general transmission of the virus in Sweden as low.[8] To sum up, we believe that there are no good reasons to expect a significant Covid-19-effect in January or February 2020. As individuals and other economic agents increasingly became aware of the pandemic during March 2020, especially after the assessment of Covid-19 as a pandemic by the WHO on March 11,[9] it seems reasonable to choose March as the starting month.

Second, for θ_3 to measure the effect of the pandemic, we need

$$E\left(\Delta Y^0_{2020} - \Delta Y^0_{2019}\right) = 0, \tag{3}$$

where we use the potential outcomes framework such that ΔY^0_t is defined as the within-year difference during year t under the assumption that there was no pandemic during year t. This is the analogue to the parallel trends-assumption in ordinary DD-settings and is not directly testable. One circumstance under which (3) would fail is if there is business-cycle variation in the average within-year variation in the outcome variable. For example, assume that 2020 and 2019 would have been in significantly different parts of the business cycle in absence of the pandemic, with 2020 faring badly in comparison. Then the ordinarily good sales figures in June would perhaps have been lower compared to January even without Covid-19 having hit the economy in 2020.

We have no direct means of assessing the counterfactual (pandemic-free) business-cycle state of the economy in 2020. However, there were clear signs already in 2018 that the economy had entered a slowdown phase. If the slowdown had been even more severe during 2020 in absence of a pandemic, than in 2019, our effect estimates are overestimated in magnitude. Although it is hard to assess the magnitude of the potential bias, we believe that the size of the bias relative to the pandemic estimates that we present is relatively small. The National Institute of Economic Research (NIER) is a government agency that performs analyses and forecasts of the Swedish and international economy as a basis for economic policy in Sweden. According to the last quarterly assessment of the Swedish economy in 2018 by NIER, published in December 2018, the Swedish economy had peaked in the first half of 2018 and was heading into a slowdown phase (Konjunkturinstitutet, 2018). In March 2019, NIER made the same assessment (Konjunkturinstitutet, 2019b). In the last pre-pandemic assessment, in December 2019,

[8]Source: https://www.folkhalsomyndigheten.se/nyheter-och-press/nyhetsarkiv/2020/februari/forandrad-riskbedomning-for-fall-av-Covid-19-i-sverige/. See also SOU 2022.

[9]Source: https://www.who.int/news/item/29-06-2020-Covidtimeline.

NIER wrote that the Swedish economy had entered a clear slowdown phase in 2019 (Konjunkturinstitutet, 2019a). Importantly, NIER's forecasts made in December 2019 for the yearly GDP-growth for 2019, 2020, and 2021 were 1.1 percent, 1.0 percent, and 1.5 percent, respectively. Thus, according to the best available forecasts of the Swedish economy prior to the pandemic, the GDP-growth in 2020 was expected to be positive and only marginally lower than the growth in 2019. According to the same source, an economic recovery was expected in 2021.

All in all, given the above mentioned identification challenges, it is reasonable to expect some bias in the effect estimates presented in this paper. Although we have argued that we expect the magnitude of the bias relative to the estimates presented in later sections to be relatively small, it is important to keep in mind throughout the text that estimating the effect of the pandemic on any country's economy is difficult. We do, however, believe that although the DD-approach that we have chosen is far from perfect, it is a more tenable alternative than a before-after comparison.

The DD-approach also allows for an informal assessment of the parallel-trend assumption (3) for outcome variables for which we have available data for 2018. We will present a placebo analysis in an appendix where we assume that 2019 is the treatment year and 2018 is the control year. It should be stressed that the placebo (as well as any check of observed parallel trends prior to treatment) is not a proper test of whether the identification assumptions hold. We believe that a placebo is nonetheless useful, and as will be seen in the results section, it has lead to the removal of outliers in firm-sales data such that unusually high sales values during a particular month do not exert a too large influence on the results.

We start by estimating a version of (1) on monthly data and with firm-fixed effects. The empirical specification is the following:

$$Y_{imt} = \delta_0 + \theta_1 D_t + \theta_2 S_m + \theta_3 D_t S_m + \delta_i + u_{imt}, \tag{4}$$

where $m = 1,2,\ldots,12$ denotes month, $S_m = \mathbf{1}[m \geq 3]$, δ_i is a firm-fixed effect, and D_t is defined as previously. As is clear from (2), the effect is identified on group level and the treatment assignment is also on group level. To a large extent, treated firms (during 2020) act as their own controls (during 2019) in this setting since there is a large population overlap in the two years. The firm-specific fixed effects are therefore not needed for consistency, but we include them to increase precision. It is reasonable to assume that the pandemic hit different industries in varying degrees, leading to within-industry correlation in the error terms. Therefore, we cluster the standard errors at the industry level. We explain the used industry classification in the next section.

3 Data

We use data on firms using full-population tax registers administered by the Swedish Tax Agency. The registers consist of employer-reported records, filed at a monthly frequency and we access data from January 2017 to March 2021.[10] The analysis centers around three tax categories: value-added taxes (VAT), excise taxes, and personal taxes on labor and sick pay.

Value-added taxation (VAT) is a comprehensive consumption tax in Sweden, representing over one-fifth of total tax revenues. The VAT targets most sectors and transactions with a 25 percent VAT and a few sectors with a lower VAT rate, and these rates have not changed during the Covid-19 pandemic.[11] This analysis uses firm-level data on VAT payments that are recorded in the VAT register by the Swedish Tax Agency. These data cover all VAT payments in the country and by using the statutory VAT rates, we are also able to back out the value of transactions for each firm. The VAT-reporting frequency is either monthly, quarterly or yearly, but the majority of firms report VAT every month. There is a considerable concentration of large firms among those reporting each month: about two-thirds of the firms (representing 90 percent of total VAT payments) report VAT each month. In the analysis, we only use data over firms reporting each month since we want to measure the within-year variation in VAT and sales as precisely as possible on a monthly basis. Thus, the population of interest in the VAT and sales analyses are firms that report monthly. As we use monthly dynamics throughout the paper, it is not reasonable to include firms reporting more seldom, even if interpolation of data points is possible. This is further discussed in section A2.

Excise taxes comprise levies and duties on more than 40 different sorts of transactions and activities that represent about 7 percent of total taxes in Sweden. We restrict the analysis to four large and economically interesting excise taxes. Two of them concern production activities: energy use tax for energy-intensive manufacturing firms (a proportional tax on the consumption of electrical energy) and on advertising (tax on printed commercials, advertisements, posters and signs etc.). Two are taxes on transportation activities: the gasoline tax (a tax on energy and CO2) and the tax on air travel (a fixed duty per passenger).

The estimation datasets using excise taxes and VAT data differ somewhat. First, for excise taxes, we have kept the whole population in the estimation sample. As discussed in section A2, it is reasonable to assume that monthly firm sales data (and the corresponding VAT-data) contain outliers that can distort the estimation results. Therefore, the outliers were removed in the regression analysis on sales and VAT. For excise

[10] Our main analysis uses 2018-2021 observations and we use 2017 data in placebo regressions. There exist earlier firm-level tax data but these were not accessed for this project. Due to late reporting by some firms, it may be up to one month delay until full population coverage for a month is achieved.

[11] The VAT is 12.5 percent on foodstuff and 6 percent on book sales.

taxes however, the way data is collected does not provide any clear *a priori* reason to suspect large outliers. This is because that for a specific excise tax (say, on air travel), the data points (firm-month observations) reflect consumption or sales made by a large number of economic agents (consumers or firms). In other words, compared to monthly firms sales data, there are considerably fewer situations where a single order or other economic transaction can constitute an extreme outlier. This conjecture is supported by the placebo tests we perform for excise taxes, which will be shown after the presentation of the results.

Taxes on labor and sick pay refer to employer-reported payroll taxes (arbetsgivar-avgifter), withheld municipal and state income taxes, and employers' expenses for short-term sick-leave pay (sjuklön). Payroll taxes include several fees and taxes, some granting a drawing right in the social security system (for example, on old-age pensions) while others being pure statutory taxes. The municipal and state income taxes are the preliminary withheld tax payments made by employers. It should be noted that in the annual tax returns, employees receive tax reimbursements due to the earned income tax credit (jobbskatteavdrag) which is granted to everyone but has its largest proportional impact on low salaries. Sick pay is the reimbursement to employees on short-term sick leave that employers are obliged to pay for (up to the first 14 days of absence). Due to the Covid-19 pandemic, the government overtook the payment of all sick pay during March–June 2020 and partially for later months in 2020. In terms of reporting, employers were still required to pay in sick pay as before, but received ex post a repayment from the authorities. Our data consists of the sick-leave payments to the employees.

The analysis results from the estimation of (4) using monthly firm-level data covering 2019 and 2020 and consisting of payroll taxes, withheld personal taxes and the amount of sick leave pay. For instance, each firm-month observation of payroll taxes is the total sum of payroll taxes that the firm has paid for all its employees during the particular month. Two of the three outcome variables (payroll taxes and sick pay) are only available at firm level. The third one, withheld personal income taxes, is available also at the individual level but in order to keep the measure units intact, we use firm-level data for all three outcomes.

