

Agrawal, David R.; Shybalkina, Iuliia

Working Paper

Online Shopping Can Redistribute Local Tax Revenue from Urban to Rural America

CESifo Working Paper, No. 10204

Provided in Cooperation with:

Ifo Institute – Leibniz Institute for Economic Research at the University of Munich

Suggested Citation: Agrawal, David R.; Shybalkina, Iuliia (2023) : Online Shopping Can Redistribute Local Tax Revenue from Urban to Rural America, CESifo Working Paper, No. 10204, Center for Economic Studies and ifo Institute (CESifo), Munich

This Version is available at:

<https://hdl.handle.net/10419/271848>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

**Online Shopping Can
Redistribute Local Tax
Revenue from Urban to Rural
America**

David R. Agrawal, Iuliia Shybalkina

Impressum:

CESifo Working Papers

ISSN 2364-1428 (electronic version)

Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH

The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute

Poschingerstr. 5, 81679 Munich, Germany

Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de

Editor: Clemens Fuest

<https://www.cesifo.org/en/wp>

An electronic version of the paper may be downloaded

- from the SSRN website: www.SSRN.com
- from the RePEc website: www.RePEc.org
- from the CESifo website: <https://www.cesifo.org/en/wp>

Online Shopping Can Redistribute Local Tax Revenue from Urban to Rural America

Abstract

What is the effect of e-commerce on the geographic distribution of local sales tax revenues? Using COVID-19 as a shock to online shopping and hand-collected high-frequency data on local sales tax revenue, we document an important shift in the state and local public finance landscape. As e-commerce increases, a destination basis for remote sales taxes results in higher growth in local sales tax collections in smaller, generally more rural jurisdictions. This increase comes at the expense of larger urban retail centers, which previously enjoyed an origin basis for sales tax collections. As households replace in-person commerce with online shopping, sales taxes no longer accrue to urban centers with large concentrations of retail establishments and instead expand the tax base of smaller jurisdictions. State-level reforms that enforce sales compliance generally mitigate the revenue falls in larger jurisdictions and amplify the increases in smaller jurisdictions.

JEL-Codes: H250, H710, L810, R510.

Keywords: sales tax, online shopping, e-commerce, COVID-19, tax revenue.

David R. Agrawal
University of Kentucky, Martin School of Public
Policy & Administration &
Department of Economics
433 Patterson Office Tower
USA – Lexington, KY 40506-0027
dragrawal@uky.edu

Iuliia Shybalkina
University of Kentucky, Martin School of
Public Policy & Administration
433 Patterson Office Tower
USA – Lexington, KY 40506-0027
iuliia.shybalkina@uky.edu

December 2022

We are grateful to the editor, Owen Zidar, and four anonymous referees for feedback that improved the paper. We benefited from comments by Richard Beem, Robert Bifulco, Leah Brooks, Jan Brueckner, Gregory Burge, Jeffrey Clemens, William Fox, William Hoyt, Samsun Knight, Justin Ross, David Sjoquist, Kirk Stark, Keith Teltser, Mark Tremblay, Caroline Weber, and David Wildasin, as well as conference/seminar participants at the Association for Budgeting and Financial Management, the Urban Economics Association, and the University of Kentucky. We thank Richard Beem for sharing data on the sales tax base breadth with us. Any remaining errors are our own. We are grateful to our research assistants, Yunjun Kim, Samuel Owens, and Lucas Taulbee for their help in assembling the tax revenue database.

Digitalization affects government finances in complex ways. Digitalization easily allows businesses to establish operations and reach consumers around the world. Thus, firms increasingly lack a significant physical presence in the jurisdictions where consumers are located. This raises fundamental questions about how and *where* digital economic activity should be taxed. The recent rise of internet commerce is an example of how evolving business practices have influenced sales tax revenues and how governments have debated tax policy responses.

We use e-commerce to study the broader phenomenon of how digitalization affects the distribution of tax revenues between agglomerated and smaller—generally more rural—jurisdictions. What is the effect of online commerce on the distribution of local sales tax revenues? Identifying the effect of online commerce on tax revenues is challenging because changes in e-commerce have occurred gradually.¹ However, the recent COVID-19 pandemic provides a large and unexpected shock, which resulted in an immediate expansion of e-commerce at the expense of traditional commerce. We exploit this shock to e-commerce and differences in state tax policies regarding the taxation of remote transactions to identify the effect of digitalization on the distribution of *local* tax revenues. Although an emerging literature studies the effect of e-commerce policies (Beem and Bruce 2021; Fox et al. 2022), these studies focus on states. They cannot shed light on the distributional consequences at a fine spatial scale where retail agglomeration historically advantaged the tax bases of larger—more urban—jurisdictions.

In most states, counties and/or towns levy sales taxes. Brick-and-mortar sales are taxed under the origin principle, so the retailer pays tax to the jurisdiction where the store is located. Unlike brick-and-mortar sales, the tax treatment of remote sales depends critically on the legal rules in place. Historically, sales taxes on remote transactions could not be enforced unless the online vendor had a physical presence in a state; many transactions were thus effectively tax-free. Only recently did some states adopt an economic presence standard, which shifted the statutory incidence of sales taxes from consumers to vendors. Following these reforms, remote vendors are now generally required to pay taxes to the consumer’s locality according to the destination principle. These rules shift *whether* a transaction is effectively taxed and *which* jurisdiction receives the revenue.²

Given the rapid increase in e-commerce from the pandemic, we need *high-frequency* data on *local* tax revenue. Such data are not available in standard datasets. We rely on state revenue/sales reports that are released for counties/municipalities at the monthly/quarterly frequency. These data are acquired from individual states via public

¹Taxes influence the propensity to buy online (Goolsbee 2000; Goolsbee et al. 2010; Ellison and Ellison 2009; Einav et al. 2014; Baugh et al. 2018).

²Recent papers show that remittance rules are an important part of tax systems (Slemrod 2008; Kopczuk et al. 2016; Bibler et al. 2021; Kacamak et al. 2020).

documents or information requests. We digitize, standardize, and harmonize the data across states, assembling the first-ever high-frequency nationally representative panel dataset of local sales tax revenues.

To identify the effect of e-commerce on the distribution of revenues, we estimate high-frequency event studies that allow for heterogeneous effects by jurisdiction size. These event studies demonstrate two main findings. First, we document a *level* effect: the COVID-19 crisis increased sales tax revenues dramatically in many jurisdictions. This contrasts with initial state revenue forecasts that predicted dramatic revenue losses (Chernick et al. 2020; Clemens and Veuger 2020). Second, we document a *distributional* effect: e-commerce raised tax revenues in rural areas at the expense of agglomerated jurisdictions that previously took advantage of origin-based brick-and-mortar taxes. Intuitively, prior to COVID-19, a small-town consumer would drive to a retail agglomeration, contributing tax revenues to the larger jurisdiction. After COVID-19, the consumer buys online and taxes accrue to the hometown. To the best of our ability, we rule out alternative explanations other than e-commerce. We then compare states that require online vendors to remit taxes with those that do not. The ability of states to enforce taxes raises the *level* of tax revenues. The tension between taxing brick-and-mortar sales at origin and remote transactions at destination changes the *distribution* by amplifying the revenue increase in small places at the expense of agglomerated ones.

Our work has implications for the taxation of digital services, teleworkers, and international corporate profits. Recent E.U. reforms switched the tax treatment of digital services from the supplier’s location to the consumer’s location (Hellerstein and Gillis, 2010), eroding the tax bases of low-tax countries where digital suppliers concentrated. In the international corporate tax setting, the extent to which profits were taxable in the consumer’s location depended on the rules for “permanent establishment,” which broadly relate to whether the multinational established a physical presence there (Devereux and Vella, 2018). And, digitalization allows labor to work remotely, which changes where people live or work (Delventhal et al., 2022), altering which jurisdictions receive local income tax revenue depending on whether those taxes are employment-based payroll or residence-based income taxes (Agrawal and Brueckner, 2022).

1 Motivation

1.1 Institutions

Sales taxes are set by the states, with over 35 states allowing county and/or town governments to levy an additional sales tax. A consumer who engages in cross-border shopping

will generally pay the sales tax rate that applies in the local jurisdiction where the retailer is located (origin principle).

The Supreme Court ruling in *Quill Corp. v. North Dakota* prohibited states from requiring remote vendors to remit taxes unless the firm had a physical presence in the state. In cases where the firms did not have a physical presence, consumer use taxes were legally due on a destination basis, but use taxes are notoriously under-enforced (Agrawal and Mardan, 2019). Thus, many e-commerce sales were effectively tax-free (Bruce and Fox 2000; Bruce et al. 2015) as a result of this non-compliance. However, the recent Supreme Court ruling in *South Dakota v. Wayfair* lifted this restriction, allowing states and localities to require remote vendors to remit taxes if they have a significant number or amount of sales in the state. Critically, in most states, the online vendor must generally remit the local sales tax to the jurisdiction where the consumer lives (destination principle), and not the jurisdiction from which the good is sold. Every state except for Florida, Missouri, and Louisiana adopted such a standard prior to COVID-19. However, because small vendors are still exempt from remitting taxes, some states have passed Marketplace Facilitator legislation. These laws require platforms that host a large number of small vendors to remit taxes on a destination basis on behalf of their vendors.

In March 2020, the COVID-19 pandemic increased e-commerce (Chetty et al., 2020). Figure 1 shows that COVID-19 immediately increased e-commerce by 32% and this level increase in e-commerce continues to persist. What effect did this increase in e-commerce have on the level and distribution of tax revenues and do the legal rules matter for those effects?

1.2 Theoretical Motivation

Local jurisdictions differ dramatically in the extent of retail stores within their borders and consumers in rural areas may need to drive to more agglomerated jurisdictions. Appendix B shows that the 10% most populous jurisdictions raise over 90% of sales tax revenue and raise revenue per capita that is three times larger than smaller jurisdictions. Given these differences, the effect of increases in e-commerce across subnational jurisdictions may be heterogeneous by jurisdiction size. Size is critical to many tax competition models, and the “density divide” indicates substantial fiscal inequality between urban and rural America. Internet sales change this paradigm.

To formalize these channels, following the assumptions of Kanbur and Keen (1993) and Agrawal and Wildasin (2019), individuals located in a small and large jurisdiction inelastically buy one unit of a commodity. The only choice is where to purchase: at home, in the other jurisdiction, or online. The initial brick-and-mortar tax bases of the large jurisdiction, $B = R + N$, and small jurisdiction, $b = r + n$, are the sum of residents and

nonresidents buying in the jurisdictions. The big jurisdiction has more residents ($R > r$). Given large jurisdictions raise revenue more than proportionate to their populations, it benefits from inward cross-border shopping ($N > n$). As online shopping increases, denote the absolute value of the changes in the tax base by Δ .

Under *Quill*, remote vendors are not required to remit taxes. After an increase in e-commerce, the tax bases become $B = R + N - \Delta R - \Delta N$ and $b = r + n - \Delta r - \Delta n$. Taxes on online sales are not enforced and lower revenues everywhere. Taking ratios, $[B - \Delta R - \Delta N]/[b - \Delta r - \Delta n] < B/b$ if the fall in the large jurisdiction is bigger than that of the small jurisdiction. This would occur if the change in the number of nonresidents buying in the large jurisdiction is sufficiently large. Although revenues fall everywhere, the share of revenue raised in the large jurisdiction falls.

In contrast, states requiring more firms to remit under *Wayfair* improves sales tax compliance. If all online shopping is from vendors required to remit taxes at destination, the tax bases become $B = R + N - \Delta N + \Delta n$ and $b = r + n + \Delta N - \Delta n$. Residents who switch online now have no effect on the base. However, the tax base of the large [small] jurisdiction falls ΔN [Δn] as it loses nonresidents that shopped there previously, but this raises one-for-one the tax base of the small [large] jurisdiction. In the case where most cross-border shopping is toward retail agglomerations ($n \rightarrow 0$), we have $[B - \Delta N]/[b + \Delta N] < B/b$. As vendors remit taxes, more e-commerce is taxed in the consumer's hometown rather than the towns containing shopping malls; this redistributes revenues from the larger to the smaller jurisdiction (Agrawal and Wildasin, 2019).

In practice, some firms remit while others do not. Thus, in states without economic nexus, the expected effects are in between the two polar cases discussed above.

To summarize, regardless of whether e-commerce vendors remit or not, the *share* of revenue raised by large jurisdictions falls. But whether firms remit or not matters for the relative magnitudes. If firms remit taxes, the shares may also converge, but only then will the *level* of revenue increase in the small jurisdiction as transactions shift from being taxed at origin by brick-and-mortar retailers to the consumer's destination by e-tailers. In the long run, the shifts in revenues may result in tax rate convergence (Agrawal, 2021).

2 Data

COVID-19 provided an abrupt shock to e-commerce starting in March 2020. Given the abrupt shock, to study this question, we need access to high-frequency data on tax revenues at the local level. Unfortunately, standard datasets such as the Annual Survey of State and Local Government Finances are at an annual frequency and are released with a substantial lag.

We hand-collect local revenue data from revenue departments. States often release quarterly or monthly reports on the sales tax base and revenue. In states with local option sales taxes, these reports are often released for sub-state jurisdictions. We scrape state websites to assemble these data for as far back as they go. Some states do not release data in readily available forms, so we obtain data from these states via information requests. The coverage of our data at most spans 1989-2021, but is less for some states. We use 2015-2021 data in our analysis to have complete coverage of states.

We digitize all data, converting pdf files to text format. State revenue statistics often do not contain Census identification codes, so we name-match counties and localities to the appropriate Census files. We then standardize and harmonize the data across states to assemble the first ever high-frequency database of all local sales tax revenue in the U.S.³ Our dataset could then be used to study many questions at the state and local levels.

To these data, we merge Census population, establishment counts, unemployment data, and COVID-19 prevalence rates. All data sources, coverage, cleaning procedures, and dates of policy adoptions are listed in Appendix A.

3 Methods

To study the effect of the shock to e-commerce on revenues in jurisdiction i in time period t (month m of year y), we start with:

$$g(R_{it}) = \alpha + \delta post_t + \theta size_i \times post_t + \Delta_y X_{it} \beta + \epsilon_{it} \quad (1)$$

where the Δ_y operator denotes a year-over-year difference (e.g., March 2021 minus March 2020), $g(R_{it}) = \Delta_y \ln(R_{it})$ is the year-over-year growth rate—the log change—in tax revenues R_{it} , and X_{it} are covariates.⁴ With a year-over-year difference, jurisdiction fixed effects from the level equation difference out. To capture the shock resulting from COVID-19, we include an indicator $post_t$ that takes on a value of one starting in March 2020. We interact it with an indicator if the jurisdiction is small, $size_i$, to capture the heterogeneous effects discussed theoretically. Then, δ represents the change in revenue growth in large jurisdictions after COVID-19, while $\delta + \theta$ represents the effect in small jurisdictions.⁵

Alternatively, because the months immediately after the start of COVID-19 appear different, rather than use a simple $post_t$ indicator, we can replace it with a “short-run” indicator that takes on a value of one in the first two months and a “longer-run” indicator,

³Other studies use single-state databases at the local level (Agrawal and Fox 2021; Afonso 2019; Burge and Piper 2012; Burge and Rogers 2011; Brunner and Schwegman 2017; Seegert et al. 2022; Sjoquist et al. 2007) or state-level data (Beem and Bruce 2021; Mikesell and Ross 2019; Fox et al. 2022).

⁴Control variables include the county unemployment rate and county new COVID-19 cases.

⁵Standard errors are clustered at the state-by-size level.

$long_t$, that takes on a value of one afterward. Focusing on the “longer-run” indicator allows us to remove many of the temporary shocks of the pandemic.

Using year-over-year differences rather than first differences helps remove seasonality from the data. In particular, revenue data are volatile month-over-month: for example, December sees a dramatic increase in sales tax revenue relative to November due to holiday shopping.⁶ Furthermore, the choice of using the log of revenue allows us to focus on growth rates of revenue, which can be approximately interpreted as percent changes.

Our preferred specifications are visual event studies. To flexibly capture dynamic effects, we replace $post_t$ with a full set of time indicators, $\mathbf{1}(t)$, that are also interacted with $size_i$. To do this, we omit March of *three* years prior to COVID-19. Omitting multiple March months rather than a single month eliminates any aggregate shock in a given month-year, and simply makes the effects relative to the average omitted month.

States release data at quarterly or monthly frequency. While we aggregate monthly data to the quarterly frequency and pool the aggregated monthly and quarterly data (Appendix C), in the main text we exploit the monthly-frequency data. Further, states differ in whether they report taxable sales or tax revenue. We initially report revenue results separately, but we also pool revenue and transactions. If tax rates do not change, the log difference specification allows us to pool these two data series. However, in addition, revenues normally include any late payments or penalties, but these same revenues are not reflected in taxable sales.

