How much does mobility matter for value-added tax revenue? Cross-country evidence around COVID-19

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Abstract

This paper studies to what extent mobility reductions and confinement measures impact value-added tax (VAT) collection, which is an increasingly important type of fiscal revenue around the world. Using evidence across twenty nations and over time, we measure these effects around the COVID-19 Pandemic. For that, we benefit from the novel IDB-CIAT *monthly* dataset on aggregate VAT revenues (2019–2020), combining it with both mobility-restriction policies and mobility outcomes. On average, monthly VAT revenues fell up to 30% around the event of the largest drop in mobility for each country. We also estimate mobility elasticities of VAT revenue. Mobility-restriction policies rising by 10% were associated with drops in VAT of 1.4%, while a 10% drop in actual mobility decreased VAT revenues by 3%. Furthermore, we show both elasticities were significantly smaller in the last quarter of 2020. Beyond the pandemic, results matter as a benchmark for fiscal and macroeconomic variables under large disruptions.

1 Introduction

The global pandemic of COVID-19 generated fiscal challenges. Some of them stemmed from non-pharmaceutical interventions (NPIs), especially policies like lockdowns that limit citizens' mobility. These interventions may have health and

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welfare effects, but also challenge tax collection. In practice, countries need to assess both costs and benefits of these policies when mandating mobility restrictions for their population.[1]

Our contribution in this paper is to measure the effect of mobility reduction on value-added tax (VAT) collection. We do so for twenty countries, combining new IDB-CIAT data on monthly VAT revenues with measures of mobility. On the one hand, we use mobility-restriction policies, namely the stringency index in Oxford Government Response Tracker (OxCGRT). On the other hand, to track actual mobility, we use the index made by Google. We find that both mobility-restriction policies and mobility outcomes had a significant effect on VAT collection.

We find that monthly VAT revenues fell up to 30% in the month following the largest fall in mobility for each country. But this goes beyond averages. Even the visual inspection for each country displays a clear joint drop in VAT and mobility, as shown in panel (A) in Fig. 1. Estimated elasticities imply that for each 10% rise in mobility-restriction policies, VAT revenues drop by 1.4%. As expected, for actual mobility outcomes, the elasticity is even stronger, since a 10% rise in mobility decreases VAT revenues by 3%. We also document that both effects were significantly smaller in magnitude over the last quarter of 2020, coherent with some adaptation in the system.

Our study relates to various strands of the literature. An important one is how mobility-induced recessions (e.g., COVID-19) impact government revenues and fiscal policy (see Agrawal & Bütikofer, 2022; Bitler et al., 2022; Dean et al., 2023; Cui et al., 2022).[2] For some tax revenues, neither the magnitudes nor the signs of the response has been obvious. For example, Dean et al. (2023) argue that despite the reasonable ex ante predictions of a drop in US state-level sales tax around COVID-19 (e.g., Clemens & Veuger, 2020), these revenues did not fall in 2020. In fact, three-quarters of US States experienced such a year-on-year increase. Moreover, they argue that almost a third of OECD countries increased their revenues from taxes on goods and services in 2020.[3]

On the other hand, Kim et al. (2020) use transaction-level data for the USA to document a 40% decline in sales revenues for small businesses, as well as a similar drop in consumption of their owners, after the onset of the Pandemic. Tax payment increased for the immediate week after the onset of restrictions, and then fell. Using US state data, Clemens and Veuger (2020) ex ante estimate the exposure of state-level sales and income tax revenues to mobility restrictions in the Pandemic. Further down the sub-national level, Asahi et al., (2021) estimate a drop in VAT collection in Chile, during months of municipality faced strict lockdowns.[4] Our contribution is

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[1] Economies may face other meaningful disruptions due to other causes like social unrest (Hadzi-Vaskov et al., 2023) which may matter for many macroeconomic dimensions, including fiscal revenues.

[2] Some papers explored beliefs supporting new taxes, as Olivera and Van Kerm (2022)

[3] In the more in-depth part of their study, Dean et al. (2023) explore the case of the US State of Utah, finding that part of the response was due to shifts in consumption patterns from services toward goods which enter the sales tax-base.

[4] Castex et al. (2021); Yeyati and Sartorio (2020) study the effectiveness and determinants of compliance with mobility restrictions.

to extend this panel-data analysis at the national level, across 20 countries, as to find the aggregate-level average response of revenues.