Because these monthly data were not collected in the administrative registers prior to 2019, we are not able to run placebo tests as in sections A2 and 4.2. We have not removed any outliers as was done in the sales and VAT-data in sections A2 since we do not believe that outliers pose a serious problem for the data used in this section. Thus, we run the estimation on the complete population using firm-fixed effects and standard errors clustered at the industry level. For the industry classification, we use the Swedish Standard Industrial Classification (SNI) of Statistics Sweden. In the Swedish Tax Agency data, if a firm is active in several industries, each firm is given the SNI-code corresponding to the largest share. We use the two-digit level denoted division by

Statistics Sweden, resulting in 88 industries.[12]

4 Results: Economic impact of the pandemic

In this section, we provide descriptive results and then estimate the impact of the Covid-19 pandemic on tax revenues using our econometric specification outline above. Three tax categories are analyzed: value-added taxes, excise taxes, and labor income taxes (where we also study sick pay).

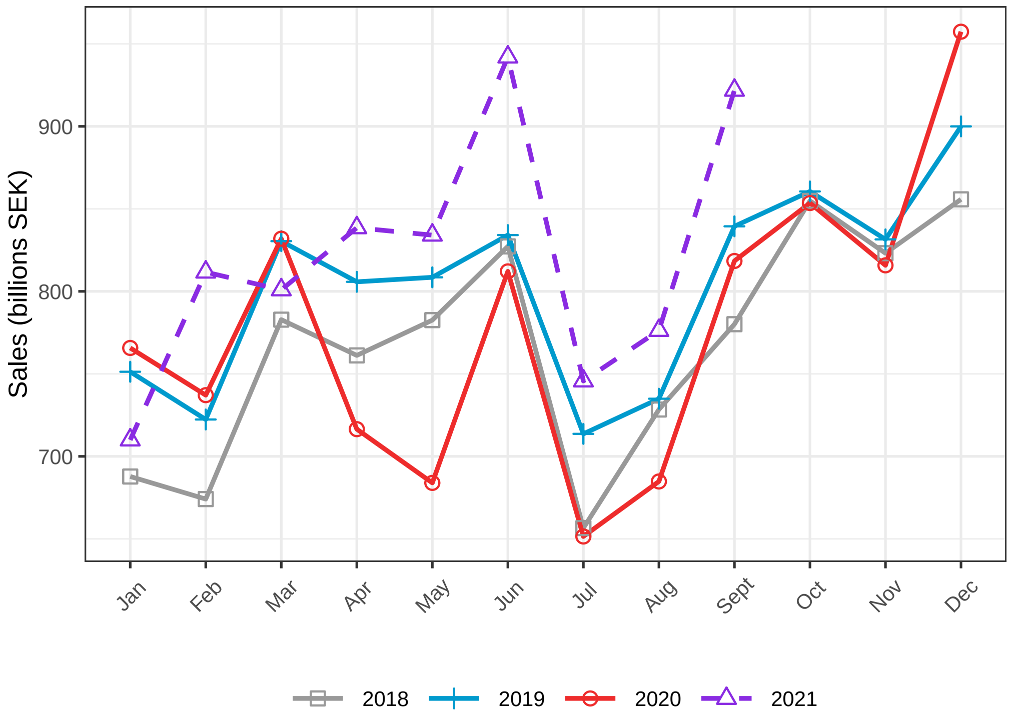
4.1 Value-added taxes and firm sales

Figure 1 displays the evolution of monthly firm sales and figure 2 shows VAT receipts of VAT-paying firms during 2018-2021. VAT payments trend upwards within each year, with a marked break during the vacation month of July. The pandemic does not seem to have changed this pattern. VAT receipts appear to have decreased after the outbreak of the Coronavirus in April–May 2020 and thereafter went back to almost pre-pandemic levels. Total turnover, or sales, among VAT-paying firms have been more variable over time. Turnover levels in 2020 and early 2021 are clearly lower than in the same months during 2019 except for December 2020 and February 2021. The larger impact on sales than on VAT is primarily explained by the fact that sectors where turnover fell the most, transportation and cultural events, are sectors with the lowest statutory VAT rate.

As noted in section 3, firms that report with a lower frequency than monthly (for instance, each trimester or year) have been removed from the analysis since we want to measure the within-year variation in VAT and sales as precisely as possible on a monthly basis. Since large firms usually report each month, we lose only about 10% of the amounts in data in this way. In principle, it is possible to interpolate monthly data from lower-frequency data under the (unrealistic) assumption that VAT and firm sales are equally distributed across months. To illustrate what this would mean in practice, we have included figure A1 in appendix A that shows the firm sales data for firms reporting each month (bottom-right, same as Figure 1), as well as interpolated data using: all firms; VAT-reporting period 1, 3, or 12 months; and VAT-reporting period 1 or 3 months. As figure A1 shows, going from all firms to firms reporting each month does not change the overall appearance. As interpolating data for firms not reporting each month would by definition introduce measurement errors of unknown size at the firm level, we think it is better to use only data on firms reporting each month.

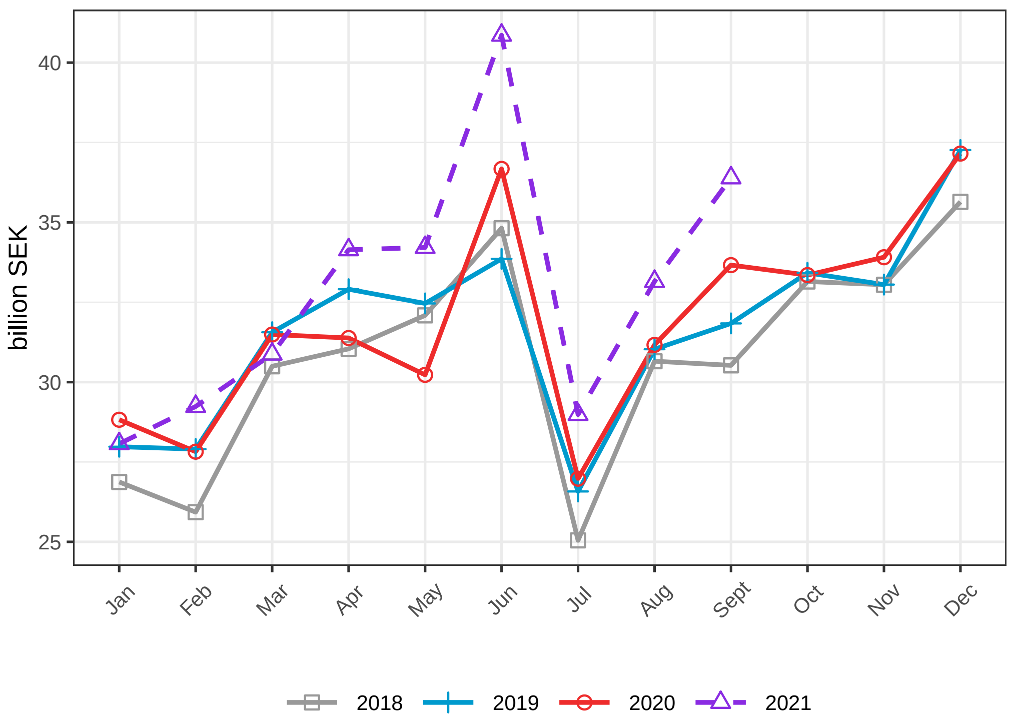
[12]See https://www.scb.se/en/documentation/classifications-and-standards/swedish-standard-industrial-classification-sni/ for details.

Figure 1: Firm sales, 2018-2021.



Note: Monthly sales of VAT-paying firms corresponds to the firm's total sales, including export and in some rare cases non-VAT eligible sales. SEK/EUR≈ 0.1

Figure 2: Value-added taxes, 2018-2021.



Note: Monthly net value-added tax (VAT) payments from all output VAT-variants (6%, 12% and 25%) net of input VAT. SEK/EUR≈ 0.1.

Table 1: Fixed-effect estimates of the Covid-19-impact on firm sales and VAT.