Finally, (1) does not necessarily have a causal interpretation as to the effect of e-commerce, unless we believe that X_{it} vector accounts for everything else happening during the pandemic. Then, the interpretation of the post-shock indicators is the effect of everything happening at the onset of the pandemic and any dynamic responses to the pandemic. Critical to our results below is the observation that tax revenues rise in many jurisdictions. This is the opposite direction of many confounding events: the recession, lockdowns, and COVID-19 cases should all lower revenues. There are other possible explanations, which we discuss later.

3.1 Size

To partition based on jurisdiction size, in our preferred specifications using county sales tax data, we exploit information on the number of establishments from Census County Business Patterns that are in the retail or wholesale sector.⁷ We use establishments

⁶If we used first differences, the shocks always common to each month are still noticeable.

⁷Some services do collect sales tax, so we verify the results are robust to the total number of establishments.

because the heterogeneity channel arises through the extent of agglomerations. Unfortunately, the number of establishments is only released at the county level. Thus, at the municipal level, we use population as a proxy for economic activity. We then partition the sample into small and large jurisdictions based on size.

Defining small and large jurisdictions is subjective. To provide a data-driven procedure, we estimate (1) replacing $size_i$ with a polynomial function in size. We select the polynomial order using leave-one-out cross-validation. In addition, we relax this parametric form by replacing $size_i$ with a vector of indicators based on decile groupings in size. The results of these exercises (Appendix C) show that the smallest five deciles of size generally have similar effects, while the top decile of jurisdictions is unique. Jurisdictions between the bottom 50% and top 10% have effects somewhere in the middle. Thus, we focus on presenting results for small and large jurisdictions; medium jurisdictions are generally more similar to the small jurisdictions and are presented in Appendix C.

Small [large] counties are defined as the bottom 50% [top 10%] of counties in 2017 based on the number of retail establishments that have NAICS codes in retail, accommodation/food, or wholesale. Small [large] counties have about 70 [3500] establishments in monthly data. Small [large] municipalities are defined as the bottom 50% [top 10%] of municipalities based on population in 2017. Small [large] municipalities have 500 [64,000] people on average. Appendix B shows descriptive statistics by size.

3.2 Remittance Rules

We also interact $post_t$ and $size_i \times post_t$ indicators for the presence of economic nexus or Marketplace Facilitator legislation. These interactions show the role of remittance rules for e-commerce, and can more readily be interpreted as the effect of increased e-commerce in places with and without the policy. Then, comparing the post-COVID-19 evolution of the growth in sales tax revenue requires states with and without economic nexus [Marketplace Facilitator] to have trended in a similar manner absent the shock to e-commerce. The presence of the policy environment interacted with size helps facilitate the identifying assumption because parallel trends need to hold for the subset of small and large jurisdiction *separately*.

4 Results

4.1 Descriptive

After restricting to a balanced panel of jurisdictions, we sum all county and municipal revenue by jurisdiction size. Figure 2 shows that county growth rates were relatively

constant (5%) prior to the pandemic. There is, however, a gradual increase in the growth of county revenue in the middle of 2018, consistent with states adopting economic nexus. At the outset of the pandemic, this consistently positive growth rate turned negative. However, this pattern is mainly driven by large jurisdictions; small jurisdictions saw increases throughout the pandemic. A similar pattern emerges for municipal data.

Figure 2.e/f use the share of revenue raised in large or small jurisdictions relative to the total amount of revenue raised. The figure indicates that the share of revenue accruing to large jurisdictions is now lower than it was historically, but that it has increased in smaller jurisdictions. Mid-sized jurisdictions also increased in their share substantially.

These aggregates indicate that changes in sales tax revenue do not match the forecasts of policymakers, and COVID-19 had a disparate impact on small and large places.

4.2 Regressions

Figure 3 shows the results estimating an event study version of (1) using monthly data. In addition, in the corner of each graph, we display δ and $\delta + \theta$ from the simple version of (1) using $post_t$ and $long_t$ indicators. We suppress middle-sized jurisdictions from the figures. The middle-sized jurisdictions see a fall in the first two months that is somewhere in between the small/large places. But then, they generally see increases in their growth of tax revenue that are similar to small jurisdictions.

In the panels a/c, we consider only states that release monthly tax revenue data. In panels b/d, we pool revenue and sales data, by assuming tax rates are constant and late payments/penalties are small so that the log difference in revenue approximates the log difference in the base. The relatively similar results when pooling suggests these other factors are second-order. Panels a/b indicate that large counties saw a dramatic decrease in revenue and sales in the months immediately after the lockdowns. But, the pattern is strikingly different in small jurisdictions. In particular, even during the lockdowns, tax revenues grew and subsequently rose by up to 10 percentage points more than prior to the pandemic. And then, in the months after the lockdowns, the growth rate of revenue remained higher than normal. This divergence is dramatically different than the three years prior to the pandemic, where large and small jurisdictions grew on relatively similar paths. The reported effects from the event studies represent an increase relative to a baseline 5% growth rate in the omitted months, so a 10 percentage point increase corresponds to a tripling of the growth rate.

The municipal data in panels c/d are noisier, but the same qualitative pattern emerges. Large and small municipalities were growing steadily prior to lockdowns. This growth fell in large cities during the lockdowns. At the same time, small places saw

increased growth rates. Again, all growth rates are relative to growth in the omitted months (5%). Then, the growth rate increases by up to 10 percentage points for small municipalities. If consumer purchases from small stores in remote municipalities did not increase after the pandemic, then because these growth rates stay higher indicates that e-commerce seems to have shifted where tax revenue accrues.

Given the results are qualitatively similar for models that only use states releasing tax revenue and states where we pool revenues and sales, in all subsequent specifications we present the results for pooled samples.

Given the high-frequency design—and because tax revenue is noisy—the confidence intervals sometimes overlap. But, as the simple $post_t$ and $long_t$ interactions indicate, we document statistically different effects when estimating the *average* effect after COVID-19 and longer-term effects after the initial lockdowns. Consider the numbers in Figure 3.b. These coefficients indicate that the growth rate after COVID-19 was 4.4 [5.4] percentage points lower [higher] in large [small] jurisdictions than the growth rate in each prior to the pandemic. In the longer term, the growth rate after May 2020 was 1.6 [6.6] percentage points lower [higher] in large [small] jurisdictions than the growth in each prior to the pandemic. Similar estimates arise at the municipal level, except with smaller falls in large municipalities, likely because the municipal population distribution is more skewed.

To benchmark the magnitudes of these effects, we convert these revenue changes into plausible expenditures. Using the average amount of sales tax revenue raised in each size grouping, we convert the percent changes in tax revenue into the number of new teachers that could be hired. Small [medium] jurisdictions could hire 1.5 [8.1] new K-12 teachers, while the revenue losses in large jurisdictions amount to 107.4 foregone teachers per year.

4.3 Discussion

While our emphasis is on e-commerce, the COVID-19 shock changed many different aspects of the retail sector and the economy. Several explanations are possible. We present several additional splits in Figure 4 for counties and in Appendix C for municipalities. We modify (1) by individually adding additional interaction terms for the jurisdiction characteristics listed in the figures.

First, COVID-19 may have induced changes in the types of consumption. Here the effect is ambiguous: people shifted consumption from untaxed services to taxed tangible goods—especially high-value durable goods—and shifted taxed restaurant meals to (generally) untaxed groceries. To formally show these were not the primary channels, we split our sample into states with broad/narrow tax bases using Beem and Bruce (2021)’s

measure of sales tax base breadth. The shift from untaxed services to taxed services should be related to the extent of the taxable base. We also split the sample by whether the jurisdiction has more/less than the median number of stores per capita with NAICS codes related to durable goods.⁸ To address the shift to groceries, we split the sample based on whether or not the state allows localities to tax grocery food and whether there is a high number of grocery stores per capita in the jurisdiction.

Figure 4 indicates only subtle differences by the proxies for the breadth of the tax base. To interpret the magnitudes, for example, recall in our baseline specification, small jurisdictions saw an increase in revenue growth of 5.4 percentage points. Using the base breadth row as an example, in states that tax many [few] services, the increase was 5.8 [5.1] percentage points. Of all the metrics, the only significant difference is that the taxation of grocery food helps explain the increase in revenues in small jurisdictions. However, this does not rule out e-commerce being the driving factor, especially given there is no differential effect in small places with a large/small number of grocery stores. Because the pandemic triggered a large increase in online shopping for groceries via applications such as Instacart, it is reasonable that states that allow for local taxation of groceries saw larger increases.

While these splits cannot entirely rule out these mechanisms, this analysis shows these alternatives cannot be the only factors. Moreover, these shifts were more transitory and focusing analysis on the months after the initial shock of the pandemic is useful.

A second possibility is that COVID-19 affected jurisdictions differently depending on income, pre-2020 tourism levels, or the extent of interjurisdictional commuting prior to the pandemic. Figure 4 indicates these variables provide little additional information beyond jurisdictional size.

Third, stimulus payments were generally inversely linked to income and aid to state and local governments was linked to income and size. Although CARES/PPP aid was generally granted to states and large sub-state jurisdictions, we digitize any subawards those jurisdictions granted to other jurisdictions within their boundaries.⁹ To explore the effect of aid, we split the sample based on COVID-19 funding received by each jurisdiction, again finding few additional differences. Moreover, these explanations do not necessarily imply relative revenue increases in small jurisdictions, especially as they received less aid.

A fourth possibility is that work-from-home induced people to move from large cities to more suburban neighborhoods with cheaper/larger housing (Delventhal et al., 2022). Unfortunately, municipal population counts are not available at the monthly

⁸Many government agencies widely use number of establishments per capita as proxies, though acknowledging limitations.

⁹Clemens and Veuger (2021) document small-state biases due to Congressional representation, but our focus is on within state variation.

level—and even if they were, many of these moves may have been informal. While this might explain a decline in sales tax revenues in cities, many of these moves were likely to areas at the periphery of the urban area. Thus, our focus on small jurisdictions helps. To explore these channels, we explore Google Community Mobility reports as a proxy. In these data, cell phone data reveal the change in the duration of time spent at home and the number of individuals going to work. While not migration, these variables again provide little additional information beyond size.

A possibility that we focus on is that the shift to e-commerce induces fewer shopping trips by rural residents to urban jurisdictions. If this channel is at work, the connectivity of the jurisdiction should matter. We exploit the USDA urban-rural continuum codes that classify counties as within a metropolitan area, adjacent to a metropolitan area, and away from a metropolitan area. All large jurisdictions are located within urban areas, but small counties can fall into any category. We find the largest increases in small counties that are well-connected, with the increases in revenue following monotonically in distance to urban centers.¹⁰ Although statistically similar, as theory would predict, the linkages that small places have with large places matter.

We conclude that increases in online shopping disproportionately benefit smaller jurisdictions.

4.4 Remittance Rules

Remittance rules matter. State differences in these institutional rules allow us to identify how remittance rules *interact* with the shift to e-commerce. First, economic nexus allows states to require more remote vendors to collect the tax, which has a *level* effect on total tax revenue. Second, in states with destination sourcing, revenues go to the place where people live. Third, the brick-and-mortar to online shift has a *distributional* effect inside the state, as big jurisdictions lose cross-border shoppers, but has no effect at the state level to the extent that economic nexus allows the revenues to still be collected. We capture the totality of these effects.

Similar to the prior section, we modify (1) to include additional interaction terms with an indicator for whether the locality is in a state with economic nexus and an indicator for whether the locality is in a state with Marketplace Facilitator laws. As these two policies often occur simultaneously, we include both indicators and all appropriate interactions in a single equation. This allows us to estimate the heterogeneous effects of the COVID-19 crisis separately by whether the locality is in a state without economic nexus, with economic nexus, or with both economic nexus and Marketplace laws.

¹⁰Increases in places away from MSAs also helps to rule out mobility from telework as the primary explanation.

For comparability with the prior results, we use monthly data for this analysis, but Appendix D shows that the results are robust to pooling states that release monthly and quarterly data. Figure 5 present the marginal effects of the $post_t$ variable, along with the alternative specification that focuses on the “longer-run” indicator to remove the transitory falls due to the lockdowns. For counties in states without economic nexus, revenue growth in large [small] counties after COVID-19 was 15.5 [3.5] percentage points lower [higher] than before it. In contrast, for counties in states with economic nexus, revenue growth in large [small] counties after the shock was 4.6 [13.4] percentage points higher [higher] than before it. Unfortunately at the municipal level, we do not have any states without economic nexus that release monthly data. But the municipal panel indicates that small places saw bigger increases than larger jurisdictions, so that these remittance rules increase compliance everywhere but also raise growth rates in small towns.

Focusing on counties, unlike localities in states with economic nexus, no-nexus large jurisdictions saw large decreases in revenue. These decreases persist even in specifications that focus on the long-run indicator. These declines did not happen in nexus states. Consistent with theory, economic nexus increases sales tax compliance on resident purchases. Revenues rose slightly in small jurisdictions without nexus, likely because many large online vendors were remitting taxes due to physical presence. But, revenues rose by 9.9 percentage points *more* for small jurisdictions in states with economic nexus. As indicated by theory, this larger increase is due to the added compliance of residents in small areas, but also the redistributive effects of firm-based remittance rules. These results provide evidence that economic nexus—and destination sourcing—were critical to revenue growth. Absent it, large counties saw declines in the growth of sales tax revenue, similar to estimates predicted by policy experts. Economic nexus was critical for the massive growth rates in small jurisdictions.

Marketplace Facilitator legislation has the potential to destination-source more online transactions. Figure 5 also plots the marginal effects in states with Marketplace Facilitator legislation. In theory, Marketplace Facilitator Laws result in additional compliance beyond economic nexus by having the platform remit for small vendors. For counties, the growth rates in these states are higher than no nexus states but are not higher than nexus states. However, the *absence* of both laws implies substantial declines in revenue in large jurisdictions: growth in Marketplace states were 13.2 percentage points higher than no nexus states. Marketplace laws may primarily benefit larger jurisdictions, perhaps because sales by smaller niche vendors not remitting taxes may be more likely to be from higher-income urban consumers.

Why are the effects smaller in states with both economic nexus and Marketplace

laws than in states with only economic nexus? A possibility could be that states differed in other dimensions of these reforms. For example, states differ in the threshold of sales necessary to trigger an economic presence. Thresholds vary between \$100,000-\$500,000 and these thresholds interact in complex ways with the physical presence of firms within the state. Our smaller effects in states with Marketplace laws suggest that the combination of two policies seems to induce less compliance than the set of states with economic nexus alone. States with compliance by large firms above a high threshold and small firms on marketplaces may have a “donut” hole in compliance if “midsized” vendors are not covered. Indeed, states without Marketplace laws may have set their nexus thresholds lower and the revenue gains from those additional midsized vendors could reasonably be larger than the revenue from small marketplace vendors.¹¹ While speculative, the administrative design of remittance rules matters.

Another aspect of tax design is whether states fully shifted to destination taxation with economic nexus. Most states have destination sourcing on remote sales. But, a few states have a quasi-origin-based system where remote sales are remitted to the destination jurisdiction for out-of-state sales but are sourced to the origin jurisdiction for within-state e-commerce—preserving the agglomeration advantages of larger jurisdictions. The last panel of Figure 4 shows that this distinction matters a little for small jurisdictions, but substantially mitigates the revenue declines in larger jurisdictions. Sourcing within-state e-commerce taxes at origin preserves the agglomeration advantages of large jurisdictions.

We conclude that the massive shift to e-commerce was *sufficient* to alter the distribution of revenues across jurisdictions. States without economic nexus saw increases in the revenues of small jurisdictions and decreases in the revenues of large jurisdictions. Institutional reforms that changed the remittance rules, however, *amplified* the positive revenue growth effects in smaller jurisdictions, while *mitigating* the fall in larger urban areas. The extent of these shifts varies depending on the thresholds used to trigger economic nexus and the extent to which states embraced destination sourcing. Thus, remittance rules altered both the level of revenue that was raised from e-commerce and *where* tax revenues accrued. Thus, e-commerce was critical for “redistributing” tax revenues.

5 Conclusion

The “density divide” in America has created a perception that urban areas are more vibrant, while rural places have stagnated. Moreover, historically, rural America has seen declining tax bases, concentrating local tax revenue into more urban areas. This fiscal

¹¹Based on December 2020 data, the average county with Marketplace laws had an economic nexus threshold that was \$30,000 lower than the average county in a state without Marketplace laws.

divide can then reinforce urban-rural inequities.