Also using cross-country panel regressions, Chudik et al. (2021) study a different fiscal relationship, namely how COVID-19 fiscal stimuli impacted macroeconomic recovery. Unlike their work, we focus on the revenue side and only on the first ten months of the pandemic, as to avoid most confounding effects. Our study also controls for the fact that some countries may have changed VAT taxes during the pandemic. Any quick changes may have been fast responses by governments and not necessarily reflecting societal preferences for types of taxes as in Olivera and Van Kerm (2022).[5] Our work is more cyclical, around a massive disruption, and in that sense complements existing studies that look at the more permanent impacts of VAT collection and its effects (Mudiyanselage & Chen, 2022; Acosta-Ormaechea & Morozumi, 2021.[6]

The rest of the paper is organized as follows. Section 2 displays data and visualizes the basic facts. Section 3 presents our empirical strategy and estimations, including robustness tests for alternative interpretations, as well as tests for heterogeneity over time and across countries. Finally, Sect. 4 concludes. Additional tests are left to the Online Appendix.

2 Data and stylized facts

2.1 Data

Here, we describe the sources and trends of our cross-country data on VAT revenues and mobility. We use monthly data from 2019–2020 for a total of 20 countries. The complete list of countries is available in Appendix B.1. A great advantage of this data is that it is "high frequency," which helps better capture the short-run dynamics. Data at higher levels of temporal aggregation may reflect relatively more other macroeconomic dynamics (e.g., feedback loop from pandemic to fiscal expenditure to more consumption), unlike the focus of our research question.

For tax revenues (VAT), we use the monthly data on revenues converted to 12-month percentage changes at constant prices. The data comes from the Revenue Report COVID-19 by the joint effort of the Inter-American Center of Tax Administrations (CIAT) and the Inter-American Development Bank (IDB).[7] We exclude Trinidad and Tobago from our sample because it is only available at current prices and Bolivia because the series for VAT revenues are incomplete. (There is no data for the second semester.)

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[5] They use survey data in Luxembourg to study attitudes toward different tax policies in response to the pandemic.

[6] Mudiyanselage and Chen (2022) provides cross-country evidence for 127 countries of the effect of introducing VAT to aggregate revenues over GDP. It studies how the introduction of VAT affects tax over GDP. Acosta-Ormaechea and Morozumi (2021) uses data from 21 OECD economies, exploring various revenue-neutral tax reforms.

[7] *Reporte de Recaudación Covid-19 CIAT* in Spanish (see link)

Our mobility-related variables come from two main sources. One for policy-based restrictions and the other for the effective mobility of citizens.

On the one hand, we use the so-called Stringency Index, which is a measure of how restrictive were the policies related to mobility. The source is Oxford COVID-19 Government Response Tracker (OxCGRT). The stringency index goes from 0 to 100, where 0 denotes no restrictions and 100 means the strictest mobility constraints. The index is computed considering nine response indicators including school closures, workplace closures, and travel bans. Also, if policies vary within country, the index is shown as the response level of the strictest sub-region.[8,9]

On the other hand, we use actual mobility indices coming from cellphone usage. That is daily data from the Google report on mobility, which compares visits to specific places (e.g., retails, workplace, etc.) benchmarked to a weekday average on the pre-pandemic visits. The difference is expressed as percentage change.[10,11] They use the period from January 3 to February 6 to create the weekday reference averages.[12]

Given that our data in VAT collection is at a country-month level, we collapse the data to that same level, creating mean indicators of mobility. In other words, consider the indicators $m_{j,c,\tau}^i$, expressed as percentage change, where i denotes the indicator, j the sub-region within the country c in the date τ. Then, our average indicator $M_{c,t}$ in the country c and the month t is given by:

$$M_{c,t} = 1 - \frac{1}{IJT} \sum_i \sum_j \sum_\tau m_{j,c,\tau}^i, \tag{1}$$

where I denotes the number of indices, J the number of sub-regions and T the number of days in the month t. We compute the indicator as one minus the average across indicators and time simply for convenience, as it is expressed as a percentage of pre-pandemic mobility.

Table 1 displays descriptive statistics for the main variables in our sample. Part of our story is already visible in the raw statistics. On the revenue side, there is an

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[8] Our World in Data has information regarding regional stringency which we could use to create an average measure of stringency at a country level. However, these data are only available for Brazil among the countries we have VAT data, so we stick to the national-level measure.