	Mean effect		Interaction with municipal Covid infection rate ($QG1 = Lowest, QG4 = Highest$)	
	(1)	(2)	(3)	(4)
	Sales	VAT	Sales	VAT
$Covid\ impact$	-15.21^{**}	-2.51^{**}	-19.36^{**}	-3.28^{***}
	(4.67)	(0.80)	(6.06)	(0.99)
$Impact \times QG_3$			7.22^{*}	1.30^{*}
			(3.30)	(0.53)
$Impact \times QG_2$			6.79^{*}	1.38^{**}
			(3.11)	(0.49)
$Impact \times QG_1$			8.95	1.52
			(5.01)	(0.92)
$Year\ 2020$	0.06	0.12	−1.05	−0.05
	(1.23)	(0.22)	(1.71)	(0.29)
$March - Dec$	49.44^{***}	9.02^{***}	45.69^{***}	8.24^{***}
	(6.23)	(0.89)	(5.62)	(0.78)
QG_3			-16.56^{***}	-3.24^{***}
			(4.34)	(0.85)
QG_2			-11.57^{***}	-2.54^{***}
			(3.17)	(0.51)
QG_1			-38.33^{***}	-7.84^{***}
			(8.57)	(1.53)
$Year\ 2020 \times QG_3$			3.04^{*}	0.45
			(1.32)	(0.24)
$Year\ 2020 \times QG_2$			2.12	0.29
			(1.49)	(0.24)
$Year\ 2020 \times QG_1$			7.39^{**}	1.55^{**}
			(2.68)	(0.54)
$March - Dec \times QG_3$			7.11^{**}	1.39^{**}
			(2.38)	(0.47)
$March - Dec \times QG_2$			10.44^{***}	2.10^{***}
			(2.37)	(0.43)
$March - Dec \times QG_1$			3.19	0.92
			(3.71)	(0.70)
$Mean\ March - Dec\ 2019$	245.57	45.61	245.57	45.61
$Covid\ impact$ (%)	-6.2	-5.49	-7.88	-7.19
$Impact \times QG_3$ (%)			2.94	2.85
$Impact \times QG_2$ (%)			2.76	3.03
$Impact \times QG_1$ (%)			3.64	3.33
Observations	5,189,228	5,189,228	5,189,228	5,189,228
Adjusted R^2	0.60	0.63	0.60	0.63

Note: *p<0.05; **p<0.01; ***p<0.001. The table presents regressions using firm-level records on monthly VAT payments and total sales in billions of SEK (SEK/EUR≈ 0.1).

We now continue with the regression-based analysis of VAT and firm sales. As mentioned in section 2, it is possible to perform a placebo analysis using data from 2018 and 2019. It should be stressed that such an analysis, akin to the commonly used informal parallel-trend check, is not a proper test of the identification assumption. The placebo is presented and discussed in appendix B. As explained in length in the appendix, guided by the results from the placebo, we have removed outliers using the interquartile range-procedure. To keep the sample the same irrespective of outcome variable, we have removed outliers with respect to firm sales. This is reasonable also because sales are the tax base for VAT and if it were not for the different VAT-rate levels for different goods (6%, 12% and 25%), the results for sales and VAT would have been in practice indistinguishable. Removing outliers in this manner is found to result in insignificant placebo effect estimates, both in economic and statistical terms.

Table 1 presents the results from estimating equation (4) on monthly firm sales and VAT data during 2019 and 2020. For firm sales, the effect is about –15,210 SEK, or about –6.2%, and the corresponding numbers for VAT are is –2,510 SEK (–5.49%). Both estimates are significant at the one-percent level. In table A1 in appendix A, we have calculated back-of-the-envelope numbers for the effect of the pandemic on commonly used monthly macro data on industrial and service production in Sweden. Compared to the numbers on the industrial and service production indices, the effects on firm sales and VAT have the same negative sign and are very similar in magnitude (5–6% compared to 5–7%).

How much of these negative effects are due to a general Covid-impact on the economy as a whole, and how much can be attributed to a direct impact of Coronavirus contagion? It is not possible to provide a direct answer to this question since the whole of Sweden was affected by both factors. We can however utilize the geographical variation of infection rates in Sweden during 2020 to shed some light on the issue. As we explain in detail below, we will use data at the finest available detail level (Sweden's 290 municipalities). An alternative would be to use Covid-related mortality or hospitalization data. Unfortunately, we do not have access to such data on municipal level. It could also be argued that in media, data over infection numbers were more commonly available than data over mortality, although we do not have objective data to back up this assertion. In addition, in figure A2 in appendix A, we present graphical evidence of clear positive correlation at the geographical level between three different potential measures of the level of Covid-spread, all measured per 100,000 individuals: infection rates, number of individuals in intensive care due to Covid, and number of Covid-related deaths. The calculations are the same as for infection rates which we detail below. However, instead of at the municipal level as in the heterogeneity analysis, the correlation analysis is performed at the highest available detail level for the three measures, which is Swedish regions. Regions are administrative units that, among other things, organize and finance the health care within their geographical area. There are 21 regions in Sweden and many of them cover vast geographical areas, which makes region-based data unsuitable for the heterogeneity analysis we have in mind.

This heterogeneity analysis is done along two lines of reasoning concerning the mechanism behind the observed effect: i) an overall decrease of economic activity in the whole

Swedish economy or ii) an overall decrease with an additional effect due to the Covid infection rate within the municipality where the firm is registered. As there were no restrictions on the municipality (or any other regional) level in Sweden, this additional effect can be interpreted as a manifestation of behavioral changes among consumers or firms as a result of precaution or fear of contracting or spreading the virus. In the following, we attempt see whether there is support in data for ii) over i) by interacting all categorical covariates in (4) with the a function of the Covid infection rate in each firm's municipality of registration.

The infection rates are calculated as follows. First, we retrieved weekly Covid infection numbers at the municipality level for the period March-December 2020 from the Swedish Public Health Agency.[13] During a particular week running over two separate months, the month in which Thursday occurs decides the month where all Covid cases are registered. This is only relevant for the first week in March and the last week in December 2020, since we sum the total number of cases for each municipality from March to December. This corresponds to the definition of S_m in equation (4). We then divide the total number of cases by the population in each municipality on December, 31, 2020 using data from Statistics Sweden.[14]. Finally, using C_k =*the total number of Covid cases from March to December 2020 per capita in municipality* k, we divide firms into four quartile groups, $QG_1 - QG_4$. Firms are ranked according to their registration municipality's infection incidence with QG_1 being municipalities with C_k below the first quartile, QG_2 with C_k between the first and second quartiles, and so forth.

Before we turn to the results from this heterogeneity analysis, it is important to stress that the results should be regarded as suggestive, rather than clearly indicative. First, there might be confounding factors at the municipal level that cause effect heterogeneity and are positively correlated with the Covid infection rate. For one, the reported infection numbers are not perfect. As the testing capacity increased throughout 2020, so did the reporting rate. Since we estimate the effect heterogeneity with respect to geographical variation (and not variation over time), this does not necessarily have to be problematic. However, if the misreporting depends on municipality characteristics, our effect heterogeneity estimates might capture the effect heterogeneity with respect to those characteristics rather than to the variation in infection rates. Second, many firms (especially large ones) operate on a national level, meaning that it is not reasonable to expect them to react significantly to local infection rates. Firm operations could in some cases still be affected by local infection rates. This could happen for instance if employees report sick due to actual sickness or precaution, especially for firms that predominantly sell services. However, it is far from clear how large such effects might be expected to be.

[13]https://www.folkhalsomyndigheten.se/smittskydd-beredskap/utbrott/aktuella-utbrott/Covid-19/statistik-och-analyser/bekraftade-fall-i-sverige/.

[14]https://scb.se/hitta-statistik/statistik-efter-amne/befolkning/befolkningens-sammansattning/befolkningsstatistik.

Keeping these objections in mind, we now proceed with the heterogeneity analysis.

The results from interacting with dummy variables corresponding to $QG_1 - QG_4$ are presented in the last two columns in Table 1. The group of firms registered in municipalities with the largest accumulated Coronavirus infection rate (that is, those in QG_4) are chosen as the reference. If conjecture ii) above is correct, we expect that a negative effect in QG_4 is larger in magnitude than in the other groups, which have lower infection rates. For firm sales, the effect estimate for QG_4 is –19,360 SEK (–7.88%) and statistically significant at the one percent level. The positive estimate for $Impact \times QG_3$ of 7,220 SEK (2.94%) indicates that the pandemic had a lower impact on firms in municipalities with infection rate between the median and the third quartile. For QG_2, the relative effect is of roughly the same magnitude (2.76%) and in QG_1 the effect is not statistically significant but the point estimate is larger in magnitude (3.64%). The interaction results for VAT in the last column in table 1 are very similar: The Covid-impact in QG_4 is about –7.19% and the relative effects in $QG_1 - QG_3$ are 2.85%, 3.03%, and 3.33%, respectively. As with firm sales, the relative effect in group QG_1 is not statistically significant at the five percent level.

The estimates presented in table 1 measure the average impact of the pandemic from March to December 2020. It is reasonable to assume that the severity of the impact varied during the year. To this end, table 2 presents the impact for firm sales and VAT estimated separately for three periods during 2020: March-May, June-August, and September-December. Below, we summarize the findings for firm sales only, as the estimates for VAT measured in percent are similar. Starting with the average seasonal effects in column (1), we find the strongest effect during the initial stage of the pandemic (-25,210 SEK or -10.27% for sales) and the least effect during September-December (-8,290 SEK or -3.37% for sales, not statistically significant at the 5%-level).