We argue that digitalization—with appropriate remittance rules—can act as an equalizer of sales tax revenue by eroding the fiscal benefits of large retail agglomerations. In the extreme, if destination sourcing always prevailed and all purchases were online, then tax revenues would be distributed proportionally to population/income. Fiscal systems are not invariant to the tax rules in place.

Is this distributional shift likely to persist? While part of our results inevitably entails some online purchases that would not have been made absent the pandemic, Figure 1 indicates that the surge in e-commerce has persisted. While the magnitude of the growth in rural revenue may slow, it is likely to maintain an upward trend as e-commerce continues to grow. Furthermore, given firm-based remittance rules will likely become universal, the results of this paper apply to further marginal increases in e-commerce.

From an efficiency perspective, individuals should pay taxes equal to the marginal congestion cost that they impose on public services (Wildasin, 1980). If cross-border shoppers do not benefit from public services where they shop, then a shift to destination-based e-commerce will be more efficient than a system of origin-based brick-and-mortar taxes. But there are also equity implications. If rural residents are lower income and those jurisdictions have lower taxes, a switch from physical to remote goods may reduce the regressivity of the sales tax.

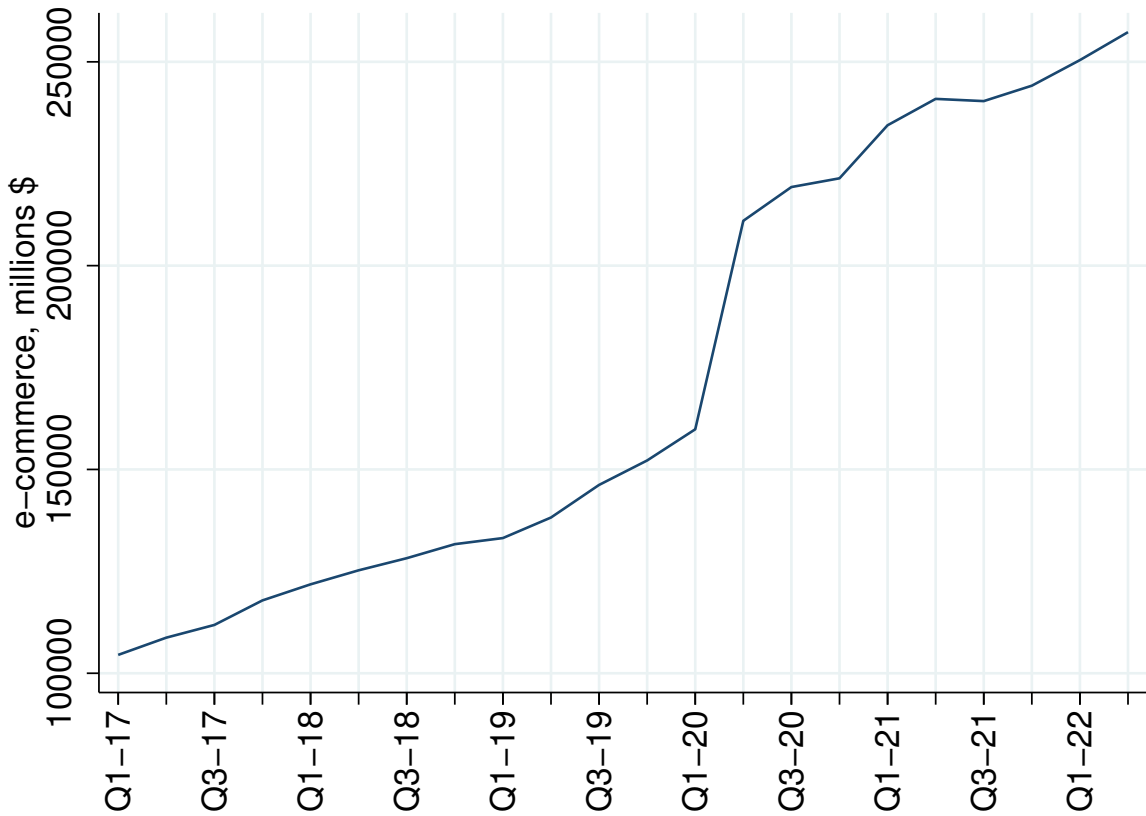
The paper has implications for the effect of digitalization on the level and distribution of tax revenues from digital services, teleworkers, and corporate income.

References

- Afonso, Whitney**, “The Impact of the Amazon Tax on Local Sales Tax Revenue in Urban and Rural Jurisdictions,” *Public Budgeting & Finance*, 2019, 39 (2), 68–90.
- Agrawal, David R.**, “LOST in America: Evidence on Local Sales Taxes from National Panel Data,” *Regional Science and Urban Economics*, 2014, 49, 147–163.
- , “The Internet as a Tax Haven?,” *American Economic Journal: Economy Policy*, 2021, 13 (4), 1–35.
- **and David E. Wildasin**, “Technology and Tax Systems,” *Journal of Public Economics*, 2019, 185, 104082.
- **and Jan K. Brueckner**, “Taxes and Telework: The Impacts of State Income Taxes in a Work-from-Home Economy,” *CESifo Working Paper No. 9975*, 2022.
- **and Mohammed Mardan**, “Will Destination-based Taxes Be Fully Exploited When Available? An Application to the U.S. Commodity Tax System,” *Journal of Public Economics*, 2019, 169, 128–143.
- **and William F. Fox**, “Taxing Goods and Services in a Digital Era,” *National Tax Journal*, 2021, 74 (1), 257–301.
- Baugh, Brian, Itzhak Ben-David, and Hoonsuk Park**, “Can Taxes Shape an Industry? Evidence from the Implementation of the “Amazon Tax”,” *The Journal of Finance*, 2018, 73 (4), 1819–1855.
- Beem, Richard and Donald Bruce**, “Failure to Launch: Measuring the Impact of Sales Tax Nexus Standards on Business Activity,” *Journal of Public Economics*, 2021, 201, 104476.
- Bibler, Andrew J., Keith F. Teltser, and Mark J. Tremblay**, “Inferring Tax Compliance from Pass-through: Evidence from Airbnb Tax Enforcement Agreements,” *Review of Economics and Statistics*, 2021, 103 (4), 636–651.
- Bruce, Donald and William F. Fox**, “E-Commerce in the Context of Declining State Sales Tax Bases,” *National Tax Journal*, 2000, 53 (4), 1373–1388.
- , – , **and LeAnn Luna**, “E-Tailer Sales Tax Nexus and State Tax Policies,” *National Tax Journal*, 2015, 68, 735–766.
- Brunner, Eric J. and David J. Schwegman**, “The Impact of Georgia’s Education Special Purpose Local Option Sales Tax on the Fiscal Behavior of Local School Districts,” *National Tax Journal*, 2017, 70 (2), 295–328.
- Burge, Gregory and Cynthia Rogers**, “Local Option Sales Taxes and Consumer Spending Patterns: Fiscal Interdependence under Multi-tiered Local Taxation,” *Regional Science and Urban Economics*, 2011, 41, 46–58.
- Burge, Gregory S. and Brian Piper**, “Strategic Fiscal Interdependence: County and Municipal Adoptions of Local Option Sales Taxes,” *National Tax Journal*, 2012, 65 (2), 387–416.
- Cameron, A. Colin and Douglas L. Miller**, “A Practitioner’s Guide to Cluster-robust Inference,” *Journal of Human Resources*, 2015, 50 (2), 317–372.
- Chernick, Howard, David Copeland, and Andrew Reschovsky**, “The Fiscal Effects of the COVID-19 Pandemic on Cities: An Initial Assessment,” *National Tax Journal*, 2020, 73 (3), 699–732.
- Chetty, Raj, John Friedman, Nathaniel Hendren, Michael Stepner, and The Opportunity Insights Team**, “The Economic Impacts of COVID-19: Evidence from a New Public Database Built Using Private Sector Data,” 2020. NBER Working Paper 27431.

- Clemens, Jeffrey and Stan Veuger**, “Implications of the COVID-19 Pandemic for State Government Tax Revenues,” *National Tax Journal*, 2020, 73 (3), 619–644.
- **and –**, “Politics and the Distribution of Federal Funds: Evidence from Federal Legislation in Response to COVID-19,” *Journal of Public Economics*, 2021, 204, 104554.
- Delventhal, Matthew J., Eunjee Kwon, and Andrii Parkhomenko**, “JUE Insight: How do Cities Change when We Work from Home?,” *Journal of Urban Economics*, 2022, 127, 103331.
- Devereux, Michael P. and John Vella**, “Implications of Digitalization for International Corporate Tax Reform,” *Intertax*, 2018, 46 (6/7), 550–559.
- Einav, Liran, Dan Knoepfle, Jonathan Levin, and Neel Sundaesan**, “Sales Taxes and Internet Commerce,” *American Economic Review*, 2014, 104 (1), 1–26.
- Ellison, Glenn and Sara Fisher Ellison**, “Tax Sensitivity and Home State Preferences in Internet Purchasing,” *American Economic Journal: Economic Policy*, 2009, 1 (2), 53–71.
- Fox, William F., Enda Patrick Hargaden, and LeAnn Luna**, “Statutory Incidence and Sales Tax Compliance: Evidence from *Wayfair*,” *Journal of Public Economics*, 2022, 213, 104716.
- Goolsbee, Austan D.**, “In a World Without Borders: The Impact of Taxes on Internet Commerce,” *Quarterly Journal of Economics*, 2000, 115 (2), 561–576.
- Goolsbee, Austan, Michael F. Lovenheim, and Joel Slemrod**, “Playing With Fire: Cigarettes, Taxes and Competition From the Internet,” *American Economic Journal: Economic Policy*, 2010, 2 (1), 131–154.
- Hellerstein, Walter and Timothy H. Gillis**, “The VAT in the European Union,” *Tax Notes*, 2010, 127, 461–471.
- Kacamak, Yeliz, Tejaswi Velayudhan, and Eleanor Wilking**, “Does it Matter Who Remits?: Evidence from U.S. States’ Voluntary Collection Agreements,” June 2020. Working Paper.
- Kanbur, Ravi and Michael Keen**, “Jeux Sans Frontières: Tax Competition and Tax Coordination When Countries Differ in Size,” *American Economic Review*, 1993, 83 (4), 877–892.
- Kopczuk, Wojciech, Justin Marion, Erich Muehlegger, and Joel Slemrod**, “Does Tax-collection Invariance Hold? Evasion and the Pass-through of State Diesel Taxes,” *American Economic Journal: Economic Policy*, 2016, 8 (2), 251–86.
- Mikesell, John L and Justin M Ross**, “After *Wayfair*: What Are State Use Taxes Worth?,” *National Tax Journal*, 2019, 72 (4), 801–820.
- Seegert, Nathan, Maclean Gaulin, and Mu-Jeung Yang**, “The COVID-19 State Sales Tax Windfall,” 2022. Working Paper.
- Sjoquist, David L., William J. Smith, Mary Beth Walker, and Sally Wallace**, “An Analysis of the Time to Adoption of Local Sales Taxes: A Duration Model Approach,” *Public Budgeting & Public Finance*, 2007, 27 (1), 20–40.
- Slemrod, Joel**, “Does It Matter Who Writes the Check to the Government? The Economics of Tax Remittance,” *National Tax Journal*, 2008, 61 (2), 251–275.
- Wildasin, David E.**, “Locational Efficiency in a Federal System,” *Regional Science and Urban Economics*, 1980, 10 (4), 453–471.

Figure 1: The COVID-19 Shock and Online Shopping



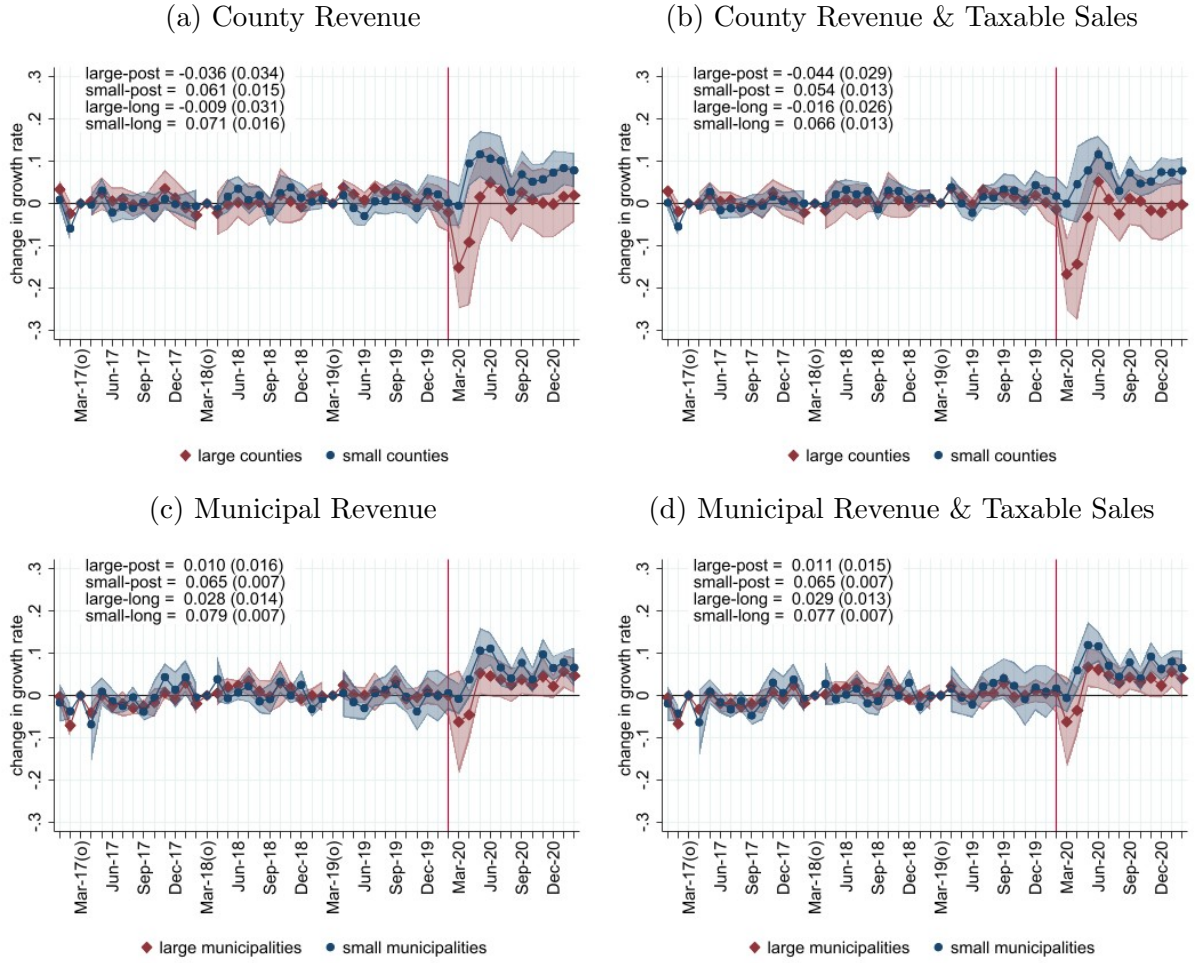
This figure shows how the COVID-19 shock affected online sales. Online sales are shown in millions of dollars, and data are sourced from the Census Bureau Quarterly Retail e-Commerce Sales Report. Appendix B presents descriptive evidence on how COVID-19 shocked unemployment and PPP/CARES in small and large jurisdictions, as well as other descriptive differences between small and large jurisdictions.