[9] Regarding the use of the strictest stringency within country, as a measure of aggregate stringency, poses a threat to our baseline estimations as may dilute the coefficient toward zero. This is because we will consider the effect of the strictest region stringency over the aggregate VAT collection. Further, this caveat is increasing in the within-country heterogeneity, which is particularly important in countries with local level restrictions (e.g., Chile, Uruguay) or federal countries (e.g., Brazil). Table B.5 in Appendix provides descriptive statistics using the full sample and then breaking the sample by countries implementing national or targeted policies. From this table, note that countries with targeted policies present a higher average stringency index, as it captures the highest level of the variable within a country. In our empirical specifications, we exclude countries with targeted policies from the sample to check how it affects our results.

[10] www.google.com/covid19/mobility

[11] These aggregated data are from mobile device location history reported for six location categories: retail and recreation, grocery and pharmacy, parks, public transit, workplaces and residential areas.

[12] Therefore, unlike the stringency index data are only available since February 2020 as January acts as the reference period.

average drop of 8.8% in VAT collection, with respect to the same month of 2019. On the other hand, average mobility during 2020 was roughly 28% lower than the pre-pandemic benchmark. This relates to the fact that we had basically no mobility restrictions prior to February or early March 2020 but then implemented strict confinement measures, especially during the second quarter of 2020. The average stringency index was 58/100, substantially above the virtually zero value before the Covid-19 outbreak. The rest of the paper will dig deeper into the intra-year dynamics of this relationship. Table 1 also adds summary statistics for (log) output pre-pandemic as a reference. In Appendix, Table B.4 reports descriptive statistics for VAT as a share of output and general government revenues from the IMF, though for a far more restricted sample of countries.

2.2 Stylized facts

Figure 1 shows the evolution of our main variables over time. Panel A shows the monthly evolution country-by-country. Eyeballing the series, it becomes evident how mobility dropped, jointly with VAT tax collection.[13] It is also noteworthy that the dynamics of this raw data are remarkably similar across countries. This is reassuring, meaning that our averages and estimates in the rest of the paper come from a relatively similar phenomenon across more than twenty nations.

Panels (B) and (C) (Fig. 1) perform a simple event study analysis, taking the average across countries. Since neither the pandemic nor the mobility reductions came to all countries at the same time, we define the time = 0 in the event study as the month with greater absolute change (in log points) of mobility restrictions (Panel B) and aggregate mobility (Panel C).[14] As one can see from both pictures, the dynamics of VAT revenues seem closely linked to mobility and its policy restrictions, especially in the 3–6 months since the event under study. Figure 2 displays the same information but as month-over-month changes instead of changes vis-a-vis 12 months ago.

Moreover, using yearly changes, we can also compare our findings with year-over-year changes reported by Dean et al. (2023). We first replicate that study, which explores revenues from tax on goods and services for OECD countries, not only VAT. This replication allows us to compare the relations with a different set of countries, noting that there is little country overlapping between the two papers (six out of our 20 countries are shared). Figure 3 displays both the results of our y-o-y differences as well as the replicated one. Panel (A) shows a negative correlation between tax revenues and the stringency index. The relation is statistically the same for both subsamples, with slopes of −0.34 and −0.39, respectively. Panel (B) then shows that in both cases, there is a positive correlation between mobility and tax revenues, although there are significant differences across samples. In particular, note that the slope between tax revenue and mobility is higher in our sample (slopes of 0.94 and

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[13] See Table B.2 for maximum change in both mobility variables for each country in our sample.

[14] Figures C.1 present panels B and C but using a weighted average (rather than simple average), using population as weights. Figure C.2 presents the evolution of specific indices of mobility.

0.21 respectively). Section 3.5 will explore potential features that may be behind this heterogeneity and the differences with Dean et al. (2023).

3 Empirical strategy and findings

3.1 Regression model

Our research question explores how mobility restrictions and effective mobility relate to changes in VAT collection. In particular, we will estimate the following linear panel OLS model

$$\Delta VAT_{c,t}^{y-o-y} = \mu_c + \lambda_t + \beta MOBIL_{c,t} + \mathbf{X}_{c,t}\gamma' + \varepsilon_{c,t} \qquad (2)$$

where $\Delta VAT_{c,t}^{y-o-y}$ denotes the year-over-year percentage change in VAT revenues for country c in month t. That means a 12-month lagged difference to take care of seasonality. On the other hand, $MOBIL_{c,t} = \{P_{c,t}, M_{c,t}\}$ denotes either of our two right-hand side variables of interest regarding mobility (estimated separately). One is the policy stringency index and the other average mobility indicator, respectively. $\mathbf{X}_{c,t}$ denotes a vector of additional covariates, μ_c and λ_t are country and period fixed effects and $\varepsilon_{c,t}$ is the error term.[15] Note that here we are not controlling for aggregate consumption. This is because the effect of mobility on VAT comes through consumption and thus including it would be a so-called "bad control."