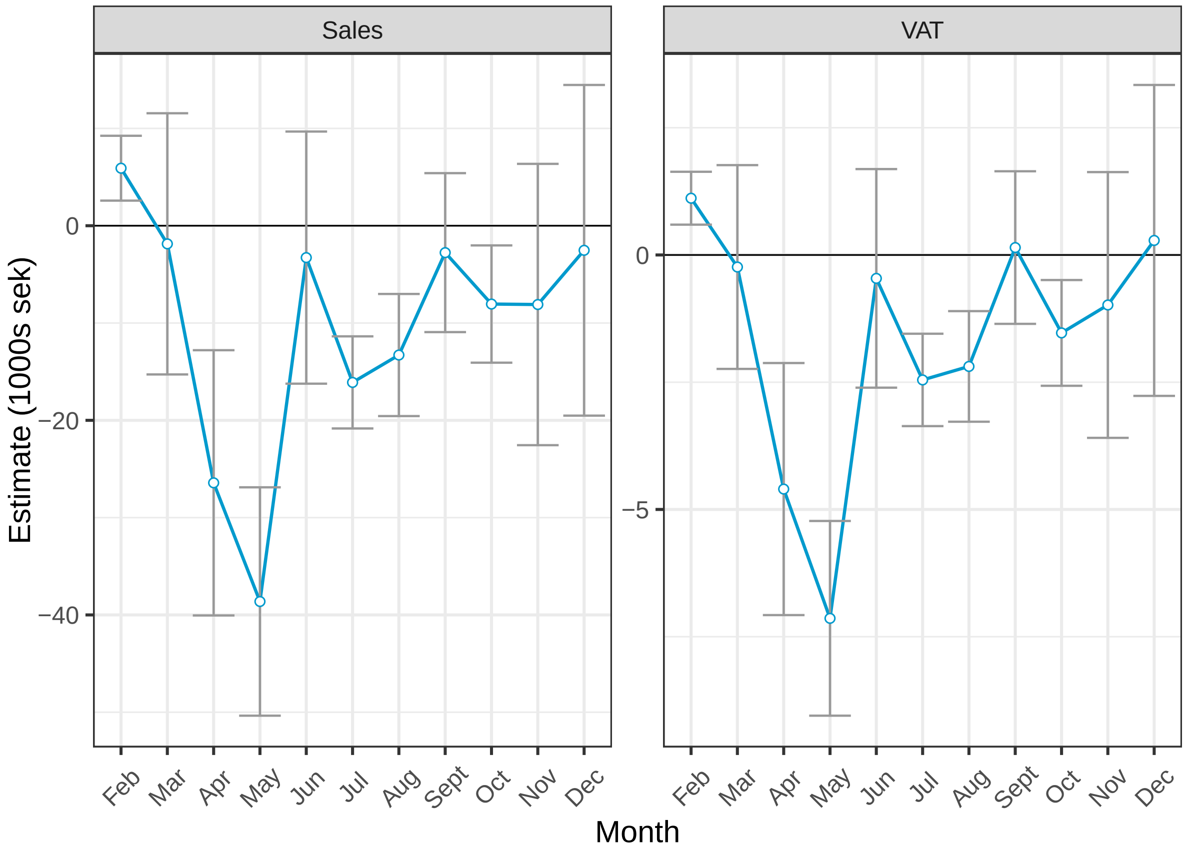
Table 2: Fixed-effect estimates of the Covid-19-impact on firm sales and VAT: estimates by season

	(1) Sales	(2) VAT
$Covid\ impact\ March-May$	-25.21^{***}	-4.54^{***}
	(6.09)	(1.01)
$Covid\ impact\ June-August$	-13.74^{***}	-2.24^{***}
	(3.38)	(0.58)
$Covid\ impact\ September-December$	-8.29	-1.07
	(4.90)	(0.88)
$Year$ 2020	0.33	0.16
	(1.20)	(0.22)
$March-May$	46.95^{***}	9.01^{***}
	(4.94)	(0.79)
$June-August$	31.56^{***}	5.60^{***}
	(5.65)	(0.97)
$September-December$	64.98^{***}	11.62^{***}
	(9.25)	(1.20)
$Mean\ March-December$ 2019	245.57	45.61
$Covid\ impact\ March-May$ (%)	-10.27	-9.95
$Covid\ impact\ June-August$ (%)	-5.59	-4.91
$Covid\ impact\ September-December$ (%)	-3.37	-2.35
Observations	5,189,228	5,189,228
Adjusted R^2	0.60	0.63

Note: *p<0.05; **p<0.01; ***p<0.001. The table presents regressions using firm-level records on monthly VAT payments and total sales in billions of SEK (SEK/EUR≈ 0.1).

Finally, in figure 3 we present estimates along with 95-percent confidence intervals of the effect of the Covid-pandemic by month, starting with the pandemic-free month of February, 2020. Estimating the effect on a monthly basis introduces some noise. For instance, there is a small statistically significant positive placebo effect during February. Although this positive effect might be interpreted as the pandemic causing a negative shock during a trend that would have been positive in absence of a pandemic, we think that this would be a stretch given the relatively high naturally occurring within-year variation in sales. This illustrates the difficulty to fully control for changes in economic conditions using our empirical approach. On the whole, however, the monthly results are very much in line with the estimates presented in table 2, i.e., that the strongest effects are found during the initial stage of the pandemic. This is also in line with the descriptive analysis in previous chapters.

Figure 3: Covid impact on sales and VAT by month



Note: The figure shows estimates along with 95-percent confidence intervals of the effect of the Covid-pandemic by month. The empirical specification is based on equation (4). The only difference is that the dummy variable S_m is replaced by a set of dummy variables for month (February–December). The effect estimates are expressed in 1000s SEK (SEK/EUR≈ 0.1).

Altogether, the results from tables 1 and 2 suggest that the the pandemic had a significant negative impact on firm sales and VAT, and that the impact was especially pronounced during the initial stages of the pandemic. We also find that the local Covid infection rate is an important factor behind firms' lost revenue. Since all quartile groups, in varying degrees, have been affected by the local Covid infection rate, it is hard to pinpoint the relative effect sizes of the general downturn of the economy due to Covid and the extra effect due to the local infection rate. However, the effect variation with respect to the Covid infection rate is fairly large, suggesting that the local contagion effect might be an important factor.

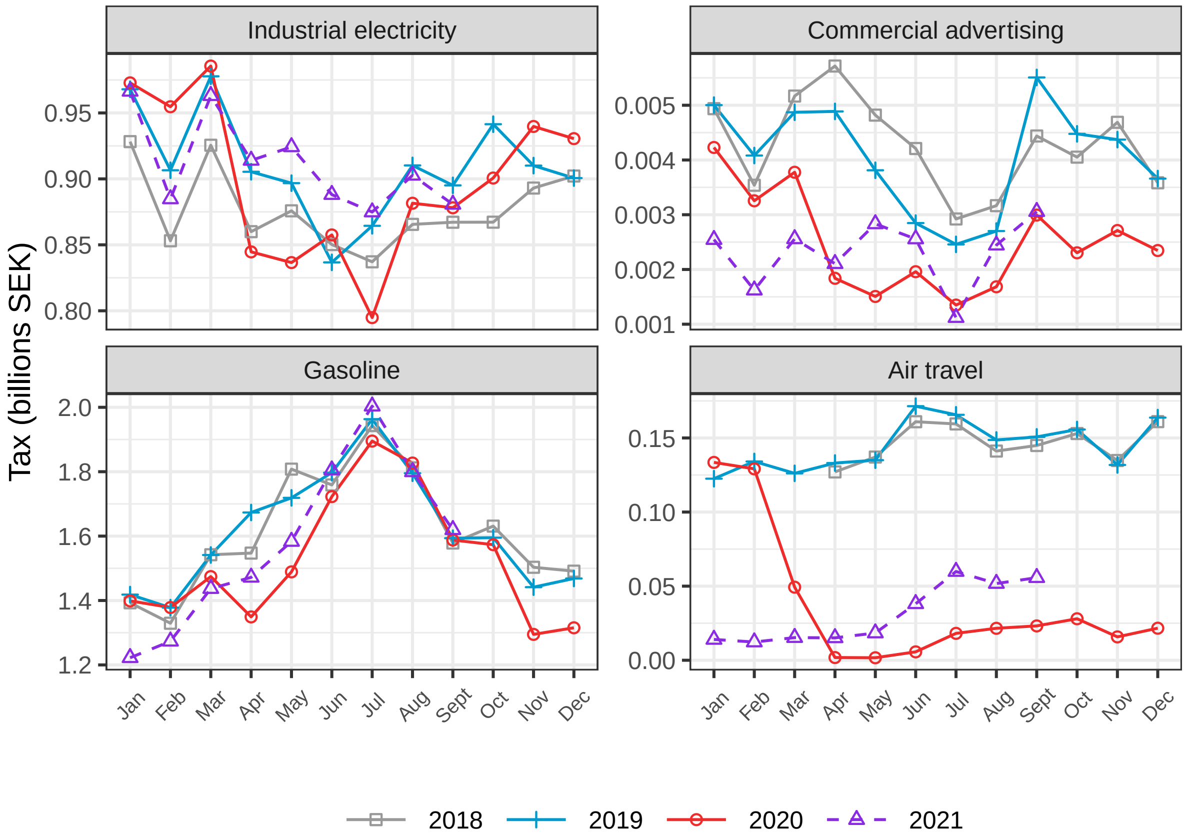
4.2 Excise taxes

Figure 4 shows the evolution of monthly revenues from four large excise taxes. The tax deductions for electricity usage among industrial firms offers an approximate measure of production activity in the private sector, have decreased during the pandemic and there is no apparent rebound during 2021. The tax on advertising expenses (printed adds, posters, signs etc.) dropped by almost half after the outbreak and has remained low ever since. The tax on car fuels, mainly gasoline, dropped notably in the first months into the pandemic, but has then been almost at the pre-pandemic level. By contrast, the air travel tax plummeted after Coronavirus outbreak, being almost zero in the first months and then increasing only slowly.