Figure 2: Raw Data: Local Revenue for the Nation as a Whole



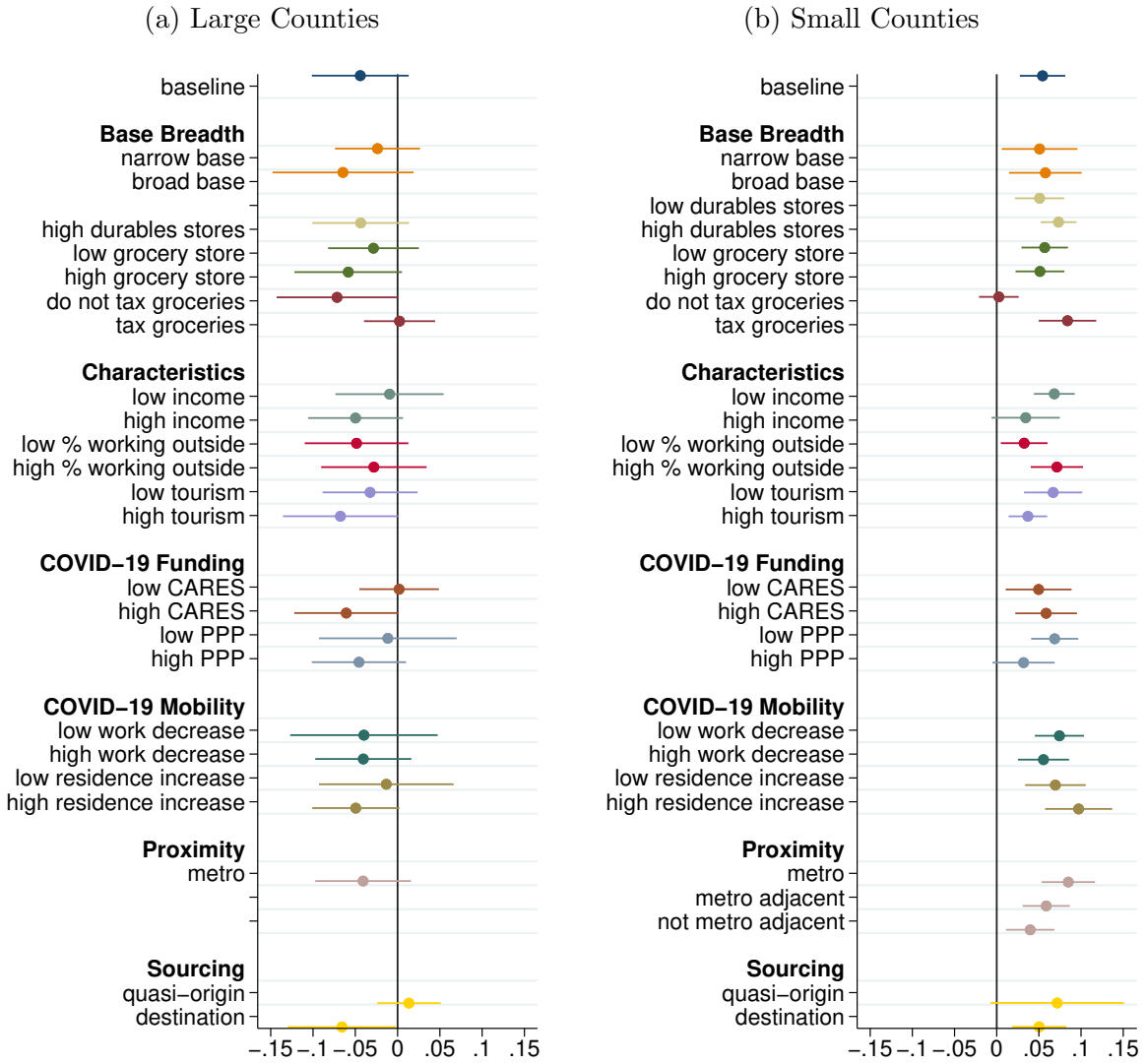
Panels (a)-(d) show the year-over-year growth rate in county and municipal sales tax revenue. To construct the figure, we focus on a balanced sample of counties and municipalities that appear in our data from 2015 to 2020. For purposes of this descriptive figure, when data are monthly, we aggregate it to the quarterly frequency in order to have the largest sample of states possible; all other figures in the main text use monthly data to capture high-frequency responses. The figure then plots the year-over-year growth rate. Panel (a) shows the growth rate for all (small, medium, and large) counties' tax revenue in the U.S., while panel (c) separately plots the growth for large and small counties. Panel (b) shows the growth rate for all (small, medium, and large) municipal tax revenue in the U.S., while panel (d) separately plots the growth for large and small municipalities. Panels (e)-(f) plot the share of local revenue raised in large and small jurisdictions exploiting our high-frequency data. The thresholds for small and large jurisdictions are defined in the text and Appendix B present precise details of this split.

Figure 3: Growth in Local Revenue: Baseline Specifications



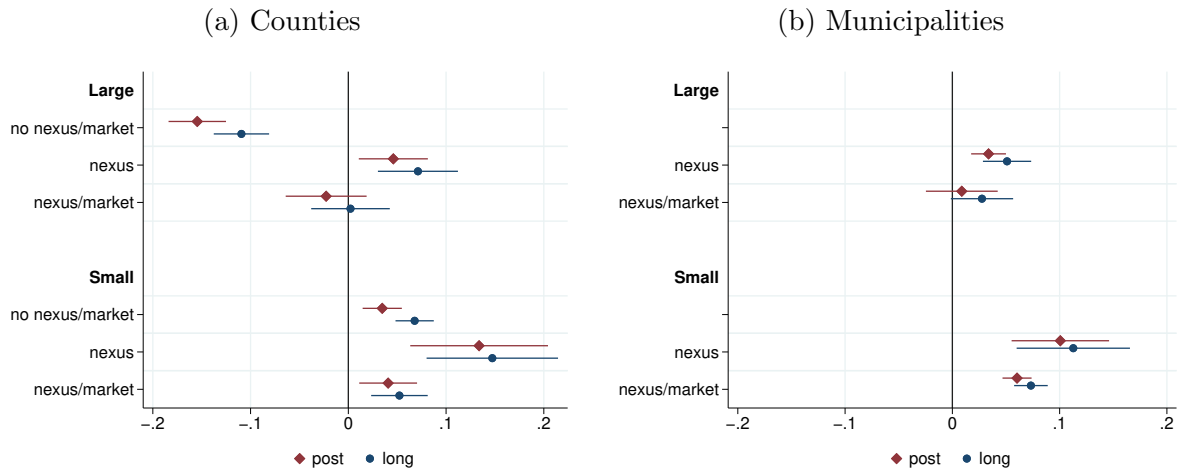
This figure shows estimation of (1) in an event-study form where the dependent variable is the year-over-year growth rate in county or municipal sales tax revenue or/and taxable sales. Each figure plots the effect separately by size with red diamonds denoting large jurisdictions and blue circles denoting small jurisdictions. Panel (a) shows the change in the growth rate for county tax revenue relative to the omitted months (marked by “o” on the figure), while panel (b) plots the change in growth after pooling states that release revenue and taxable sales. Panel (c) shows the change in the growth rate for municipal tax revenue, while panel (d) plots the change in growth after pooling states that release revenue and taxable sales. Results for taxable sales and revenues can be pooled if tax rates do not change over time and other components of revenue (e.g., late fees or penalties) are negligible. The thresholds for small and large jurisdictions are defined in the text. Shaded areas denote 95% confidence intervals with standard errors clustered at the state-by-size level. Text boxes present the results of (1) with a single $post_t$ (one if March 2020 - February 2021) or $long_t$ (one if May 2020 - February 2021) indicators. In the latter expression, we include a $short_t$ indicator, so the interpretation on $long_t$ is relative to pre-COVID-19. The changes in growth rates for small and large jurisdictions are reported alongside standard errors clustered at the state-by-size level. Appendix C presents various robustness check including using the full sample of pooled monthly and quarterly states, presenting results for mid-sized jurisdictions, and other flexible specifications based on size.

Figure 4: Heterogeneity by Various Characteristics



This figure shows the change in the year-over-year growth rates in county sales tax revenue or taxable sales in the post period (March 2020 - February 2021) for different types of large and small jurisdictions. We focus on states that release monthly data in this figure. We construct indicators for whether the jurisdiction is below (low) and above (high) the median separately for various metrics. We then interact this with the $post_t$ and $size_i \times post_t$ variables in the regression. The metric of base breadth is the ratio of the tax base to income as discussed in Beem and Bruce (2021). For durable goods stores, grocery stores, and tourism, we use the number of establishments per capita in NAICS codes 442 Furniture and Home Furnishings Stores / 443 Electronics and Appliance Stores, 445 Food and Beverage Retailers, and 721 Accommodation, respectively. All large counties have an above-median number of durable goods stores per capita. Data on state and local grocery taxes are from the Center on Budget and Policy Priorities (Figuroa and Legendre 2020). Income and the share of workers working outside the county of residence come from the American Community Survey. PPP and CARES transfers are aggregated by county and standardized by population. CARES transfers include transfers to all local governments within each county from the federal and state governments. Changes in places of work and residence are derived from cell phone data. US Department of Agriculture metro designation is used; all large counties belong to a metro area. Finally, data on sourcing rules are from Avalara; quasi-origin sourcing can be used for inter- and/or intra-state sales. Spikes denote 95% confidence intervals with standard errors clustered at the state-by-size level. We do not present similar results for $long_t$ but have verified that those effects are rightward shifts of the estimates in this figure. Appendix C presents analogous results for our pooled monthly and quarterly sample as well as for the sample of municipalities.

Figure 5: Nexus Effects



This figure shows the change in the year-over-year growth rate in county and municipal sales tax revenue or taxable sales in the post period (March 2020 - February 2021) separately by size in states that do not have economic nexus or Marketplace Facilitator legislation, states that only have economic nexus, and states that have both institutions. We focus on states that release monthly data in this figure. The thresholds for small and large jurisdictions are defined in the text. Spikes denote 95% confidence intervals with standard errors clustered at the state-by-size level. Appendix D shows event studies based on these remittance rules, similar results for pooling monthly and quarterly data, and the results of a placebo exercise where we randomize nexus and Marketplace Facilitator policies across states.

Online Appendix for “Online Shopping Can Redistribute Revenue from Urban to Rural America”

David R. Agrawal and Iuliia Shybalkina

A Appendix (Online Only): Data Appendix

A.1 Tax Revenue Data

We assemble a comprehensive database of taxable sales and/or local sales tax revenue data. The finished product includes local sales tax data from 33 states at the monthly or quarterly frequency, 7 additional states that release local data but only at the annual frequency, and 6 additional states that do not provide local data but provide state data. Of the latter six, only one allows for local sales taxes and thus many of these states do not require local information on their filing returns. Thus, we have an extremely comprehensive database covering most states that allow for local option taxes.¹ In this appendix, we describe the data construction and harmonization for other researchers wishing to use the data. These states and respective data sources are listed in Table A.1.

Given our empirical design requires a high-frequency and local government approach, although we collect data for all states when available, we do not include some of them in the analysis. In particular, the seven states that provide data at the county and/or municipality level, but only at the annual frequency are Alabama, Alaska, Indiana, Louisiana, Maine, Pennsylvania, and South Carolina. The six states that do not provide any county or municipality-level data are Arizona, Kentucky, Massachusetts, Michigan, New Jersey, and Rhode Island. States that either only report local-level data at the annual frequency or do not report any local-level data are likely to have no local sales taxes (Indiana, Kentucky, Maine, Massachusetts, Michigan, New Jersey, and Rhode Island).² Thus, of six states with no sub-state data, only Arizona allows for local sales taxes; all the other states do not have local sales taxes and thus likely do not even collect information on where the sales occur. Four states, including Delaware, Montana, New Hampshire, and Oregon, have no sales taxes of any kind.

To assemble the data, we download the revenue reports from state websites or, when not available, submit information requests for the state asking them to provide the

¹If presented with choices, we focus on general unearmarked sales and use taxes. For example, we do not include product-specific sales taxes, such as lodging taxes and we do not include sales taxes that are earmarked for various special districts.

²Connecticut and Maryland are two other states without local sales taxes, but they provide taxable sales at the local level.

data. The two most common formats for original data are Excel or PDF. We convert PDF files into machine-readable data formats.

States differ in the length of time that they have maintained and released local sales tax data with some states starting as early as 1989. Even though we only use recent years of data in our analysis, we digitize and collect data dating back to the earliest possible year for each state. Figure A.1 summarizes the range of data for each state.

Most states provide some but not all data types (i.e., revenue data versus transaction data or monthly data versus quarterly data). Table A.2 presents available data types by state by taxing jurisdiction (municipal versus county), type of data (revenue versus sales), and the time frequency (monthly versus quarterly). The states in this table form the basis of our empirical analysis. In samples using quarterly data, we aggregate monthly-frequency data to obtain quarterly-frequency data for all states releasing data. Therefore, quarterly data samples include states that provide quarterly data and, additionally, all states that provide monthly data. Vermont, Virginia, and Washington report both quarterly and monthly data, in which case we use original reported quarterly data.³ Iowa also reports both quarterly and monthly data, but we discard monthly amounts, which the state calculates by dividing quarterly amounts by three. Sometimes states report taxable sales and the local tax rate for each period. In these cases, we multiply sales together by tax rates to calculate tax revenue. Similarly, we can also obtain taxable sales if the state releases revenue and tax rates.

We discarded all county-level data for Mississippi. These data are year-to-date revenue. We attempt to obtain monthly data by subtracting year-to-date revenue numbers from the month prior. However, these data appear unusually volatile, likely because updates to filings, and thus the revenue series, cannot be accounted for. Finally, we discard all county- and municipality-level data for Idaho. Idaho distributes sales tax revenue to localities based on a formula unrelated to the location of actual sales. A small number of other states use a formula, but we deemed that it was appropriate to keep them because we either have transaction data (for example, California) or the formula is at least partially related to the location of actual sales (for example, Utah).

We focus on the period between 2015 and 2020 in quarterly data and until February 2021 in monthly data. However, the panel is unbalanced for several states. Thus, some most recent data are not yet available (Connecticut post-2019 and Illinois post-3rd quarter 2020). In addition, some states began reporting after 2015. Thus, Oklahoma and Utah data start in 2017. Nebraska taxable sales data for counties and municipalities began in 2016. Kansas county taxable sales started in 2018, and municipal revenue started in 2019. Colorado municipal taxable sales started in 2016. Although we generally use this window

³Quarterly data reported by states may be preferable to data aggregated from month because in states releasing both series quarterly data may be more complete. For example, Vermont includes only monthly filers in monthly data, but quarterly reports include monthly and quarterly filers.

for our analysis, we have cleaned and harmonized earlier revenue data when available, and will release this data as part of our data package.

States may report taxable sales and local sales tax revenue data in month t based on economic activity in month t , $t - 1$, or $t - 2$ or based on the time period when the tax return is due. To standardize revenue into the month of sale, we exploit the fact that Christmas/holiday shopping creates a noticeable spike in revenues for the month of December. Therefore, we have standardized our monthly sales tax data by month of economic activity. To do so, we visually inspected each state's time series to identify a jump in economic activity caused by Christmas/holiday shopping. Then, we used that information to recenter the data so that December corresponds to that month. Quarterly data are as reported by states because quarter four generally is the quarter with the highest sales.

Sometimes data appear erroneous or substantially outside the normal range of plausible values, so we clean the data to address these issues. To give one example, sometimes jurisdictions keep receiving revenue from some filers by mistake after the expiration of a tax. If we thought that was the case, we adjusted the data accordingly. We disregarded the revenue that Georgia allocates to local governments due to one-off audits or distributions of revenue unmatched to any location. We collected data for as many periods as possible even though some states made some changes to their reporting practices at some point (for example, Colorado in 2019). Sometimes these changes involve how states require vendors to report transactions, including online transactions. Thus, these small changes may cause variations in the data reported from previous years. As a quality control, we plot the revenue series for each state to guarantee no noticeable differences. We discard data if these reporting changes rendered data utterly incompatible, but every one of these cases occurs in historical periods not used in the timeframe for our event studies. Nonetheless, researchers using our database for historical purposes should keep this in mind. To minimize the influence of the remaining outliers, calculated changes $g(R_{it})$ are winsorized by month (quarter) at the top and bottom 2.5 percent.

Generally, states do not include county or place FIPS codes in sales tax data, so these data cannot be mapped to Census data. While some states indicate a county name for localities, others do not. Except for two states, we matched municipalities by name to the list of places provided by the U.S. Census Bureau. For Connecticut and Vermont, we used county subdivisions instead of places. That way, we obtained place (county subdivision) FIPS for nearly every municipality from our tax revenue database. Thus, we achieved a nearly 100 percent matching rate for data in the tax files for all but three states. The average match rate for Minnesota, Missouri, and South Dakota is 80 percent due to these three states including many non-Census designated units in their tax data. If needed, we also assigned municipalities to counties in the case where municipalities have the same name in a state. For municipalities located in multiple counties, total revenue

or sales are calculated if the state partitions municipalities by county in their tax data. Further, these municipalities are assigned to the county which houses the largest share of the municipal population.

Note, matching all municipalities in the tax database does not mean we find a match for all Census places. This could be due to a number of reasons: Census places include statistical units that have no governance authority or, alternatively, because states only release revenue data for a subsample of municipal jurisdictions. We compared municipalities in our municipal-level tax data to the list of places provided by the U.S. Census Bureau for states in our sample. Our data cover 57 percent of places (from 4 to 95 percent by state).