3.2 Baseline results

Table 2 presents our baseline results.[16] Note that a 10% higher policy restrictions (i.e., higher stringency index) reduces VAT revenues by 13.5% (p-value 0.015), though the magnitude of the point estimate is less than half of the effect of mobility in VAT collection, which is 29.6% in column 4 (p-value 0.001). This relation is somewhat the same when restricting the sample only to observations that have only national-level mobility-restriction policies.

Note that specifications (2), (3), (5), and (6) control by Covid cases and deaths. Not including them may potentially bias our results. In fact, Yang et al. (2020) argue that people respond to COVID even before government policies come into effect, for example, because they perceive threats from the incidence or mortality of the disease.[17]

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[15] One aspect corrected by the μ_c is, for example, the general difference across countries in the capacity of tax administrations to enforce VAT, which tends to be lower in poorer countries. See Mascagni et al. (2023)

[16] See Appendix B for additional results. Table B.6 presents the results using only countries from Latin America and The Caribbean. Table B.9 presents results excluding parks and residential indices from average mobility. Table B.10.

[17] Here, we use disease prevalence as in the usual public health statistics. To be more precise, one needs settings in which testing is randomized, as illustrated by Gaulin et al. (2023).

Table 1 Summary statistics

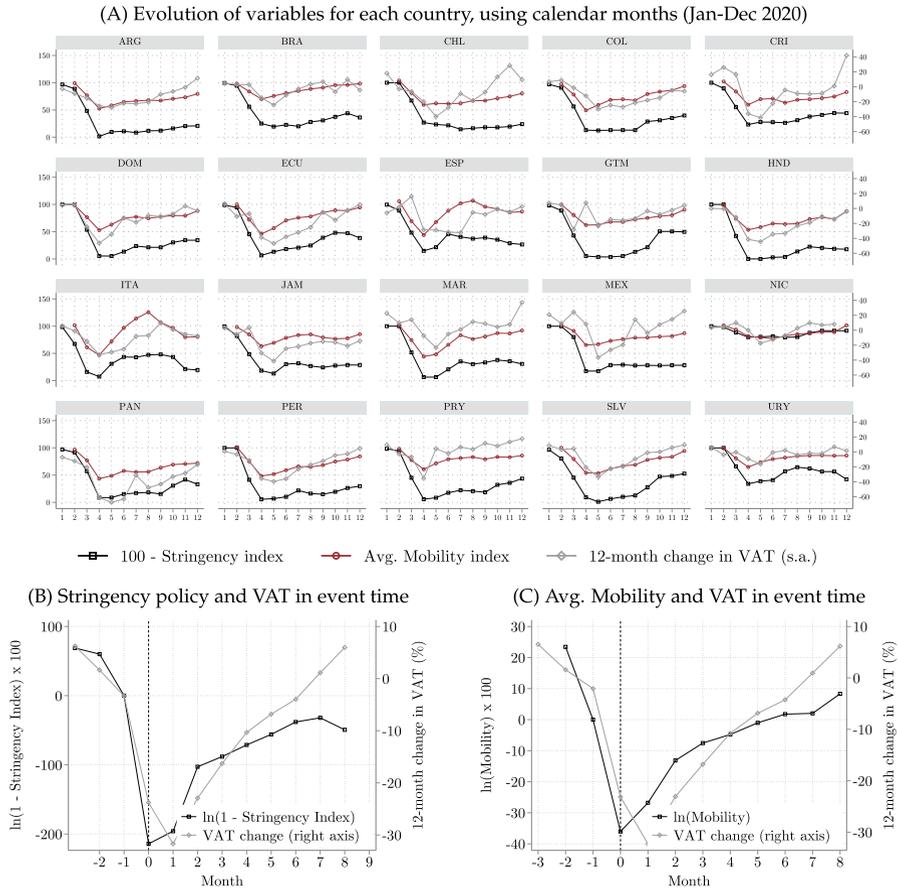|	Mean	SD	Min	Max	Obs.
VAT change	−8.79	17.73	−67.50	41.90	239
Stringency Index	58.48	30.52	0.00	100.00	240
Average mobility	77.75	14.66	43.54	125.63	220
New deaths per 100,000	0.19	0.30	0.00	1.78	240
New cases per 100,000	6.03	8.62	0.00	61.72	240
Real GDP (in logs)	25.65	1.54	23.30	28.28	240