We estimate the pandemic effect on excise taxes using the same framework as above in equation (4).[15] Table 3 presents the estimated mean effect. All point estimates have the expected negative signs and the approximate percentage effects are all economically significant. The only statistically significant mean effects are for air travel and industrial electricity. For air travel, the estimated effect is -1,084,080 SEK and it is statistically significant at the 0.1 percent level. The percentage effect approximation (–95.84%) is imprecisely measured for such a high effect, but it is nevertheless clear that air travel was hit very hard by the pandemic. The point estimate for industrial electricity is –189,920 SEK or about –5.74%.

[15]A difference in the empirical specification compared here to the one in section A2 is that it is not feasible to cluster the standard errors on industry level. Although there is some variation in the industry affiliation of the firms that fill in and report tax declarations for a certain excise tax, there is likely a weak connection between firm industry and the industry of the actual consumption upon which the excise tax is based. Instead of clustering the standard errors on industry level, we cluster on firm level in order to take into consideration the within-firm correlation of observations measured in different months.

Figure 4: Excise tax revenues, 2018-2021.



Note: The figure shows total excise tax revenues on air travel, gasoline, advertisement and industrial electricity usage. We measure industrial electricity usage by tax deductions for electricity usage for industrial purposes. One SEK is approximately equal to 0.1 EUR.

Table 3: Fixed-effect estimates of the Covid-19-impact on excise taxes.

	(1) Air travel	(2) Gasoline	(3) Advertisement	(4) Industrial electricity
Covid impact	−1,084.07***	−1,911.74	−21.55	−189.92*
	(316.65)	(1,120.39)	(14.00)	(78.23)
Year 2020	−3.28	15.23	−16.53*	122.06*
	(89.97)	(383.44)	(7.61)	(54.29)
Mar-Dec	141.92	4 683.47*	−12.23	−104.30*
	(88.05)	(2 226.17)	(9.52)	(42.89)
Mean March-Dec 2019	1,131.11	29,410.62	78.25	3,305.94
Covid impact (%)	-95.84	-6.5	-27.54	-5.74
Observations	3,010	1,399	1,151	6,588
Adjusted R^2	0.67	0.98	0.64	0.99

Note: *p<0.05; **p<0.01; ***p<0.001. The table presents regressions using firm-level payments of four excise taxes in billions of SEK. Electricity usage for industrial purposes is measured by tax deductions for electricity usage for industrial purposes. One SEK is approximately equal to 0.1 EUR.

Table 4: Seasonal effects of the Covid-19-impact on excise taxes.

	(1) Air travel	(2) Gasoline	(3) Advertisement	(4) Industrial electricity
Covid Mar–May	−961.32**	−3 618.24	−30.33*	−170.49*
	(297.51)	(1,999.95)	(12.85)	(72.90)
Covid Jun–Aug	−1,217.65***	−1,068.84	−8.57	−271.47*
	(340.95)	(716.56)	(11.71)	(110.02)
Covid Sep–Dec	−1,075.62***	−1,348.68	−24.40	−143.79
	(315.97)	(872.20)	(20.39)	(73.23)
Year 2020	−2.82	−15.37	−16.65*	121.97*
	(90.14)	(377.26)	(7.63)	(54.29)
Mar-May	19.13	4,420.88*	−0.38	−42.39
	(88.85)	(2,145.17)	(10.36)	(43.76)
Jun-Aug	248.63*	8 243.70*	−38.30***	−190.27***
	(104.10)	(3,832.55)	(10.59)	(56.09)
Sep-Dec	154.27	2,286.79	−1.92	−86.76*
	(84.71)	(1,159.33)	(15.34)	(43.87)
Mean March-Dec 2019	1,131.11	29,410.62	78.25	3,305.94
Covid impact March − May (%)	-84.99	-12.3	-38.76	-5.16
Covid impact June − August (%)	-107.65	-3.63	-10.95	-8.21
Covid impact September − December (%)	-95.09	-4.59	-31.18	-4.35
Observations	3,010	1,399	1,151	6,588
Adjusted R^2	0.67	0.99	0.64	0.99

Note: *p<0.05; **p<0.01; ***p<0.001. The table presents regressions using firm-level payments of four excise taxes in billions of SEK (SEK/EUR≈ 0.1).

Table 4 presents the effect of the pandemic, estimated separately for three periods during 2020: March-May, June-August, and September-December.[16] As previously, all point estimates have the expected, negative, signs and are economically significant.

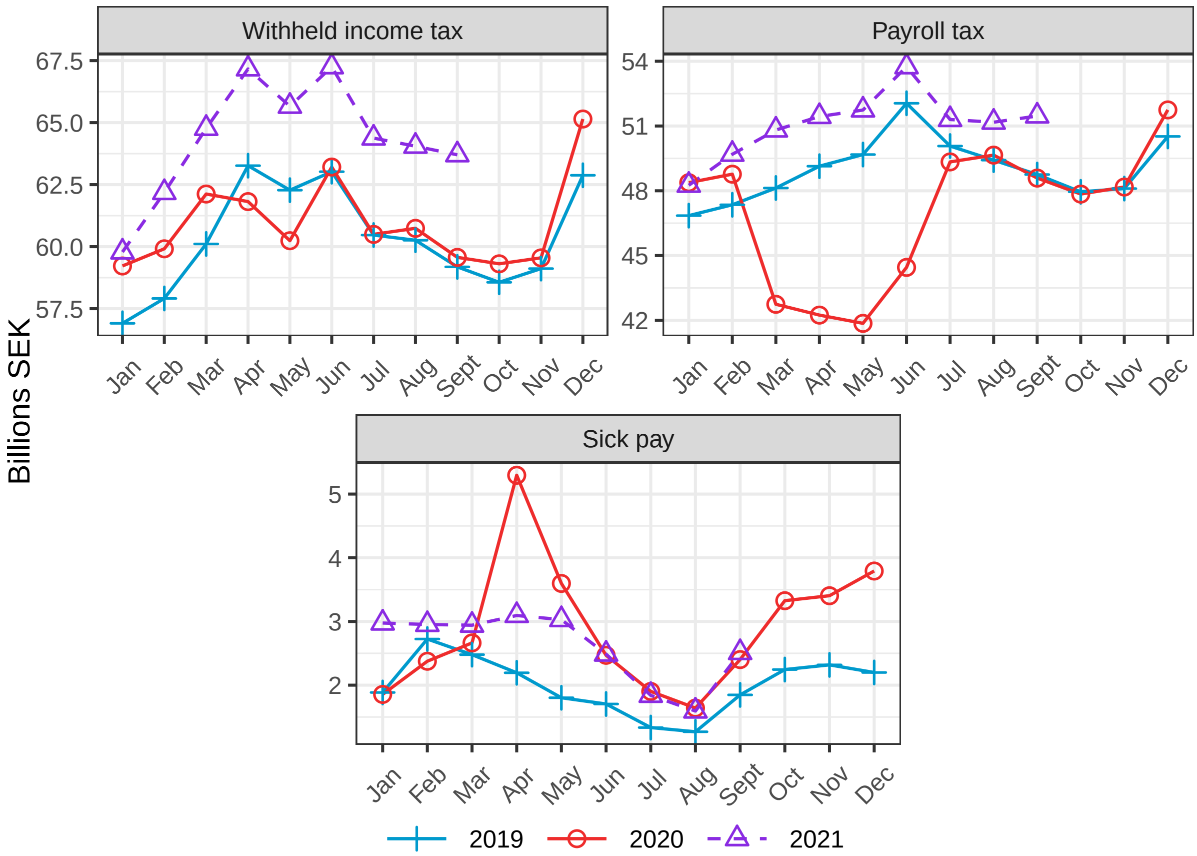
The largest negative effects are recorded for the air travel tax, with effects being large and negative in all three sub-periods. The largest effect is recorded for the summer period (–1,217,650 SEK), which most likely reflects the relatively more profound anti-pandemic policy measures on international rather than on national travel and the fact that this affected the most intense holiday season when long-distance traveling is the most common.