We matched counties by name to the list of counties to obtain county FIPS codes. Consolidated city-county units are generally in the county-level data. We compared counties in our county-level tax data to the list of counties provided by the U.S. Census Bureau for states in our sample. Except for two states, our data covers nearly all counties. Two exceptions are that 38 counties (out of 87) counties in Minnesota and 131 (out of 254) counties in Texas are not in our tax data, likely because they do not have county local sales taxes.⁴ In addition to that, our data do not include 4 counties in Wisconsin (also, likely due to no local sales taxes) and Kalawao County in Hawaii. New York City values are reported for the city as a whole.

A.2 Other Data

In order to determine the relative sizes of counties, we obtain the number of establishments from the U.S. Census Bureau County Business Patterns. Specifically, we calculate the total number of establishments in 2017 in NAICS sectors 42 Wholesale Trade, 44-45 Retail Trade, and 72 Accommodation and Food Services. County Business Patterns only publish a cell if it contains three or more establishments. We replace missing values with zero for our baseline calculations. Replacing missing values with three instead produces the same breakdown of counties into small and large. The number of establishments is not available for municipalities. Therefore, as a proxy of relative retail size we use population from the 5-Year American Community Survey 2015-2019 to determine the size of municipalities.

Monthly unemployment rate data for counties are from the U.S. Bureau of Labor Statistics. We use the unadjusted unemployment rates. We calculate quarterly unemployment rates by averaging monthly rates within each quarter.

The daily number of COVID-19 cases for counties comes from the Centers for Disease Control and Prevention (CDC). At the county level, the CDC reports a 7-day rolling

⁴In Texas, some counties participate in metropolitan transit authorities instead of using county sales taxes.

average number of new cases. We aggregate these data to the monthly and quarterly levels in three different ways: by calculating the average, the maximum, and the minimum of rolling averages within each month or quarter. We treat suppressed values as zeros, which given the suppression is in very small places, is a reasonable approximation. We also replaced a small number of negative values in CDC data with zeros. We standardize the number of cases per 100,000 population from the American Community Survey.

Paycheck Protection Program (PPP) loan data are from the Small Business Administration. We aggregated 2020 data by county using the date of loan approval, original loan amount, and the project state and county. The project state and county were missing for a negligible number of loan observations. We matched counties by name to the list of counties to obtain FIPS codes. We standardized the total loan amount for each county by county population from the American Community Survey.

CARES Coronavirus Relief Fund award data are from the U.S. Treasury. The federal government sent money directly to states and large cities and counties (above 500,000) proportional to population. States and large places could further share these funds with smaller places within their borders. We collected county-level (sum of county and all general local governments) data on those primary and secondary (shared by the state) awards. We matched recipient and sub-recipient municipalities and counties in the Treasury data by name to the list of places (subdivisions) and counties to obtain FIPS codes and used a crosswalk by the Missouri Census Data Center to assign municipalities to counties. We standardized the total award amount for each county by county population from the American Community Survey.

Economic nexus and Marketplace Facilitator data are from Fox et al. (2022). These data are summarized in Table A.3.⁵ Data on grocery taxes are from the Center on Budget and Policy Priorities as of April 2020 (Figueroa and Legendre 2020). Groceries can be taxed by the state governments, local governments, or both. AR, IL, MO, VA, UT, AL, HI, TN, OK, SD, ID, KS, and MS tax groceries at the state and local levels. AZ, CO, GA, LA, NC, and SC tax groceries at the local level only. Data on origin- and destination-based sourcing rules are from Avalara. As of 2020, 12 states used quasi origin-based sourcing for inter- and/or intra-state sales: AZ, CA, IL, MS, MO, NM, OH, PA, TN, TX, UT, and VA.

We also use data on quarterly online sales from the Census Bureau Quarterly Retail e-Commerce Sales Report, various jurisdiction characteristics from the American Community Survey, movement trends from Google Community Mobility Reports, rural-urban continuum codes and commuting zones from the U.S. Department of Agriculture, stay at home order dates from Wikipedia, revenue from the Annual Survey of State and

⁵Remittance rules are not always set by states. For example in Alaska, these rules are set by localities. This may also be the case in other states where local governments are given substantial autonomy over their local tax systems because they are not administered by the state government.

Local Government Finances, tax base breadth from Beem and Bruce (2021), and tax rates from Agrawal (2014).

Table A.1: Data Sources

No	State	Source
1	Alabama*	Department of Revenue
2	Alaska*	Department of Commerce, Community, and Economic Development
3	Arizona [†]	Department of Revenue
4	Arkansas	Department of Finance and Administration
5	California	Department of Tax and Fee Administration
6	Colorado	Department of Revenue
7	Connecticut	Connecticut Open Data
8	Florida	Department of Revenue
9	Georgia	Georgia Tax Center
10	Hawaii	Department of Taxation
11	Idaho	State Tax Commission
12	Illinois	Standard Industrial Classification Code Reporting (Information Request)
13	Indiana*	STATS Indiana
14	Iowa	Department of Revenue
15	Kansas	Department of Revenue
16	Kentucky [†]	Office of State Budget Director
17	Louisiana*	Department of Revenue
18	Maine*	Revenue Services
19	Maryland	Comptroller
20	Massachusetts [†]	Department of Revenue
21	Michigan [†]	State Budget Office
22	Minnesota	Department of Revenue
23	Mississippi	Department of Revenue
24	Missouri	Department of Revenue
25	Nebraska	Department of Revenue
26	Nevada	Department of Taxation
27	New Jersey [†]	Department of the Treasury
28	New Mexico	Taxation and Revenue
29	New York	Open NY
30	North Carolina	Department of Revenue
31	North Dakota	Office of State Tax Commissioners
32	Ohio	Department of Taxation
33	Oklahoma	Tax Commission
34	Pennsylvania*	Department of Revenue
35	Rhode Island [†]	Department of Revenue
36	South Carolina*	Department of Revenue
37	South Dakota	Department of Revenue
38	Tennessee	Department of Revenue
39	Texas	Open Data Portal
40	Utah	State Tax Commission
41	Vermont	Department of Taxes
42	Virginia	University of Virginia Center for Economic and Policy Studies
43	Washington	Department of Revenue
44	West Virginia	Tax Department
45	Wisconsin	Department of Revenue
46	Wyoming	Department of Revenue

This table gives the data sources where we obtain data from each state. Seven states provide data at the county and/or municipality level but only at the annual frequency (*). Six states do not provide any county or municipality-level data ([†]). Four states, including Delaware, Montana, New Hampshire, and Oregon, have no sales taxes of any kind.

Table A.2: Data Types Released by Each State

	Taxable Sales		Local Revenue	
	Month	Quarter	Month	Quarter
County	1. Colorado 2. Florida 3. Hawaii 4. Kansas 5. Maryland 6. Nebraska 7. Nevada 8. New Mexico 9. North Carolina 10. Ohio 11. Oklahoma 12. South Dakota 13. Tennessee 14. Texas 15. Vermont 16. Wyoming	17. California 18. Illinois 19. Missouri 20. New York 21. North Dakota 22. Virginia 23. Washington	1. Arkansas 2. Florida 3. Georgia 4. Kansas 5. Minnesota 6. Nevada 7. New Mexico 8. North Carolina 9. Ohio 10. Oklahoma 11. Tennessee 12. Texas 13. Utah 14. Virginia 15. Washington 16. Wisconsin 17. Wyoming	18. Illinois
Municipality	1. Colorado 2. Nebraska 3. New Mexico 4. Oklahoma 5. South Dakota 6. Vermont	7. California 8. Connecticut 9. Illinois 10. Iowa 11. Missouri 12. North Dakota 13. Washington	1. Arkansas 2. Georgia 3. Kansas 4. Minnesota 5. Mississippi 6. Nebraska 7. New Mexico 8. Oklahoma 9. South Dakota 10. Utah 11. Washington	12. Illinois 13. West Virginia

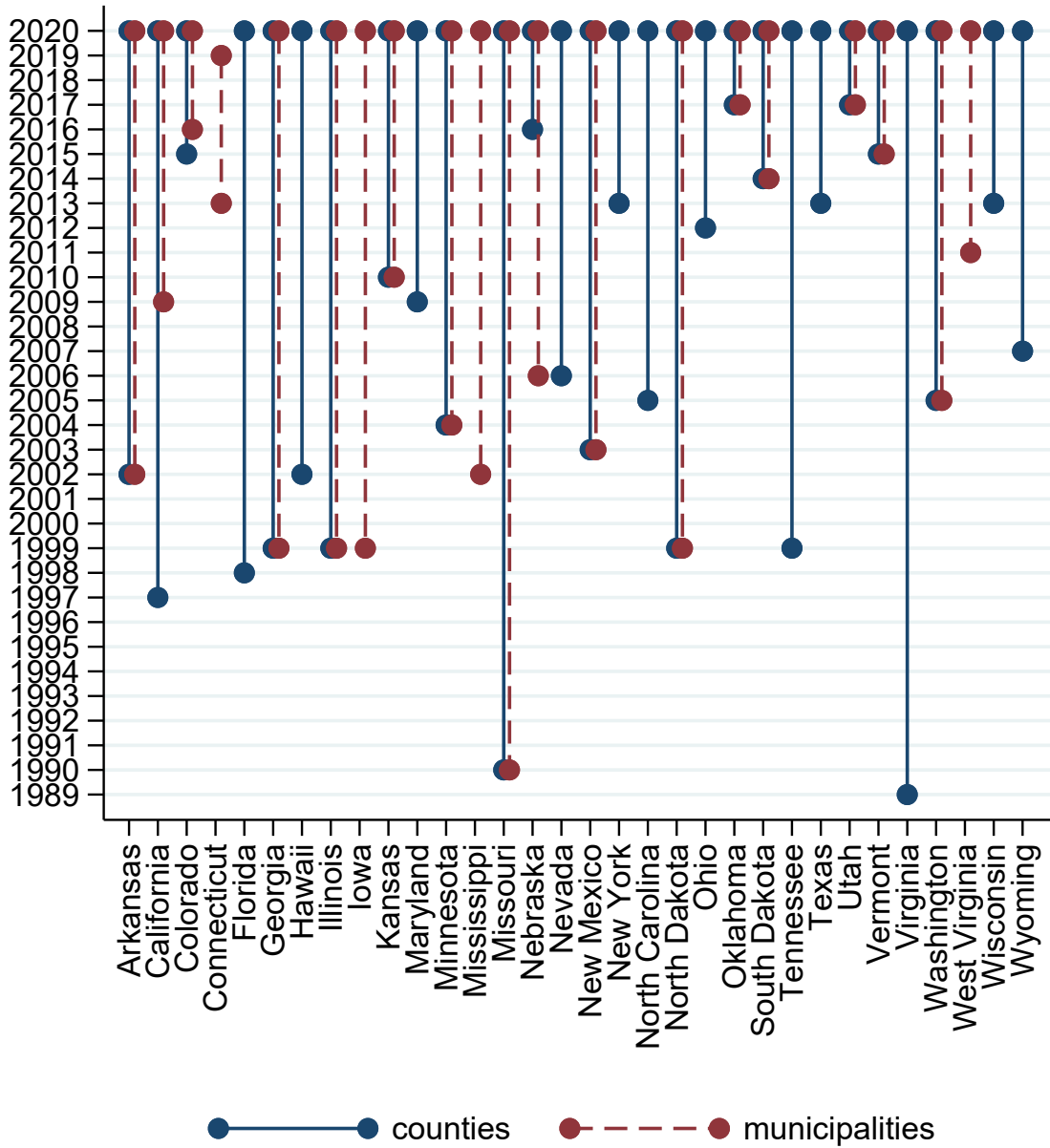
For states that release local data, this table gives the type and frequency. Although we can aggregate monthly-level data to the quarterly frequency, we do not repeat these states in those columns. Even though a state releases data at the municipal level, that does not necessarily mean the state allows for local sales tax autonomy at that level of government. For example, in Georgia, municipalities, other than the city of Atlanta, do not have sales taxes.

Table A.3: Critical Dates of States Adopting Rules Before the Pandemic

State	Economic Nexus	Marketplace Facilitator	Stay at Home Order
Alabama	Oct-18	Jan-19	4-Apr-20
Alaska*	NA	NA	28-Mar-20
Arizona	Oct-19	Oct-19	31-Mar-20
Arkansas	Jul-19	Jul-19	
California	Apr-19	Oct-19	19-Mar-20
Colorado	May-19	Oct-19	26-Mar-20
Connecticut	Dec-18	Dec-18	23-Mar-20
Delaware*	NA	NA	24-Mar-20
Florida			3-Apr-20
Georgia	Jan-19	Apr-20	3-Apr-20
Hawaii	Jul-18	Jan-20	25-Mar-20
Idaho	Jun-19	Jun-19	25-Mar-20
Illinois	Oct-18	Jan-20	21-Mar-20
Indiana	Oct-18	Jul-19	25-Mar-20
Iowa	Jan-19	Jan-19	
Kansas	Oct-19		30-Mar-21
Kentucky	Oct-18	Jul-19	26-Mar-20
Louisiana			23-Mar-20
Maine	Jul-18	Oct-19	2-Apr-20
Maryland	Oct-18	Oct-19	30-Mar-20
Massachusetts	Oct-19	Oct-19	24-Mar-20
Michigan	Oct-18	Jan-20	24-Mar-20
Minnesota	Oct-18	Oct-18	27-Mar-20
Mississippi	Sep-18		3-Apr-20
Missouri			6-Apr-20
Montana*	NA	NA	28-Mar-20
Nebraska	Apr-19	Apr-19	
Nevada	Oct-18	Oct-19	1-Apr-20
New Hampshire*	NA	NA	27-Mar-20
New Jersey	Nov-18	Nov-18	21-Mar-20
New Mexico	Jul-19	Jul-19	24-Mar-20
New York	Jun-18	Jun-19	22-Mar-20
North Carolina	Nov-18	Feb-20	30-Mar-20
North Dakota	Oct-18	Oct-19	
Ohio	Aug-19	Sep-19	23-Mar-20
Oklahoma	Nov-19	Nov-19	2-Apr-20
Oregon*	NA	NA	23-Mar-20
Pennsylvania	Jul-19	Jul-19	1-Apr-20
Rhode Island	Jul-19	Jul-19	28-Mar-20
South Carolina	Nov-18		7-Apr-20
South Dakota	Nov-18	Mar-19	
Tennessee	Oct-19		2-Apr-20
Texas	Oct-19	Oct-19	2-Apr-20
Utah	Jan-19	Oct-19	
Vermont	Jul-18	Jun-19	25-Mar-20
Virginia	Jul-19	Jul-19	30-Mar-20
Washington	Oct-18	Oct-18	23-Mar-20
West Virginia	Jan-19	Jul-19	23-Mar-20
Wisconsin	Oct-18	Jan-20	25-Mar-20
Wyoming	Feb-19	Jul-19	

Source: Nexus and Facilitator dates are from Fox et al. (2022). Stay-at-home order dates are from Wikipedia. No date listed implies no action by the state until after April 2020. Some states have subsequently adopted facilitator and nexus laws, such as MS in July and TN in October. NA denotes states without a sales tax.

Figure A.1: Timeline of Our Data



Notes: This figure shows the earliest and latest year we were able to obtain local high-frequency sales tax data for each state. The series extends partially into 2021, which we do not include in the figure given it is not a full year.

B Appendix (Online Only): Motivation and Summary Statistics

This appendix motivates various asymmetries between small and large jurisdictions and summary statistics for small and large jurisdictions.

B.1 Motivation of Small and Large Asymmetries

Large agglomerated jurisdictions raise a disproportionate share of local sales tax revenue. To show this, we use Census of Governments data to tabulate the share of revenue raised by large jurisdictions. For purposes of this section, given we include municipal tax revenues, we use population as a proxy for the extent of agglomerations within jurisdictions. We then calculate the share of tax revenue raised in the top 1% and top 10% of jurisdictions based on size. This is a way to measure the interjurisdictional “inequalities” in tax revenue across jurisdictions. Figure B.1 indicates that the top 1% of jurisdictions raise approximately 65% of all local sales tax revenues while the top 10% of jurisdictions raise approximately 90%. These shares are in excess of what would occur if tax revenues in a jurisdiction were proportionate to aggregate income or population. As can be seen from the Figures, the share raised by the top jurisdictions has been declining since the 1970s, perhaps due to the shift toward services, or more recently, to e-commerce. Rather than show this in shares, Figure B.2 shows this size-based asymmetry in per capita terms. The top 10% of jurisdictions raise three times more per capita in tax revenue than all other jurisdictions. This asymmetry is even more pronounced if focusing on the bottom 50% of jurisdictions.