This table reports descriptive statistics for our sample of 20 countries (see Table B.1). Data are available at a monthly level for the year 2020. Table B.5 presents descriptive statistics for different samples. Changes in VAT revenues come from IDB-CIAT, expressed as year-over-year percentage changes. The stringency index, average mobility, new deaths and new cases are presented as monthly averages. The stringency index goes from 0 to 100 where 100 denotes the strictest confinement measures. Deaths and new cases are shown as deaths/new cases per 100,000 using population data from the World Bank. Average mobility from the Google mobility report and expressed as a percentage of the pre-pandemic mobility, using January 2020 as the reference period (see Eq. (1)). Real GDP (2015 USD) presents cross-sectional output for 2018 as a reference

The magnitude of the effect of mobility restrictions on VAT revenues is in the ballpark of other empirical estimates. For example, Asahi et al. (2021) finds that one month of lockdown decreases monthly VAT by 12.5%, while our results indicate that going from no restrictions to a stringency index of 0.83 (average increase in our sample from January to April) decreases VAT in 11.2%,[18] roughly a 0.5% of GDP considering the average VAT revenues over GDP of 4.6% in our sample.[19] Likewise, Appendix A uses a simple mediation analysis (Baron & Kenny, 1986) to show that most of the effect of policy on VAT comes from a reduction in mobility. This result can be interpreted as a *forced* intertemporal substitution effect, where households delay consumption due to mobility restrictions. Following that interpretation, our results are somewhat similar to the ones in Bachmann et al. (2021), whereby exploiting variation on households' information on VAT rate path after a tax cut, they estimate an intertemporal substitution effect of roughly 10%. Also, the mobility-related elasticities in Table 2, which we estimate with cross-country data, is not far away from estimates of activity in Japan during the same period, where a 10% lower mobility induced 2.8% lower firm-level sales (Kawaguchi et al., 2022).[20]

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[18] $(-0.135 \times 0.83) \times 100$

[19] Source: IMF World Economic Outlook and Government Finance Statistics. We use 2018 as the reference year for this average.

[20] Kawaguchi et al. (2022) find that show how mobility restrictions during the recent pandemic reduced sales of Japanese firms but also labor hours. A 10 percent reduction in mobility led also to a 2.1% reduction in hours worked.

(A) Evolution of variables for each country, using calendar months (Jan-Dec 2020)

(B) Stringency policy and VAT in event time

(C) Avg. Mobility and VAT in event time

Fig. 1 Evolution of VAT collection, mobility restrictions and mobility during 2020. *Notes*: VAT changes from CIAT and expressed as percent variation with respect to VAT collected in 2020 relative to the same month in 2019. Stringency index data from OxCGRT and expressed as a monthly average. The index goes from 0 to 100 where 100 denotes the strictest mobility restrictions. For illustrative purposes, this figure reports $\log(1 - \text{stringency index}) \times 100$. The average mobility index is computed following Eq. (1), and it is expressed relative to pre-pandemic mobility, normalizing the latter to 100%. Given that January 2020 is used as the reference period, data are only available from February 2020 onward. To see the country name for a specific three-letter code, see Table B.1. Panels B and C present and event study analysis defining the event as the highest change in the mobility variable (stringency or avg. mobility). Both measures are presented in logs and normalized so that the month before the shock (month = −1) is zero

3.3 Robustness tests dealing with potential concerns

Now, we test for potential concerns regarding our basic results, namely the relationship between mobility and VAT. Overall, these basic results remain qualitatively robust to various alternative tests.