Turning to industrial electricity, the largest effect was during the summer months June-August (–271,470 SEK or –8.21%), but the effect was considerable also in March-May (-170,490 SEK or –5.16%). For advertisement, the effect is only statistically significant in the start of the pandemic (March-May) and large in magnitude: –30,330 SEK (–38.76%).

In table B2 in appendix A, we present placebo effect estimates estimated on data from 2018 and 2019 (as opposed to 2019 and 2020 in Table 3) and assuming 2019 was

[16] Note that due to the way data on excise taxes is collected, and also the characteristics of several of the taxes, it is not meaningful to interact the effect with local Covid infection rates as was done in section A2.

Figure 5: Labor tax revenues and sick pay, 2018-2021.



Note: The figure shows total tax revenues each month in billions of SEK (SEK/EUR≈ 0.1) for withheld municipal and central government labor income taxes, payroll taxes ("arbetsgivaravgifter") and sick pay.

the treatment year. It is not feasible to run the placebo test on the air travel tax since it was introduced in April 2018. Out of 15 placebo effects, one is found to be statistically significant at the five percent level (-18,401 SEK or -23.68% for advertisement in March–May). Although not an unreasonable outcome given the usual five percent significance level (which on average implies one false rejection of a true null out of 20 tests), the point estimate for advertisement in table B2 has the same size and roughly the same magnitude as the one in table 3. Therefore, our results on advertisement cannot be said to have passed the placebo test.

In summary, we find substantial negative effects of the pandemic on excise taxes for industrial electricity during the spring and summer of 2020, and a huge negative impact on air travel throughout the year. Advertisement appears to have been experiencing a strong ongoing negative trend even prior to the pandemic.

4.3 Labor income taxes and sick pay

Figure 5 displays the evolution of labor income taxes and of firms' sick pay costs. Withheld taxes fell after the virus outbreak but increased thereafter to 2019 levels. Payroll taxes decreased much more during March-June 2020 and rebounded thereafter. This drastic decrease is a combined effect of the reduction in the payroll tax rate (described in section 3) and the drop in employment during the downturn. Sick pay doubled during the first months into the pandemic, then fell somewhat toward the end of 2020. Mounting sickness incidence is a natural driver, but the behavioral responses to extended government support of employers' costs for sick pay and the removal of qualifying days for absent employees could also have contributed. Below, we examine the relative importance of these two channels by analyzing the temporal and geographical correlation between reported Coronavirus incidence and sick pay costs. Table 5 presents results from regressing using equation (4), showing both mean effects in columnes 1-3 and interacted effects using municipal infection rates as captured by the quartile group dummies $QG_1 - QG_3$ where the most infected municipalities QG_4 are the reference group.[17].

The mean effect on payroll taxes is –9,610 SEK, or –8.22%, and on withheld taxes –4,180 SEK, or –2.9%. Both effects are negative, as expected, and statistically significant. The relatively larger impact on payroll taxes is likely to a large extent explained by the payroll tax rate reduction in March-June 2020.[18] The interacted effect with respect to the Coronavirus infection rate in column 4 shows that the effect on payroll taxes is by far the largest among firms registered in municipalities with the largest infection rate, –10,730 SEK or –9.18% (this is the reference group). The interacted effect on withheld labor income taxes is also highest in municipalities with the highest infection rates, but the difference compared to less affected municipalities is not so large and estimated with less precision: while the mean effect on withheld labor income taxes is significant at the 1%-level (specification (2)), none of the effect estimates in the specification with interactions are significant.

Turning to sick pay, we find a large mean effect of the pandemic, 3,110 SEK, or 71.15%. Doubtlessly, the effect is related to Covid-morbidity, either actual or suspected, in which case people were advised not to go to work. However, the government's expanded compensation of firms' sick-leave costs and the removal of the two qualifying days for worker absence could potentially also have contributed to the effect through incentive effects on sickness reporting.

[17]Monthly data were not collected in the administrative registers prior to 2019 which makes us unable to run placebo tests similar to those in sections A2 and 4.2

[18]An additional partial contribution of the larger payroll tax decrease is the lower progressivity of payroll taxes in combination with the fact that mostly low-income earners have lost their jobs during the pandemic.

Table 5: Fixed-effect estimates of the Covid-19 impact on payroll taxes, withheld personal income taxes, and sick-leave pay.

	Mean effect			Interaction with municipal Covid infection rate		
	Payroll tax	Withheld income tax	Sick pay	Payroll tax	Withheld income tax	Sick pay
	(1)	(2)	(3)	(4)	(5)	(6)
$Covid\ impact$	-9.61^{***}	-4.18^{**}	3.11^{**}	-10.73^{***}	-5.23	3.27^{**}
	(0.80)	(1.61)	(0.95)	(0.97)	(2.75)	(1.00)
$Impact \times QG_3$				2.29^{***}	1.99	-0.31
				(0.58)	(2.76)	(0.47)
$Impact \times QG_2$				2.43^{***}	2.70	0.35
				(0.59)	(2.72)	(1.02)
$Impact \times QG_1$				3.59^{**}	3.66	-1.31
				(1.11)	(2.80)	(0.82)
$Year\ 2020$	2.73^{***}	4.91^{**}	-0.34^{*}	3.76^{***}	7.05^{*}	-0.26
	(0.67)	(1.69)	(0.14)	(0.89)	(2.82)	(0.16)
$March-Dec$	5.03^{***}	8.22^{***}	-0.83^{**}	5.51^{***}	9.95^{***}	-0.76^{**}
	(0.61)	(1.69)	(0.27)	(0.77)	(2.58)	(0.24)
QG_3				-1.56	1.43	0.38
				(1.63)	(2.85)	(0.27)
QG_2				1.46	3.76	0.29
				(2.07)	(3.01)	(0.34)
QG_1				-3.05	1.28	-0.03
				(2.62)	(2.82)	(0.38)
$Year\ 2020 \times QG_3$				-1.74^{*}	-4.16	-0.14
				(0.70)	(2.71)	(0.21)
$Year\ 2020 \times QG_2$				-1.81^{**}	-4.16	-0.17
				(0.66)	(2.57)	(0.19)
$Year\ 2020 \times QG_1$				-2.95^{**}	-6.63^{*}	-0.13
				(0.97)	(2.94)	(0.17)
$March-Dec \times QG_3$				-0.63	-3.62	-0.17
				(0.65)	(2.48)	(0.23)
$March-Dec \times QG_2$				-0.50	-3.60	-0.35
				(0.73)	(2.55)	(0.38)
$March-Dec \times QG_1$				-3.08^{**}	-5.71^{**}	0.15
				(1.01)	(1.98)	(0.34)
Mean Mar-Dec 2019	116.88	144.18	4.59	116.88	144.18	4.59
$Covid\ impact$ (%)	-8.22	-2.9	67.77	-9.18	-3.63	71.15
$Impact \times QG_3$ (%)				1.96	1.38	-6.78
$Impact \times QG_2$ (%)				2.08	1.87	7.57
$Impact \times QG_1$ (%)				3.07	2.54	-28.5
Observations	10,239,274	10,239,274	10,239,274	10,239,274	10,239,274	10,239,274
Adjusted R^2	0.99	1.00	0.75	0.99	1.00	0.75

Note: *p<0.05; **p<0.01; ***p<0.001. The table is based on firm-level payments each month of withheld municipal and central government labor income taxes, payroll taxes ("arbetsgivaravgifter") and sick pay. Billions of SEK (SEK/EUR≈ 0.1) for

There is a large research literature on the effect of incentives in the sickness insurance system.[19] Also, Swedish media has reported about hundreds of cases where firms

[19] See for instance Johansson and Palme (2005) who estimate the effect of the replacement level in the Swedish national sickness insurance system on work absence behavior or Böheim and Leoni (2020) who

without any wage payments before the pandemic suddenly reported full-wage sick pay during the first months during the pandemic. There are also reports about firms requiring their employees to report sick but keep on working, either at work or from home.[20]

To investigate these two potential mechanisms behind the large effect on sick pay, we interact the main Covid effect with the municipal infection incidence as captured by the four groups defined previously. If the effect on sick pay is primarily driven by Covid-morbidity, we should expect a lower effect among firms registered in municipalities with a low infection rate and vice versa. The results in the last column of table 5 show that only two out of the three groups with relatively lower infection intensity have less sick pay, and one of these groups even has a positive sign of the sick pay coefficient. None of the three interacted effects are statistically significant at the five-percent level.

It is intriguing that we cannot trace any clear effect-heterogeneity with respect to local Covid-infection rates in the outcome where such heterogeneity would seem most natural, namely sick pay. The reliability of the Covid-infection rate data could in principle be questioned, but is should be noted that it yielded expected and statistically significant results for VAT and sales in table 1. Furthermore, for the same data and sample as for sick pay, the Covid infection rate interaction yields reasonable and statistically significant results for payroll tax (model (4) in table 5). Therefore, we cannot exclude that a significant part of the estimated Covid impact on sick pay costs could be due to monetary incentives, created by the temporary government compensation scheme for firms' costs for sick workers, and not entirely due to actual Covid-related sickness among employees.