B.2 The COVID-19 Shock in Small and Large Jurisdictions

COVID-19 may have affected small and large jurisdictions in different ways. To demonstrate this, Figure B.3 shows an event study that indicates that the unemployment rate rose more in large counties than in small counties relative to the omitted March months prior to the pandemic. But the figure makes it clear that even in small counties, unemployment increased. Thus, if revenues were negatively correlated with unemployment, revenues should have fallen in both small and large counties. At the same time, rising unemployment may not have had a negative effect on demand and sales tax revenues because enhanced unemployment insurance benefits resulted in personal income increases for a significant share of the population. Moreover, while the highest income households did not receive stimulus payments, many households well above the median received them. These latter policies may have increased revenue growth even as GDP briefly declined and employment fell for a longer time.

Figure B.4 shows PPP and CARES data by jurisdiction size as constructed using the description in our data appendix. As the figure makes clear, larger jurisdictions received more CARES and PPP per capita. All else equal, this should mute the fall of tax revenues in large jurisdictions.

B.3 Constructing Small and Large Jurisdictions

As discussed in the main text, we partition jurisdictions into small, medium, and large jurisdictions. Table B.1 shows the mean number of establishments and the mean population in small and large jurisdictions. At the municipal level, we do not have establishment data and thus only show the population. As can be seen, small counties only have about 70 establishments while large counties have over 3,000. The sample of states releasing monthly rather than quarterly data generally have smaller establishment/population counts, suggesting the size asymmetries in monthly data understate those of the overall population.

At the municipal level, as discussed in the prior appendix, we need to name merge towns in the revenue data to the Census in order to obtain population. Furthermore, while many states release municipal data, they might exclude some observations. To demonstrate if the data released are representative, Figure B.5 shows a density plot of the population distribution of all places in the Census and the sample of places we have in the revenue data. As can be seen, our sample is slightly larger on average. But, this is likely the case because Census places include formal jurisdictions that are incorporated plus informal jurisdictions the Census uses for statistical purposes. These latter jurisdictions are likely unincorporated and do not have taxing authority.

Table B.2 shows the differences in historical tax rates by jurisdiction size. As we do not have access to data on current local tax rates, we use historical data as a proxy. Traditional models of tax competition usually imply that larger and more agglomerated jurisdictions set higher tax rates. This result is confirmed at the municipal level. But, these data indicate this pattern is not necessarily true at the county level. Of course, this figure does not partial out any other characteristics. And, as a result, if we were to control for other factors (access to other tax instruments, preferences for public goods, etc.) then the size result may reappear at the county level.

Table B.3 shows differences in observable characteristics by jurisdiction size. To do this, we present summary statistics on jurisdiction demographics from the 2017 American Community Survey. We also present differences in each of the variables used to construct Figure 4. One noticeable similarity is that the number of establishments per capita is generally quite similar across jurisdictions. This fact, combined with the observation that large jurisdictions raise a disproportionate share of sales tax revenue, indicates that the establishments in small jurisdictions are likely much smaller stores.

Table B.1: Mean Number of Establishments and Population

Counties	Small		Large	
	Establishments	Population	Establishments	Population
Monthly				
Entire sample	70.7	12,123.1	3,453.9	506,108.8
Taxable sales	66.2	10,944.1	3,605.5	521,552.5
Revenues	75.2	13,983.5	3,512.0	508,812.9
Quarterly				
Entire sample	73.1	12,241.1	5,061.2	726,268.3
Taxable sales	70.1	11,610.4	5,319.5	762,928.4
Revenues	78.0	14,238.9	4,044.4	589,536.2
<hr/>				
Municipalities	Small		Large	
	Population		Population	
Monthly				
Entire sample	493.7		63,587.2	
Taxable sales	435.7		82,795.7	
Revenues	502.3		59,140.3	
Quarterly				
Entire sample	460.8		91,146.9	
Taxable sales	448.0		97,750.5	
Revenues	483.3		75,128.1	

This table gives the mean number of establishments with NAICS codes 44-45 Retail Trade, 72 Accommodation and Food Services, and 42 Wholesale Trade and the mean population by small and large jurisdiction size. We use these NAICS codes as they are most likely to be ones with sales taxes due on purchases, but the results are robust to using total establishments. Establishment data are only available for counties. Counties are partitioned based on the number of establishments, while municipalities are partitioned based on population counts.

Table B.2: Historical Tax Rates by Jurisdiction Size

	Counties		Municipalities	
	Mean Tax	Median Tax	Mean Tax	Median Tax
Large	0.94%	0.75%	1.75%	1.50%
Medium	1.16%	1.00%	1.42%	1.20%
Small	1.18%	1.00%	1.29%	1.00%

This table shows the mean and median tax rates in small, medium, and large jurisdictions. Unfortunately, we do not have access to current tax rates for all municipalities, so we use 2011 historical data on tax rates from Agrawal (2014). Jurisdiction sizes are partitioned in the same manner as in our regression specifications.

Table B.3: Summary Statistics of Small and Large Jurisdictions

	Small	Large
Counties		
Population	12,241.1	726,268.3
Black, %	8.0	13.6
Hispanic, %	8.6	16.8
Seniors, %	20.5	15.2
BA or above, %	13.0	24.0
Median income	49,453.8	69,431.6
Working outside of county, %	34.4	22.8
Annual sales tax revenue, \$1000s	1,536.7	141,100.0
Establishments	73.1	5,061.2
Establishments per 1,000	7.1	6.8
Grocery stores per 1,000	0.4	0.4
Accommodation establishments per 1,000	0.4	0.2
Durable goods stores per 1,000	0.1	0.3
CARES per capita	89.3	139.9
PPP per capita	1,025.2	1,706.8
Places of work change, %	-23.1	-32.4
Places of residence change, %	5.5	10.7
N	966	193
Municipalities		
Population	460.8	91,146.9
Black, %	6.2	8.9
Hispanic, %	5.0	24.6
Seniors, %	20.0	13.9
BA or above, %	10.5	23.5
Median income	48,295.1	74,290.7
Working outside of municipality, %	80.7	63.8
Annual sales tax revenue, \$1000s	79.9	14,576.5
N	3,435	679

This table shows summary statistics for small and large jurisdictions. We focus on key demographic variables plus each of the variables indicated in Figure 4. At the municipal level, we suppress any variables that are only defined at the county level in our analysis. All demographic data are from the 2017 American Community Survey while all other variables are sourced as in Figure 4. Jurisdiction sizes are partitioned in the same matter as in our regression specifications.

Figure B.1: Inequality in Tax Revenue by Jurisdiction Size

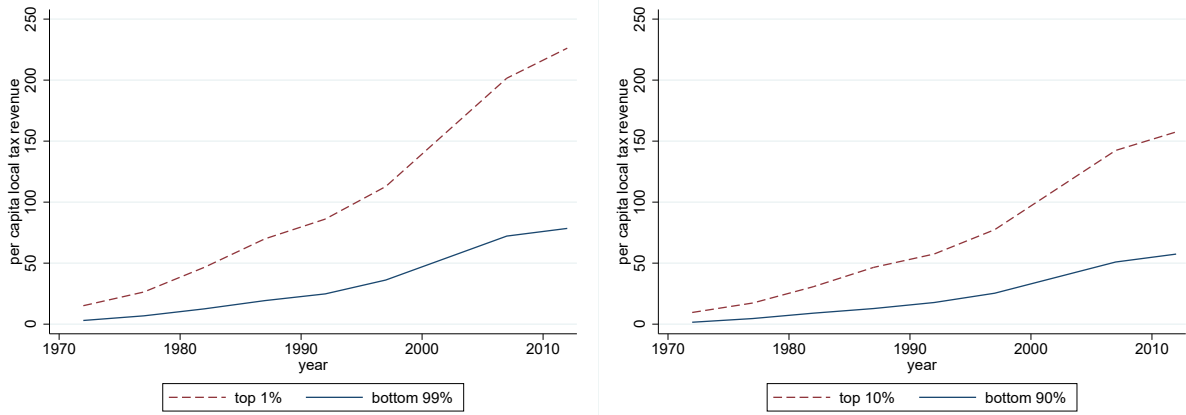


Notes: This figure shows the percent of municipal sales tax revenue in the U.S. raised by the largest and smallest jurisdictions in the country. The largest and smallest jurisdictions are split based on population into the top 1 percent [10 percent] and bottom 99 percent [90 percent] of jurisdictions for panel a [b]. To construct this figure we use all local governments in the Census of Governments. The top 10 percent of jurisdictions generally have more than 13,000 inhabitants, while the top 1% are relatively large cities. Source: Agrawal (2021) and Census of Governments Data.

Figure B.2: Per Capita Revenue Raised by Small and Large Jurisdictions

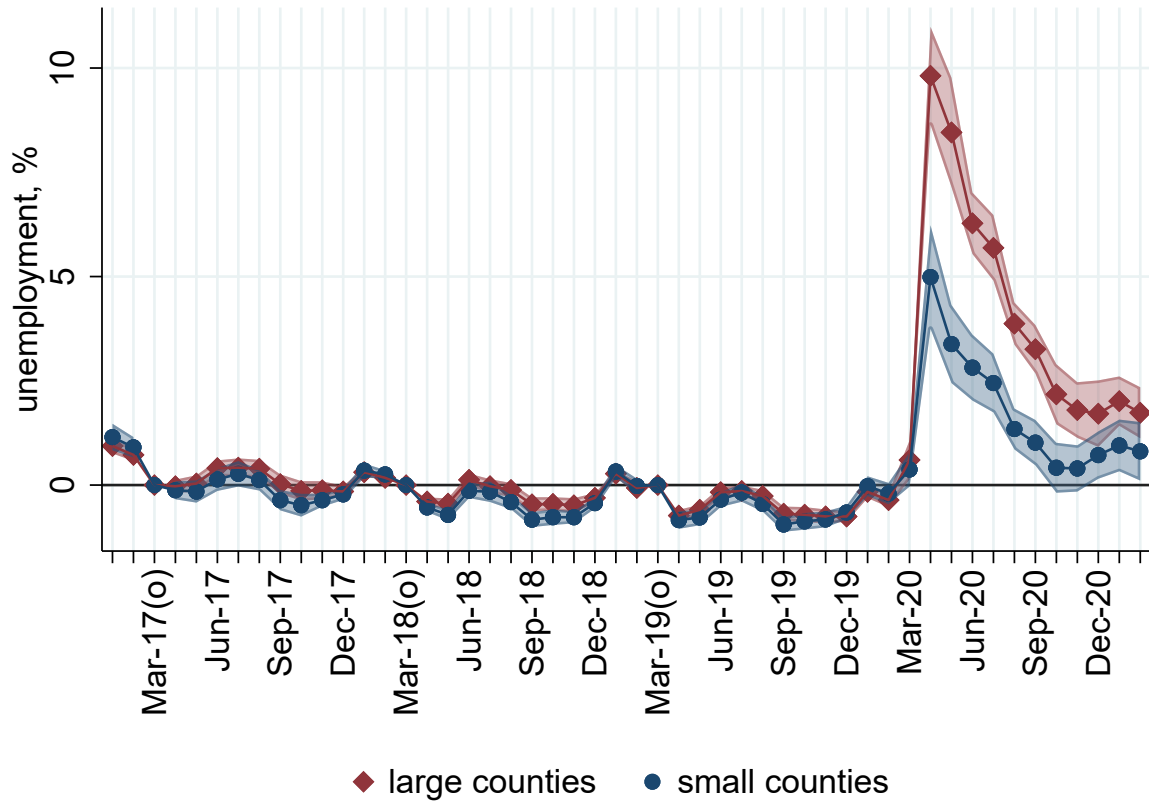
(a) Top 1%

(b) Top 10%



Notes: This figure shows the per capita municipal sales tax revenue in the U.S. raised by the largest and smallest jurisdictions in the country. Depending on the panel, the largest and smallest jurisdictions are split based on population into the top 10 [1] percent and bottom 90 [99] percent of jurisdictions. To construct this figure we use all local governments in the Census of Governments. Source: Census of Governments Data.

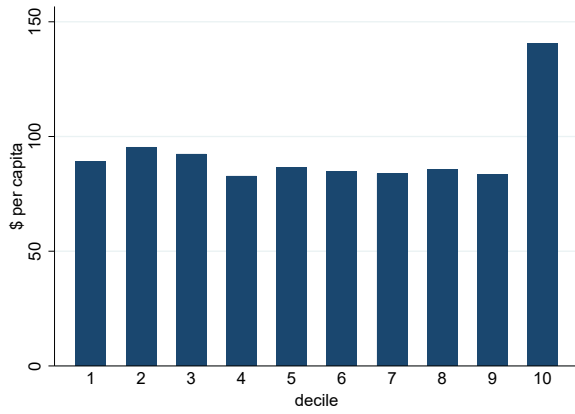
Figure B.3: Unemployment and the COVID-19 Shock



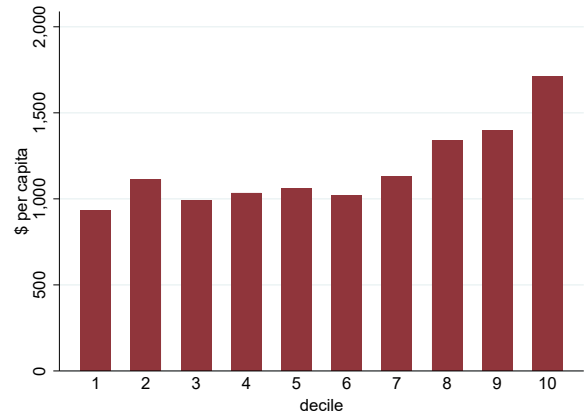
This figure shows an event study of the unemployment rate separately for large and small counties. We omit the March indicators from prior to the pandemic (denoted by “o” on the figure), so the figure shows the change in the unemployment rate relative to those periods. The unemployment rate is the overall not seasonally adjusted unemployment rate for small and large counties in our sample. The data are sourced from the Bureau of Labor Statistics.

Figure B.4: COVID-19 Relief Spending

(a) CARES per Capita By Size Decile

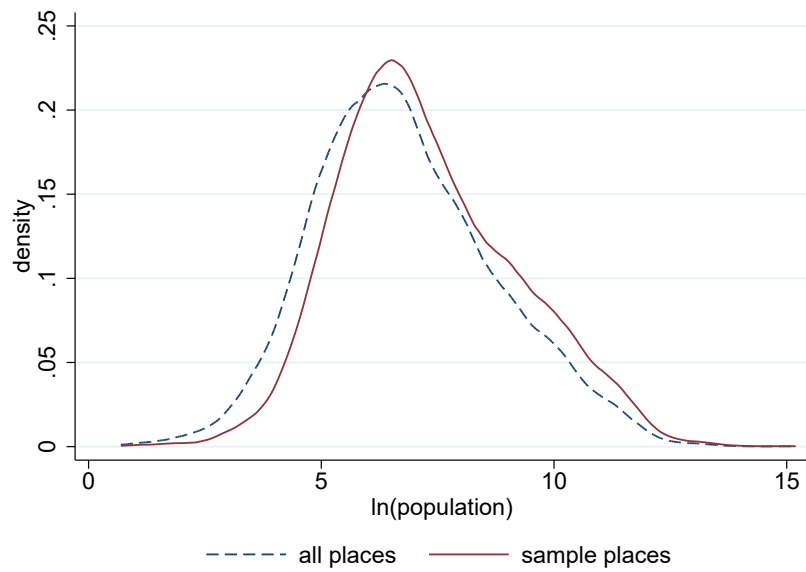


(b) PPP per Capita by Size Decile



This figure shows the per capita CARES and PPP spending by decile of county size. PPP and CARES transfers are aggregated by county. CARES transfers include transfers to all local governments within each county from the federal (primary awards) and state (subawards) governments.

Figure B.5: Census Places Versus Our Sample of Towns



Notes: This figure shows the full population distribution for all Census “places” and the population distribution of places in the tax dataset that we merge. Some Census places are purely statistical areas, so even if our tax dataset had full coverage, we would not expect the merge to be exact.

C Appendix (Online Only): Additional Results on the Role of Size

This appendix presents various extensions and robustness checks for our analysis documenting the asymmetric revenue effects by jurisdiction size.