One first potential concern could be measurement error in the mobility measures, coming from aggregation in some of the countries. As mentioned, our base analysis

(A) m-o-m change in Stringency policy and VAT   (B) m-o-m change in Mobility and VAT
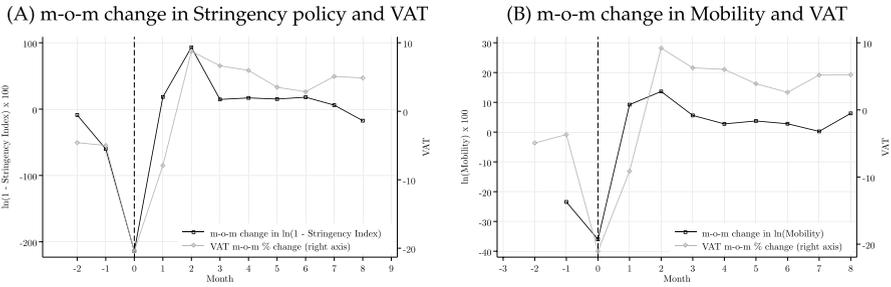
Fig. 2 Month-over-month changes of VAT, restrictions, and mobility during 2020. *Notes*: This figure closely follows Fig. 1 panels (**B**) and (**C**), respectively. The difference is that panels (**A**) and (**B**) here display data in terms of month-on-month changes instead of changes vis-á-vis 12 months ago. Our actual measure of m-o-m change is not raw, but corrected by the same difference in 2019, so we can get a proxy for a "seasonally adjusted" change m-o-m

(A) Stringency index   (B) Avg. Mobility index
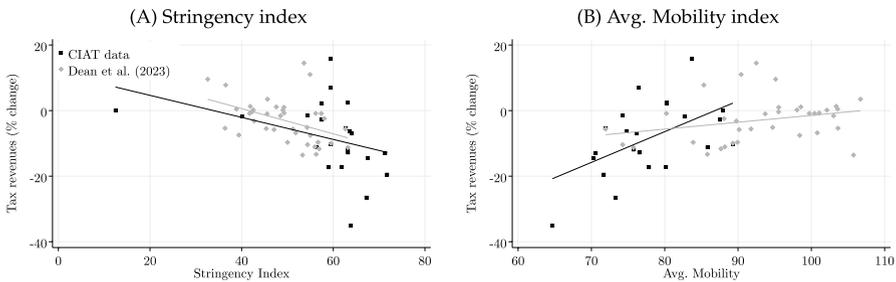
Fig. 3 Tax revenues, mobility restrictions and mobility comparing our study to Dean et al. (2023). *Notes*: This figure presents the relationship between tax revenues and mobility or mobility restrictions. Panel (**A**) uses the stringency index in the horizontal axis. Data for the stringency index from OxCGRT and expressed as a yearly average for the year 2020. Panel (**B**) uses average mobility, computed using Eq. 1, and it is expressed relative to pre-pandemic mobility, normalizing the latter to 100%. On the vertical axis, squared dots represent year-over-year percentage changes in VAT using data from CIAT. Gray diamonds reproduce results from Dean et al. (2023) using tax revenues on goods and services from OECD, also in year-over-year percentage changes

considers the strictest policy within a country to build the aggregate stringency index. This measurement error in the right-hand side variable may drive coefficients toward zero. To test for that concern, Columns (3) and (6) in Table 2 compare baseline results with a sub-sample that only considers national-level policy observations.[21] In that sub-sample, the measurement error is arguably smaller. It turns out that, as expected, the coefficients increase for both policy restrictions and mobility,

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[21] OxCGRT tags observations that are considered targeted policies (e.g., that apply to only a specific region or a country). We also compute an alternative sample dropping all observations tagged as "targeted policies" before computing monthly averages of the stringency index. Table B.5, panels B and C show descriptive statistics for the national and targeted samples.

Table 2 Baseline results

	Dep. Variable: 12-month change in VAT					
	Baseline	Add. Controls	Alt. Sample	Baseline	Add. Controls	Alt. Sample
	(1)	(2)	(3)	(4)	(5)	(6)
Stringency index	−0.135**	−0.105*	−0.257***			
	(0.055)	(0.060)	(0.060)			
Avg. Mobility				0.296***	0.292***	0.485***
				(0.089)	(0.099)	(0.130)
New deaths per 100,000		−0.021	−0.035		0.012	0.024
		(0.026)	(0.040)		(0.026)	(0.039)
New cases per 100,000		−0.001	−0.001		−0.001	−0.001
		(0.001)	(0.001)		(0.001)	(0.001)
Observations	239	239	166	219	219	146
Countries	20	20	20	20	20	20
Adj. R^2	0.72	0.72	0.75	0.72	0.72	0.74
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports estimates from Eq. (2)(2) for the year 2020. Data on VAT changes from CIAT report on Covid-19 and expressed as a fraction. Dependent variable denotes changes in VAT revenues relative to the same month in the previous year. Avg. mobility is computed as a simple average of all mobility indicators and expressed as a percentage of pre-pandemic mobility. All mobility indicators represent country-month averages of mobility. For more detail on the construction of these measures, see Sect. 2. Stringency index, new deaths per 100,000 and new cases per 100,000 data from OxCGRT are computed as a monthly average. The alternative sample in columns (3) and (6) restricts observations to countries that implemented country-wide mobility restrictions. To see the full sample of countries, see Table B.1. For results using only countries from LCN, please refer to B.6. All specifications include country and time fixed effects. Robust standard errors are presented in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level