A part of the effect of the pandemic on sick could also be driven by precaution, rather than by having contracted Covid. Arguably, reporting sick as a precautionary measure could well be related to the temporarily changed incentives in the sickness insurance system, as the changed incentives made it easier to take such precautionary measures. However, if one believes that the main driver of the effect on sick leave is precautionary behavior, precaution should be more prevalent where the infection rate is higher. Therefore, our hypothesis is that changed monetary incentives (irrespective of own sickness or local infection rates) are probably a likely candidate as a major effect driver. Unfortunately, we cannot test this assertion using data from tax registers. To gain a more complete understanding of what is going on it would be useful to have sick-pay data on the individual level, which are not available in the tax registers.

study the effect of abolishing the refunding of firms' sick leave costs using Austrian data.

[20] See, for example, news from public service media, https://www.svt.se/nyheter/inrikes/foretag-fuskar-med-sjuklon-under-coronakrisen-300-arenden-har-stoppats-for-kontroll (2020-07-08) and https://sverigesradio.se/artikel/7616510 (2020-12-06).

5 Conclusion

Administrative tax registers offer a promising data source for measuring economic activity in the private sector. These registers are collected continuously, often at high frequencies, and offer full-population coverage with high precision at the firm- or individual levels. We used this data source obtained from Swedish registers to measure the economic impact of the Covid-19 pandemic in Sweden. While Sweden practiced initially a less restrictive policy to counter the pandemic, the country experienced similar Coronavirus outbreaks during 2020 as other developed countries did. For this reason, we believe that the methodological approach to use high-frequency tax register data to analyze private sector activities offer a useful data source for business cycle analysis as well as evaluating effects during times of economic turbulence.

Our main findings show that the pandemic had significant negative effects on both firm turnover in most sectors and on tax revenues. We identify the impact of the pandemic by exploiting both between-year trends and within-year variation and regional differences. Firm sales and VAT dropped around eight percent and seven percent, respectively, and the effect was more negative in areas hit harder by the Coronavirus. Excise tax revenues show that the revenues from the tax on air travel fell by almost 100 percent. The tax deductions for electricity usage for industrial purposes, which offers an alternative approach to capturing industrial output, fell around five percent as a result of the pandemic, which is close to the back-of-the envelope numbers on industrial production.

Firms' sick pay expenses soared during the pandemic, and we estimate an average pandemic effect of 71 percent during 2020. While this effect is expected, a puzzling fact is that the effect on sick pay correlates little with Covid-19 infection rates at the municipal level. This lack of correlation might reflect measurement errors, but another explanation is incentive responses among firms and workers following government measures to compensate firms for sick pay and abolish qualifying days for absent workers.

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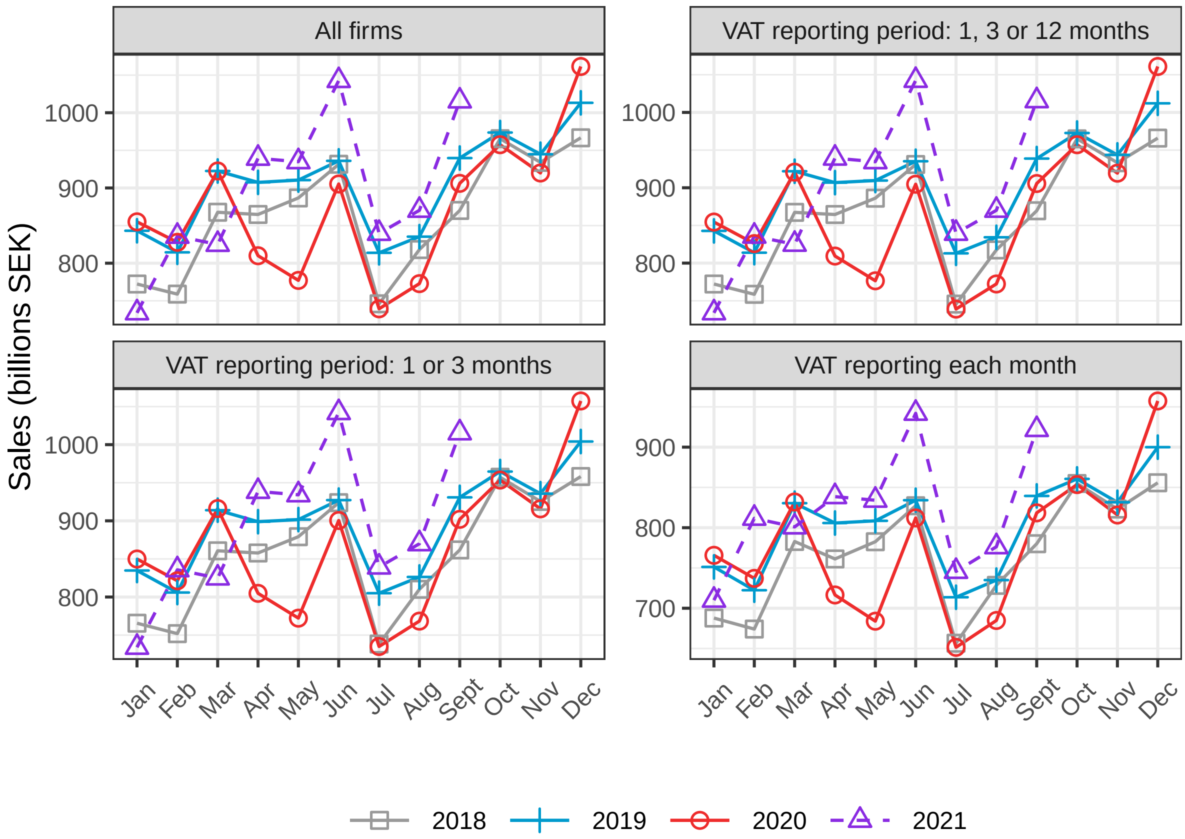
Appendices

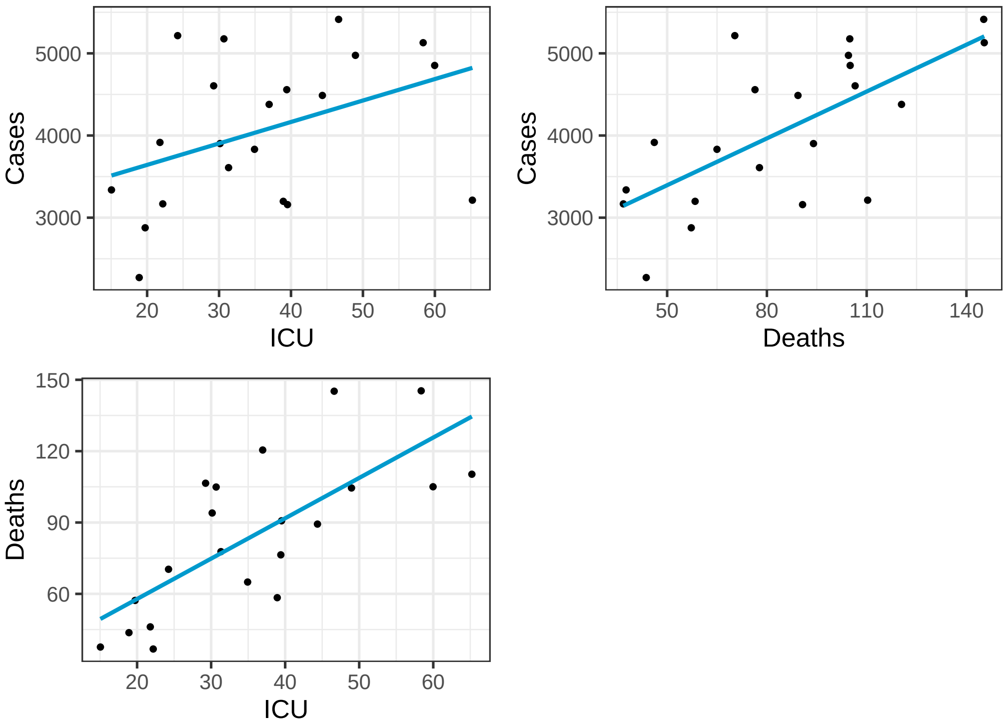
A Additional figures and tables

Figure A1: Firm turnover by reporting frequency, 2018-2021.



Note: Monthly turnover/sales of VAT-paying firms corresponds to the firm's total sales, including export and in some rare cases non-VAT eligible sales. SEK/EUR≈0.1. See section 3 for details.

Figure A2: Correlation between three different measures of Covid-spread.



Note: The graphs are based on data from the Swedish Public Health Agency. All measures are at the regional level and per 100,000 individuals. The numbers are calculated using the procedure described in detail in section , the only difference being the level of detail (regions instead of municipalities). Each data point corresponds to a particular measure summed from March to December, 2020 for a specific region (21 regions in total).