C.1 Standard Errors

Table C.1 shows how the confidence intervals change depending on how we cluster standard errors. We present confidence intervals rather than standard errors because we include bootstrapped confidence intervals as well. The table first presents standard errors that are robust to heteroskedasticity and then proceeds to cluster at different levels (county, commuting zone, state by year, state by size, state). The final column clusters at the state level and uses a wild cluster bootstrap to obtain the confidence interval. As the table indicates, confidence bands increase in size as we cluster at higher levels. However, clustering at the state-by-size level results in almost identical standard errors to clustering at the state level. This evidence combined with the fact that we model within-state heterogeneity at the size level leads us to select the state-by-size level for clustering. This follows the best practices in Cameron and Miller (2015) to stop clustering when standard errors are unchanged. Moreover, because the number of states is different depending on the dataset we analyze, clustering at the state-by-size level avoids having a small number of clusters, allowing us to present standard errors without needing to bootstrap in some specifications and not others.

C.2 Size

In order to identify heterogeneous effects, we partition the sample into small, medium, and large jurisdictions. To identify the thresholds for this, we use a flexible data-driven procedure. First, we interact $post_t$ with a continuous function in jurisdiction size. We consider polynomial orders of degrees one through seven in addition to the log functional form. We use leave-one-out cross-validation to select the polynomial order, yielding a degree five polynomial. We then present in Panel (a) and (c) of Figure C.1 the marginal effects of $post_t$ evaluated at given percentiles of jurisdiction size. Although the polynomial order was selected via leave-one-out cross-validation, its parametric form may not be ideal. For this reason, we partition the sample into deciles based on jurisdiction size. We then interact those decile indicators with $post_t$ and plot the post-COVID-19 effect for each decile. Visual inspection of Panel (b) and (d) shows that the first five deciles have broadly similar effects,⁶ while the top decile has dramatically lower effects. The four

⁶The main exception is the bottom decile, which contains very small places.

deciles in the middle are somewhere in between. Given this visual inspection, we proceed with these three groupings, which facilitates the presentation of event studies over this more flexible approach.

Throughout the main text, we focus on small and large jurisdictions and suppress the results for middle-sized jurisdictions for simplicity. However, Figure C.2 shows the event studies for middle-sized jurisdictions. These event studies can be compared to Figure 3 in the main text. As can be seen, the effects are somewhere between small and large jurisdictions, but closer to the effects in small jurisdictions.

C.3 Robustness Checks

In the main text of the paper, we focus on the sample of towns and counties available in monthly data. The advantages of using monthly data are that (1) it shows the dynamic responses of revenue at a high-frequency and (2) it allows us to precisely eliminate the specific “abnormal” months when estimating the “longer-run” interactions.⁷ However, an advantage of quarterly data is that it allows us to focus on a larger sample of states because states that release monthly data can be aggregated to the quarterly frequency and then pooled with states that release quarterly data.

Figure C.3 shows the event studies after pooling monthly and quarterly data. Comparing the $post_t$ effects for small and large places in Figure C.3 with that of Figure 3 reveals broadly similar patterns. Slight differences in the magnitudes may emerge because, as shown in Table B.1, the sample of quarterly states have large cities that are bigger than those in the sample of monthly states (e.g., the quarterly sample adds in states like California and New York). Figure C.4 then conducts an analysis similar to Figure 4 for counties in the main text but using the pooled monthly and quarterly sample of states. Again, the results are broadly similar suggesting that while the sample of states are different, the overall conclusions in the main text remain.

Finally, Figure C.5 shows the same analysis but at the municipal level. One noticeable difference from the county analysis is that the estimates for large municipalities are generally not negative. Part of the reason for this is that the municipal-level distribution of jurisdictions is much more skewed than the county distribution. As a result, very large negative effects might not emerge until higher in the municipal population distribution. Another explanation is that at the municipal level, we use population as a proxy for retail agglomerations, and this may be an imperfect measure. For simplicity, we suppress the same figure for quarterly data, except to note that—like the county results—the quarterly and monthly samples are qualitatively similar.

⁷The monthly regressions reveal a downturn of revenues in March and April that are well below subsequent months. This allows us to classify the longer run as May-onward and the short run as March and April alone. A challenge in quarterly data is that these two months span two different quarters.

C.4 Interpreting the Magnitude of the Effects

While the analysis in terms of growth rates is intuitive, in this section, we benchmark the magnitudes of these effects in terms of additional expenditures. Table C.2 shows the amount of sales tax revenue raised in 2017 from the Census of Governments. To give an idea of how important the sales tax is based on jurisdiction size, we calculate the sales tax as a share of total own-source revenues. These data indicate the share of revenue raised by jurisdictions is increasing in size up until the eighth decile, where it levels off. Small jurisdictions raise 15% of revenue from the sales tax while larger jurisdictions raise 19%. Using these baseline revenue numbers we calculate the implied dollar change in revenue using our regression effects from estimation of (1).⁸ At the median U.S. teacher salary, each small jurisdiction could hire one additional full-time K-12 teacher. At the top of the distribution, the losses in large jurisdictions amount to a loss of over 100 full-time teachers. Given teacher salaries in rural jurisdictions are likely below the median, while they are above the median in large jurisdictions, we likely underestimate the effect in small places and overestimate it in large places.⁹ But, the purpose of this section is to simply show that the changes in growth rates imply non-trivial implications for the level of public services that can be provided.

⁸Note that because baseline revenue is from 2017, we will slightly underestimate the dollar increase in revenue from the COVID-19 shock because revenue was growing over the period 2017-2020. However, this effect is second order. We used the 2017 numbers in this calculation because that is the most recent year that tax revenue is released for all local governments. Recall, our regressions pool sales and revenue, assuming tax rates are constant over time.

⁹Obviously, we do not intend to imply that these revenue changes are linked to education spending. Jurisdictions could alter other expenditures or change the tax rates on the property tax base to achieve budget balance.

Table C.1: Standard Errors Using Different Clustering Approaches

	Robust	County	Commuting zone	StateXyear	StateXsize	State	Boot. state
Large	-0.044	-0.044	-0.044	-0.044	-0.044	-0.044	-0.044
	[-0.053,-0.035]	[-0.065,-0.024]	[-0.069,-0.019]	[-0.085,-0.004]	[-0.101,0.013]	[-0.105,0.017]	[-0.108,0.019]
Middle	0.039	0.039	0.039	0.039	0.039	0.039	0.039
	[0.033,0.045]	[0.029,0.049]	[0.027,0.051]	[0.015,0.063]	[0.010,0.067]	[0.009,0.068]	[0.011,0.071]
Small	0.054	0.054	0.054	0.054	0.054	0.054	0.054
	[0.049,0.060]	[0.044,0.064]	[0.043,0.066]	[0.025,0.083]	[0.027,0.081]	[0.028,0.080]	[0.027,0.081]
N	88436	88436	88312	88436	88436	88436	88436

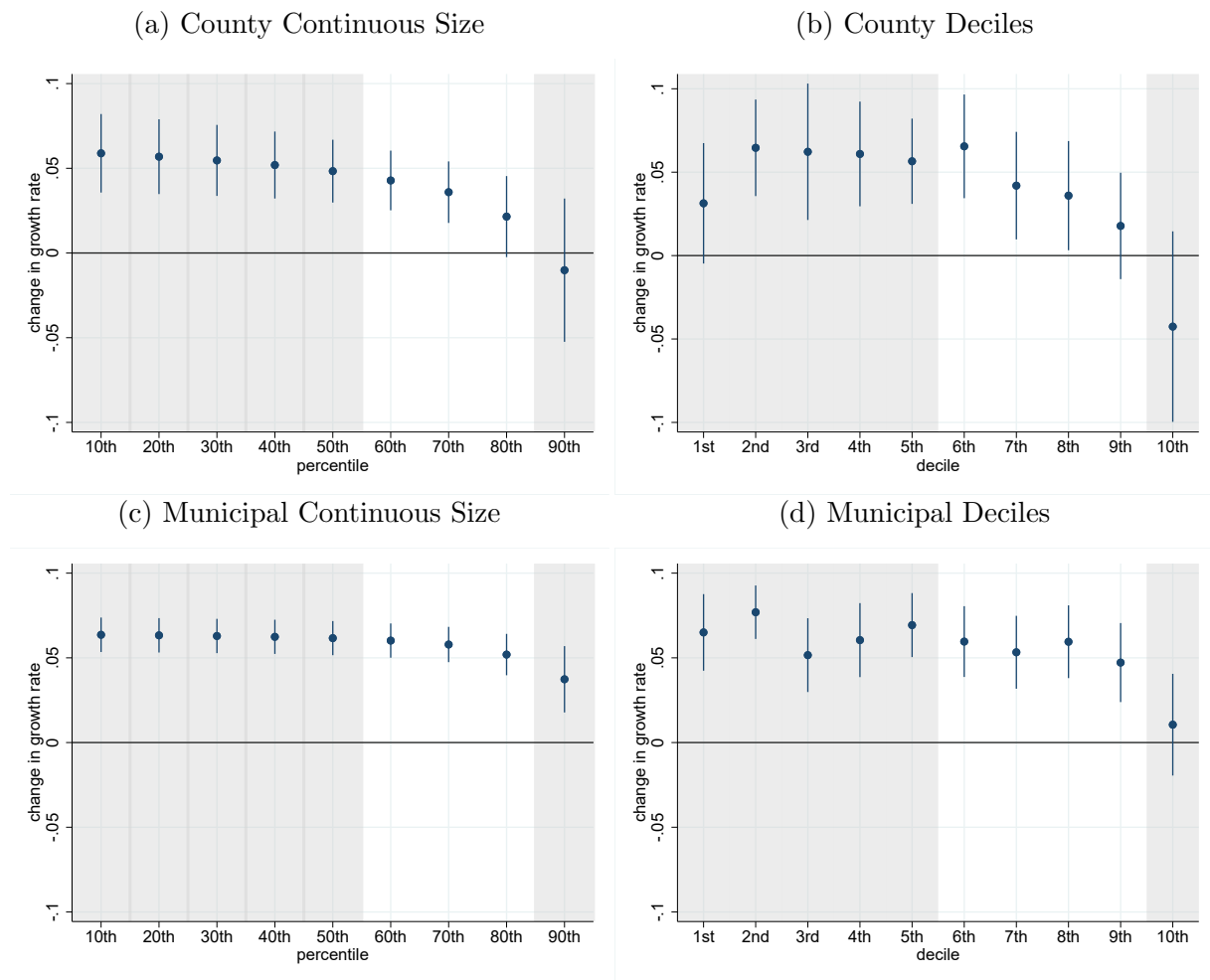
This table shows the sensitivity of the standard errors to different clustering approaches. To do this, we display 95% confidence intervals. In column 1, standard errors are robust to heteroskedasticity, in column 2, the standard errors are clustered at the county level, in column 3 standard errors are clustered at the commuting zone level, in column 4 standard errors are clustered at the state by year level, in column 5 standard errors are clustered at the state by size level, in column 6 standard errors are clustered at the state level, and in column 7 we use the wild cluster bootstrap with clusters at the state level. We follow the best suggested practices in Cameron and Miller (2015) and stop clustering at the highest level where standard errors are unchanged. Clustering at the state-by-size level also has the advantage of not having a small number of clusters.

Table C.2: Benchmarking the Magnitudes of the Effects

county decile/size	sales tax rev, 1000s	own-source rev, 1000s	% sales tax	rev post coef.	change in rev, 1000s	teachers, med. salary
1	448	5606	8%	0.031	14	0.2
2	834	6975	12%	0.065	54	0.9
3	1417	10,621	13%	0.062	88	1.4
4	2246	13,754	16%	0.061	137	2.2
5	3527	19,945	18%	0.057	199	3.2
6	5057	26,533	19%	0.065	331	5.3
7	7193	33,289	22%	0.042	302	4.8
8	12,271	66,278	19%	0.036	441	7.0
9	27,449	140,813	19%	0.018	488	7.8
10	154,184	826,855	19%	-0.043	-6565	-104.4
Small	1,691	11,404	15%	0.054	91	1.5
Medium	12,980	66,144	20%	0.039	506	8.1
Large	153,465	822,701	19%	-0.044	-6752	-107.4

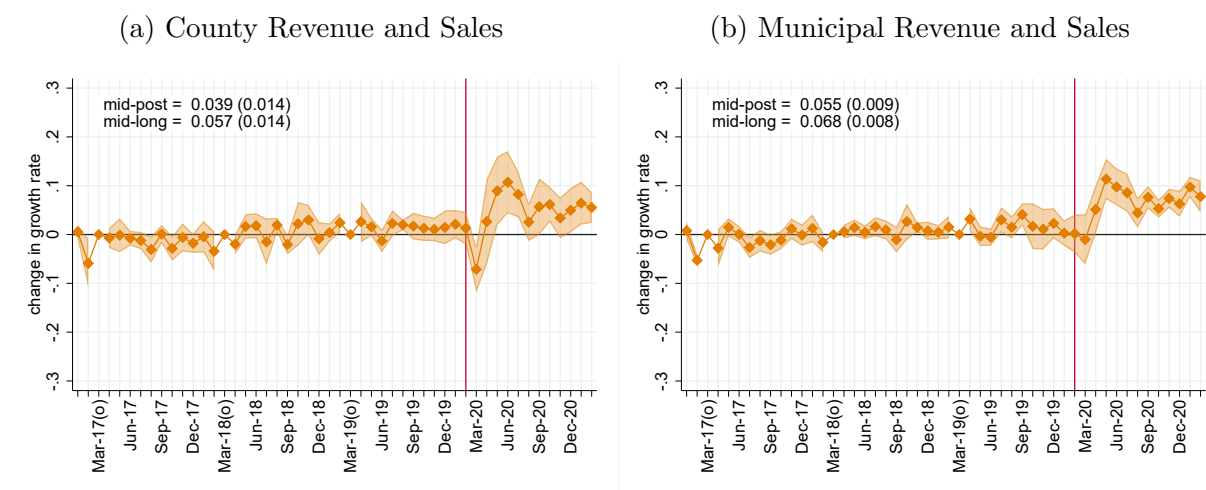
This table shows, by decile of jurisdiction size, the number of teachers that a jurisdiction could finance with the gains or losses we estimate. The first ten rows show results where our size indicators are based on a decile of population size, while the last three rows correspond to the size indicators used in our main specifications. The first column shows the decile of county size, the second column shows mean sales tax revenue in thousands of dollars, the third column gives own-source revenue in thousands of dollars from the 2017 Annual Survey of State and Local Government Finances, and the fourth column is the percent of own-source tax revenue that the sales tax raises. Then, the fifth column shows the regression marginal effect on the interaction of decile by post. The sixth column shows the implied growth in (annual) sales tax revenue based on this marginal effect. The final column then translates this growth in revenue into the number of teachers the county could hire using this revenue. To calculate this we assume all counties can hire a teacher at the median salary in the United States. If rural teacher salaries are lower than the median and urban teacher salaries higher, we underestimate the number of teachers in rural jurisdictions and overestimate it in absolute value in urban jurisdictions.