though the direction of the effect and the difference across regressions remain somewhat consistent.

There is an extra aspect to note in our stronger results for columns (3) and (6) of Table 2, with the alternative sample of countries. These may initially seem contradictory to papers that argue that *local* mobility restrictions tend to be stronger at depressing economic activity (e.g., Seegert et al., 2020). This would be, for example, because a local restriction may signal to citizens that the disease is getting worse around them. Having said that, we believe, that our results are not necessarily at odds with the ones in Seegert et al. (2020). They argue that mask mandates affect mobility as they provide a signal to people about contagion risk. This signal is also weaker in state-level mandates as it provides less information to individuals about such risk. In our setting, the stringency index can also provide a signal for individuals beyond the mobility restrictions. However, in the way the policy variable is computed, the stringency in countries with national mobility restrictions is a better representation of local mobility across the country. This is because, in countries with local mobility restrictions, the value of the stringency index denotes the

maximum stringency index within the country. Regarding our stronger result for the mobility regression (i.e., specification 6), maybe we are picking a different behavior than Seegert et al. (2020). But overall, our main result is a relevant mobility-to-VAT elasticity.

There are other two potential econometric concerns in the econometric use of two-way fixed effects and clustering. Regarding the simultaneous use of both time and country fixed effects, Table B.7 shows that results also hold without time FE.[22] There are a few other reasons that suggest the two-way effects should not be a relevant concern. In fact, panels (b) and (c) in Fig. 1 show that the sensitivity of VAT to changes in mobility has a similar magnitude than when estimating the regression without time FE. For instance, the ratio of the change in VAT and mobility from period $t = -1$ and $t = 0$ is roughly 60%, while the estimated coefficient is 0.71. Moreover, panel (a) of the same figure shows that the qualitative response was similar across countries. There seems to be little staggering and limited heterogeneity. Overall, these additional results limit the concerns about our baseline estimations being driven by the biases from two-way fixed effects. Regarding potential concerns in the estimation of Standard Errors, Table B.8 re-estimates our baseline results clustering at different levels. Despite falling below common thresholds for an appropriate clustering (Cameron & Miller, 2015), our qualitative results remain somewhat constant for different choices of standard error.

One could also be concerned that, despite the intra-year nature of the analysis, the baseline results could be driven by government expenditure rather than fiscal income. To test for that possibility, Figure C.3 plots VAT and log government expenditure (year-on-year changes) for the five countries in our sample for whom we have monthly data on government expenditure (Brazil, Chile, Dominican Republic, Morocco, Mexico and El Salvador). From this descriptive analysis, we do not see a tight link between both variables for these five countries. This helps reduce concerns on the expenditure margin.

Yet another potential challenge to our interpretation could be whether the tax rate changed around these months. Nonetheless, Table B.3 shows that there have been only very few and rather small changes (e.g., reduced VAT rate for medical supplies). Using the IMF Policy Tracker, we found that in 14 out of the 20 countries in our sample there is no explicit mention to VAT rate changes, while the other six only implemented small exemptions and deferrals. Moreover, Table B.12 adds an interaction term with a dummy that takes the value 1 when countries implemented VAT changes or deferrals (according to IMF policy tracker). Results show no differential effect on VAT revenues for these countries.

Summing up, the tests above seem coherent with our baseline interpretation that the lowered VAT collection seems to come from mobility-related effects in the economy.

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[22] Recent papers (see, e.g., De Chaisemartin & d'Haultfoeuille, 2020) raised awareness on the *simultaneous* use of group and time fixed effects, where the existence of negative weights may even change the direction of the average effect.