Table A1: Industrial and service production in Sweden: Impact of Covid-19.

	Industrial production	Service production
$Covid\ impact$	−5.6	−7.7
$\%\ (Base: March-Dec\ 2019)$	−5.0%	−6.9%
$Placebo\ (2019\ vs.\ 2018)$	−0.8	−1.5
$\%\ (Base: March-Dec\ 2018)$	−0.7%	−1.3%
$Triple-DD\ over\ calendar\ time$	−4.8	−6.2
$Triple-DD\ \%\ (Base: March-Dec\ 2019)$	−4.3%	−5.6%

Note: The $Covid\ impact$ is calculated as the difference between average index values in March-December and January-February 2020 divided by the same difference in 2019. The $Placebo$ is the same calculation one year before (2019 vs. 2018). The $Triple-DD$ is the difference between the two estimates above. Data on indexes over value-added in the industrial sector (manufacturing, mining, electicity) and service sector (trade, commerce, transport excluding finance) come from Statistics Sweden.

B Placebo analyses

Below, we present estimates of the Covid-impact for sales and VAT and a placebo analysis. The estimation of Covid-impacts on VAT and firm sales uses (4) and data on monthly VAT and sales in thousands SEK per month during 2019 and 2020. Results are presented in the first column of table B1. The estimated impact corresponds to θ_3 from (4) and shows an effect of -204,710 SEK (approximately 20,500 EUR) which is statistically significant at the one percent level. As it is quite common for firms to declare zero sales during a particular month, it is not feasible to log-transform the data for easier interpretation and comparison. Instead, we provide an approximate percentage effect interpretation by relating the effect to the mean of the outcome variable during the period from March to December 2019 (the row $Mean$, 3,357,250 SEK).[21] Doing this suggests a Covid-impact on firm sales of about –6.1%. The effect on VAT is negative, significant a the five percent level, but smaller in magnitude at about –3.4%.

In Section 2, we discussed the potential bias in the results due to the pre-pandemic slowdown in the economy. To measure the slowdown in terms of VAT and firm sales and provide an informal assessment of the parallel-trend assumption (3), we can perform a placebo analysis, since we have VAT and sales data for 2018. To this end, the third column of table B1 contains the placebo effect estimated using specification (4) but on data covering 2018 and 2019 and with the treatment group variable re-defined as $D_t = \mathbf{1}[t = 2019]$. The point estimate for the placebo effect is about -98,660 SEK, that is, of the same sign and about half in size compared to the effect estimate. Although not statistically significant on the usual 5 percent level, we are not convinced by this placebo analysis as the placebo effect is quite substantial. To further investigate, in specification (5) in table B1, we roll back the analysis one more year and estimate a placebo effect using data covering 2017 and 2018 with $D_t = \mathbf{1}[t = 2018]$. Now the point estimate of the placebo effect is positive and lower in magnitude (about 38 thousand SEK) and far from being significant on any reasonable significance level. This result could be interpreted as suggesting to estimate the effect on data from 2020 and 2018 (or even 2017), but that does not seem satisfactory for at least two reasons. First, the longer the distance between the treatment and control years, the harder it is to argue that the equivalent of assumption (3) holds. Besides the pandemic, more changes in the economy have arguably occurred between 2018 and 2020 than between 2019 and 2020. Second, using another year as a reference does not provide any clue as to the reason for the relatively large placebo effect using 2019 as the treatment year.

[21] An alternative would be to relate the effect to the mean during January and February 2019. This would have been equivalent to relating the effect to the intercept in a model without firm-fixed effects. In a previous version of the paper, we used Jan-Feb as a reference point which resulted in larger (in magnitude) effect for VAT and firm sales, some mixed-sign differences for excise taxes, and a lower effect size for sick leave. After a thoughtful comment from a referee, we changed the reference point to March-December since the within-year variation in the outcome variables is in general larger than the between-year variation. Thus, relating the effect measured during March-December 2020 to the level during March-December 2019 is more reasonable than comparing to January-December 2019.

Table B1: Fixed-effect estimates of the COVID-19-impact on firm sales and value-added taxes: Choice of empirical specification.

	Effect estimate Full sample		Placebo 2019 Full sample		Placebo 2018 Full sample		Placebo 2019 No outliers	
	(1) Sales	(2) VAT	(3) Sales	(4) VAT	(5) Sales	(6) VAT	(7) Sales	(8) VAT
Covid impact	−204.71**	−16.26*	−98.66	−9.16	38.49	−1.60	0.43	−0.10
	(68.47)	(6.56)	(55.83)	(6.59)	(36.77)	(3.76)	(1.64)	(0.23)
Year 2020	24.49	3.25	228.89**	23.08**	176.73***	25.67***	2.18	0.59**
	(54.12)	(6.34)	(72.89)	(7.88)	(44.86)	(7.48)	(1.45)	(0.20)
March − Dec	316.39**	50.54**	425.25***	60.93***	394.24***	63.48***	50.35***	9.26***
	(96.29)	(15.82)	(98.87)	(15.11)	(92.33)	(16.79)	(5.81)	(0.88)
Mean	3,357.25	478.15	3,235.35	463.84	3,020.03	439.48	236.96	44.38
Covid impact (%)	-6.1	-3.4	-3.05	-1.97	1.27	-0.36	0.18	-0.22
Observations	5,827,645	5,827,645	5,831,611	5,831,611	5,822,419	5,822,419	5,195,583	5,195,583
Adjusted R^2	0.87	0.94	0.91	0.94	0.92	0.94	0.96	0.79

Note: *p<0.05; **p<0.01; ***p<0.001. The table presents regressions using firm-level records on monthly VAT payments and total sales in billions of SEK (SEK/EUR≈ 0.1). Notice that in the placebo regressions (columns 5 and 6), variables used are the year 2019 and the "placebo-Covid impact".

The results from the placebo analysis indicate that the year 2019 stands out somehow in relation to 2018 and 2017. When studying firm sales, it is not unreasonable to believe that there are instances of monthly outliers, for example, reporting a very big order, that can distort the results. To test this conjecture, in specification (7) in table B1, we have once more estimated a placebo effect on data covering 2018 and 2019 with 2019 as treatment year, but this time on a sample where outliers have been removed on a monthly basis. During a particular month, an outlier is defined as a value that lies above $Q_3 + 3(Q_3 - Q_1)$, where Q_1 and Q_3 are the first and third quartiles, respectively.We get similar results when we remove outliers defined as any value above the 95th percentile of the firm sales distribution during a particular month. It is more common to see a factor of 1.5 instead of 3, but we have chosen the latter in order to keep a larger share of the population in the sample. Removing outliers in this manner appears to render the placebo effect estimates insignificant, both in economic and statistical terms. The point estimate for firm sales transformed to a percentage effect falls dramatically in magnitude and changes sign (from –3.05% for the full sample to 0.18% when outliers are removed). We find this result more convincing than the placebo for the full sample assuming 2018 is the treatment year (specification (5)). Thus, on data with the outliers removed, the placebo test provides informal support that the parallel trends assumption holds. Although we have not mentioned VAT as outcome variable, it is evident from the results in Table B1 that removing outliers is needed for VAT also. Consequently, for both VAT and sales, we remove outliers in the subsequent regression analysis.

The results in table B2 below are mentioned in the main text.

Table B2: Placebo effect estimates of the Covid-19-impact on excise taxes: Seasonal effects with 2019 assumed to be the treatment year.

	(1) Gasoline	(2) Advertisement	(3) Industrial electricity
Covid impact March – May	−935.07	−18.41*	4.13
	(710.82)	(8.53)	(62.34)
Covid impact June – August	−962.44	−23.47	12.75
	(867.27)	(12.38)	(68.68)
Covid impact September – December	−1,612.72	−0.82	19.69
	(838.30)	(10.12)	(63.50)
Year 2020	1,171.83	7.56	89.06*
	(1,066.91)	(8.81)	(38.97)
March – May	5,205.74*	18.06	−46.13
	(2,372.55)	(12.21)	(39.58)
June – August	9,040.52*	−14.66	−203.73***
	(4,166.87)	(12.64)	(53.42)
September – December	3,741.54*	−0.93	−97.50*
	(1,756.58)	(13.34)	(43.14)
Mean March-Dec 2018	30,486.81	77.75	3,389.01
Covid impact March – May (%)	-3.07	-23.68	0.12
Covid impact June – August (%)	-3.16	-30.18	0.38
Covid impact September – December (%)	-5.29	-1.06	0.58
Observations	1,330	1,272	6,330
Adjusted R^2	0.99	0.73	0.99
Residual Std. Error	11 717.03 (df = 1256)	91.53 (df = 1182)	922.38 (df = 6015)

Note: *p<0.05; **p<0.01; ***p<0.001. The table presents regressions using firm-level payments of four excise taxes in billions of SEK (SEK/EUR≈ 0.1).