3.4 Declining mobility effect on VAT revenues on 2020Q4

Firms, households, and policymakers adjusted to the different economic environments in the months after the onset of Covid-19 shock. For instance, demand for online shopping increased dramatically due to strict mobility restrictions (Bounie et al., 2020). Also, there is evidence suggesting compliance to mobility restrictions decreased as the pandemic evolved (Goldstein et al., 2021). So it would not be surprising that after some months the effects on the economy changed. Table B.11 tests for that last possibility including an interaction term for being in the last quarter of 2020, some 7–10 months after the onset of the Pandemic in most countries. We find that both the effect of mobility restrictions and average mobility on VAT is significantly reduced toward the end of the year 2020. This is coherent with the long-run response displaying some adaptation, including in compliance.[23] In fact, the correlation of policy on aggregate mobility also decreased significantly in the last quarter of our sample. Summing up, there is evidence of a heterogeneous effect over time, coherent with adaptation and lower sensitivity to mobility.

3.5 Exploring potential cross-country heterogeneity in the VAT-mobility relation

After having documented a significant average effect of mobility on VAT revenues, it might be interesting to explore whether specific cross-country characteristics correlate with this mobility-VAT relation. On the one hand, one may wonder, for example, whether richer countries could display a different effect. Similarly, there could be differences stemming from urban–rural composition of the population. On the other hand, there could be country characteristics that proxy for more specific economic channels behind the VAT-mobility relation. One of them is Internet penetration, which could in principle mitigate the effect of mobility restrictions on VAT, as more (VAT-paying) commerce takes place through the Internet, even with little mobility. Another channel could be that some countries tend to have a stronger reliance on imported goods vis-a-vis exported goods, noting that the latter tend not to pay VAT, while the former does.

Empirically, here we estimate a variation on our baseline model that includes interactions of mobility measures and country attributes on the right-hand side of the regression. In particular, we merge our data with standard World Bank data on income level, percentage of the population with access to the internet, % rural population, and the country's tendency to have a negative trade balance. For the latter one, we define net import countries as countries with a negative trade balance over the period 2000–2019. Note that for the interactions, we are looking for preexisting country characteristics rather than using changes in the characteristics, which could be more endogenous to the crisis suffered by the country.

Table D.1 in Appendix shows the results using mobility-restriction policies. The share of rural population shows no statistically significant effect on how stringency

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[23] Moreover, these results are robust to controlling for government expenditure, even despite the substantially smaller sample (only six countries).

of restrictions affects VAT. This holds both in individual interactions and when all interactions are included together. The interaction with a country's income per capita indicator is only slightly significant in the joint model, with all interactions together. Similarly, there is some support for the view that more internet could mitigate some of the dampening effects of mobility restrictions (p-value < 0.1). However, those effects do not hold in the regressions with individual interactions. In contrast, net-importer countries display stronger effects that amplify the mobility-VAT relation both in the specification with a single interaction term (coefficient 0.13; p-value < 0.01), as well as in the joint regression with many interactions. Recall that this aims to capture countries for which imports were relatively more important in general, using a preexisting measure of that tendency, before COVID. And since in the context of COVID international trade was somewhat limited, it could have been more difficult for the national consumption of imported goods and, thus, having an additional mechanism through which mobility restrictions dampened VAT revenues.

Having explored the above results, we prefer to keep the various interpretations above as speculation, because these qualitative results are not robust to using actual mobility measures instead of mobility restrictions. In fact, Table D.2 in Appendix shows that none of the interaction terms is significant when using mobility measures instead of mobility-restriction policies neither individually nor in the joint model with multiple interactions. Summing up, we do not see robust evidence that some of the hypothesized country characteristics generate a systematic change in the response of VAT to mobility. Understanding the channels behind our average response would be a matter for further research.

4 Concluding remarks

This paper estimated the effect of lowered mobility on VAT collection, using a monthly panel of twenty countries around COVID-19. It used both mobility-restriction policy indices as well as mobility outcomes measured from cell phones. The estimated elasticity of aggregate VAT revenues to the mobility-restriction policy is 14%, while the VAT elasticity with respect to actual changes in mobility was twice as large, around 30%. Additionally, the effects significantly decreased toward the last quarter of 2020, suggesting some adaptation both in the economy and in the levels of compliance.

Author contributions Both authors contributed to all the tasks in similar proportions.

Declarations

Conflict of interest The authors declare no competing interests.

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