Figure C.1: Growth in Local Revenue: Continuous and Nonparametric Sizes



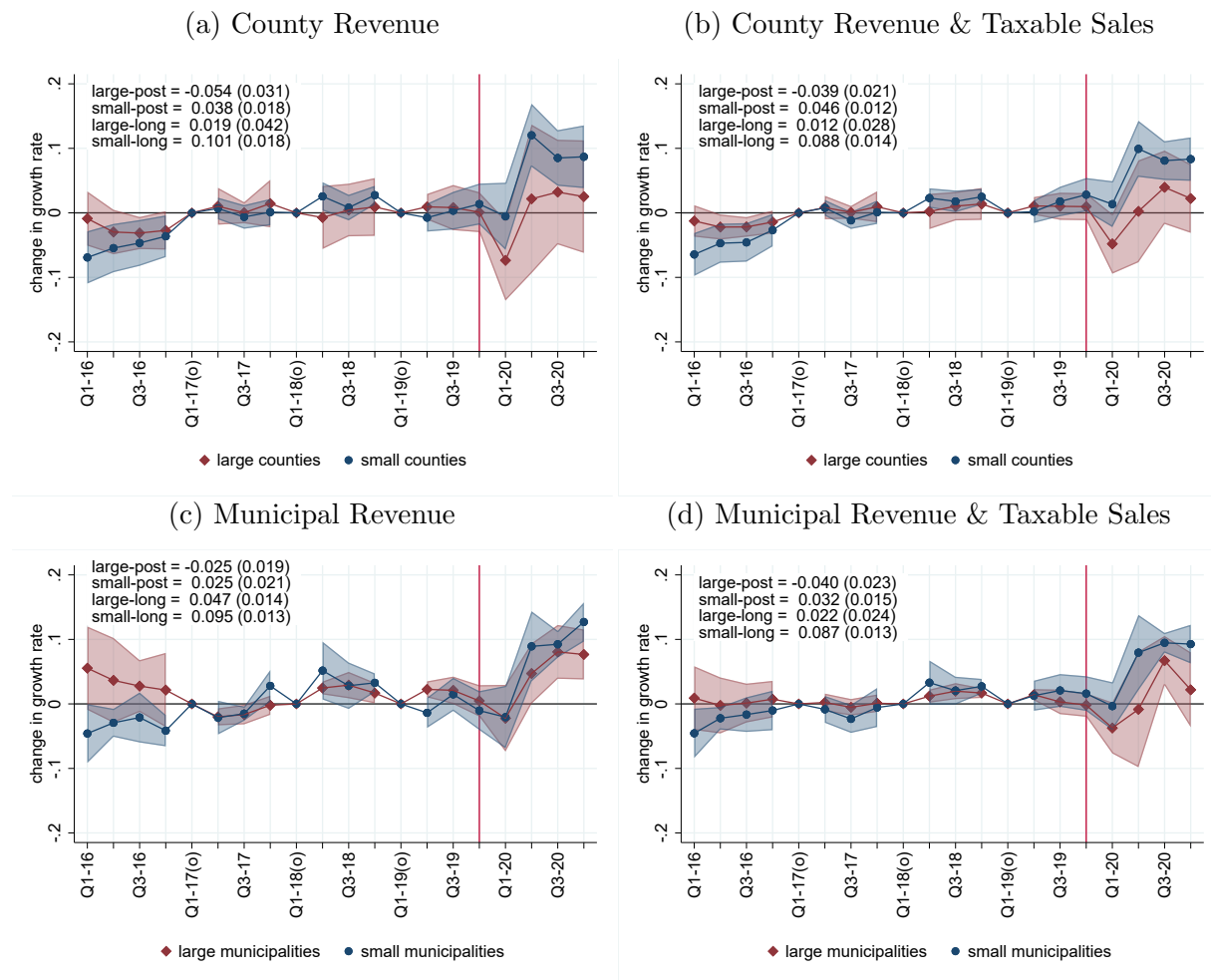
This figure shows estimation of (1) except replacing a single indicator for size with more flexible specifications. The dependent variable is the year-over-year growth rate in county and municipal sales tax revenue or taxable sales. We focus on states that release monthly data in this figure. In panels (a) and (c), we include a polynomial of degree five (selected through leave-one-out cross-validation) fully interacted with an indicator for $post_t$ (one if March 2020 through February 2021). In panels (b) and (d), we divide jurisdictions into deciles based on size. The interpretation of the effects is the change in the growth rate after COVID-19 relative to its pre-reform baseline. Marginal effects for specifications using a continuous function are evaluated at the given percentiles of the size distribution. Shaded areas denote 95% confidence intervals with standard errors clustered at the state-by-size level.

Figure C.2: The Effect on Mid-Sized Jurisdictions



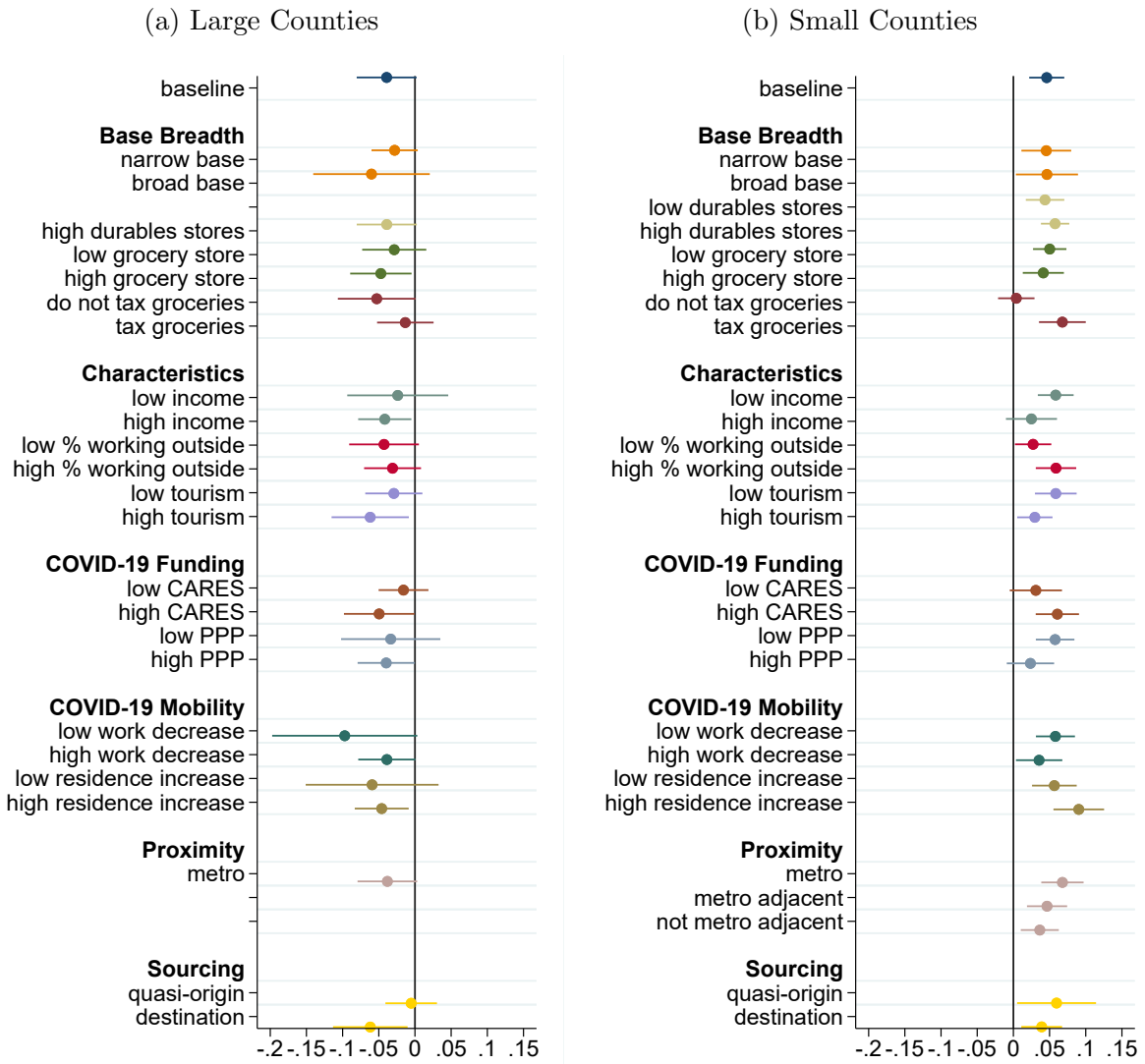
This figure shows estimation of (1) where the dependent variable is the year-over-year growth rate in county and municipal sales tax revenue or taxable sales. The interpretation of the presented effects is the change in the growth rate relative to the omitted periods (denoted by “o” on the graph). Each figure plots the effect for mid-sized jurisdiction, which are omitted from our main figures in the text. All effects can be interpreted as the change in the growth rate relative to the omitted periods (denoted by “o” on the graphs). Panel (a) shows the growth rate for counties, while panel (b) plots the growth rate for municipalities. Results for taxable sales and revenues can be pooled if tax rates do not change over time and other components of revenue (e.g., late fees or penalties) are negligible. The thresholds for middle-sized jurisdictions are defined in the text. Shaded areas denote 95% confidence intervals with standard errors clustered at the state-by-size level. Text boxes present the results of (1) with a single $post_t$ (one if March 2020 - February 2021) or $long_t$ (one if May 2020 - February 2021) indicators. In the latter expression, we include a $short_t$ indicator, so the interpretation on $long_t$ is relative to pre-COVID-19. The changes in growth rates for small and large jurisdictions are reported alongside standard errors clustered at the state-by-size level.

Figure C.3: Growth in Revenue and Transactions: Quarterly Data



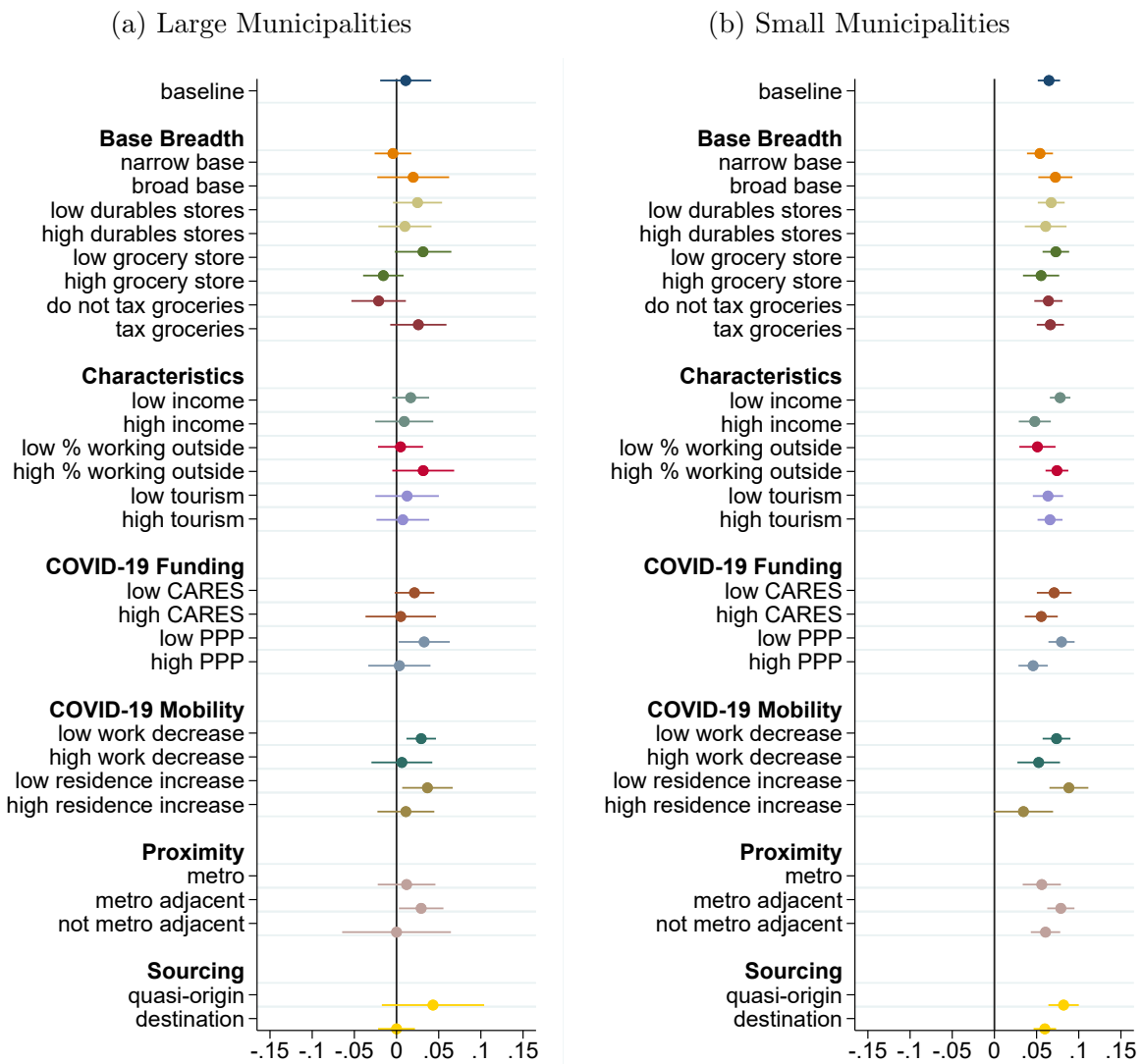
This figure shows estimation of (1) where the dependent variable is the year-over-year growth rate in county and municipal taxable sales. The only difference from Figure 3 is that it uses quarterly data instead of monthly data. Each figure plots the effect separately by size with red diamonds denoting large jurisdictions and blue circles denoting small jurisdictions. The interpretation of the presented effects is the change in the growth rate relative to the omitted periods (denoted by “o” on the graph). Panel (a) shows the growth rate for county tax revenue, while panel (b) plots the growth after pooling states that release revenue and taxable sales. Panel (c) shows the growth rate for municipal tax revenue, while panel (d) plots the growth after pooling states that release revenue and taxable sales. Results for taxable sales and revenues can be pooled if tax rates do not change over time and other components of revenue (e.g., late fees or penalties) are negligible. The thresholds for small and large jurisdictions are defined in the text. Shaded areas denote 95% confidence intervals with standard errors clustered at the state-by-size level. Text boxes present the results of (1) with a single $post_t$ (quarters 1-4 of 2020) or $long_t$ (quarters 2-4 of 2020) indicators. In the latter expression, we include a $short_t$ indicator, so the interpretation on $long_t$ is relative to pre-COVID-19. The changes in growth rates for small and large jurisdictions are reported alongside standard errors clustered at the state-by-size level.

Figure C.4: County Heterogeneity by Various Characteristics, Quarterly Data



This figure shows the change in the year-over-year growth rate in county sales tax revenue or taxable sales in the post period (2020) for different types of large and small jurisdictions. We construct indicators for whether the jurisdiction is below (low) and above (high) the median separately for various metrics. We then interact this with the $post_t$ and $size_i \times post_t$ variables in the regression. The metric of base breadth is the ratio of the tax base to income as discussed in Beem and Bruce (2021). For durable goods stores, grocery stores, and tourism, we use the number of establishments per capita in NAICS codes 442 Furniture and Home Furnishings Stores / 443 Electronics and Appliance Stores, 445 Food and Beverage Retailers, and 721 Accommodation, respectively. All large counties have an above-median number of durable goods stores per capita. Data on state and local grocery taxes are from the Center on Budget and Policy Priorities (Figuroa and Legendre 2020). Income and the share of workers working outside the county of residence come from the American Community Survey. PPP and CARES transfers are aggregated by county and standardized by population. CARES transfers include transfers to all local governments within each county from the federal and state governments. Changes in places of work and residence are derived from cell phone data. U.S. Department of Agriculture metro designation is used; all large counties belong to a metro area. Finally, data on sourcing rules are from Avalara; quasi-origin sourcing can be used for inter- and/or intra-state sales. Spikes denote 95% confidence intervals with standard errors clustered at the state-by-size level. We do not present similar results for $long_t$ but have verified that those effects are rightward shifts of the estimates in this figure.

Figure C.5: Municipal Heterogeneity by Various Characteristics, Monthly Data



This figure shows the change in the year-over-year growth rate in municipal sales tax revenue or taxable sales in the post period (March 2020 - February 2021) for different types of large and small jurisdictions. We focus on states that release monthly data in this figure. We construct indicators for whether the jurisdiction is below (low) and above (high) the median separately for various metrics. We then interact this with the $post_t$ and $size_i \times post_t$ variables in the regression. The metric of base breadth is the ratio of the tax base to income as discussed in Beem and Bruce (2021). For durable goods stores, grocery stores, and tourism, we use the county number of establishments per capita in NAICS codes 442 Furniture and Home Furnishings Stores / 443 Electronics and Appliance Stores, 445 Food and Beverage Retailers, and 721 Accommodation, respectively. Data on state and local grocery taxes are from the Center on Budget and Policy Priorities (Figuroa and Legendre 2020). Income and the share of workers working outside the place of residence come from the American Community Survey. PPP and CARES transfers are aggregated by county and standardized by population. CARES transfers include transfers to all local governments within each county from the federal and state governments. County changes in places of work and residence are derived from cell phone data. U.S. Department of Agriculture county metro designation is used. Finally, data on sourcing rules are from Avalara; quasi-origin sourcing can be used for inter- and/or intra-state sales. Spikes denote 95% confidence intervals with standard errors clustered at the state-by-size level.

D Appendix (Online Only): Additional Results on Remittance Rules

In this section, we conduct various robustness checks for the analyses that exploit economic nexus and Marketplace Facilitator laws.

Figure D.1 shows event studies from a triple interaction model that interacts time indicators with $size_i$ and an indicator for the presence of economic nexus or Marketplace laws. Relative to the policy specification in the text, this model allows us to trace out the high frequency dynamic responses. Unlike the marginal effects presented in Figure 5 of the text, these specifications do not account for both economic nexus and Marketplace laws in the same regression, but rather simply include a single indicator. For this reason, the marginal effects in the upper left panel are not directly comparable to those in Figure 5. They are simple event studies by economic nexus or Marketplace laws, not accounting for the presence of the other laws.

Figure D.1.a/b compares nexus versus non-nexus states while Figure D.1.c/d compares states with and without Marketplace Facilitator laws. Unlike states with economic nexus, no-nexus large jurisdictions saw large decreases and this decline persisted through the year-end. These large declines did not happen in nexus states. Small jurisdictions saw no short-run declines and revenue growth in the longer term.

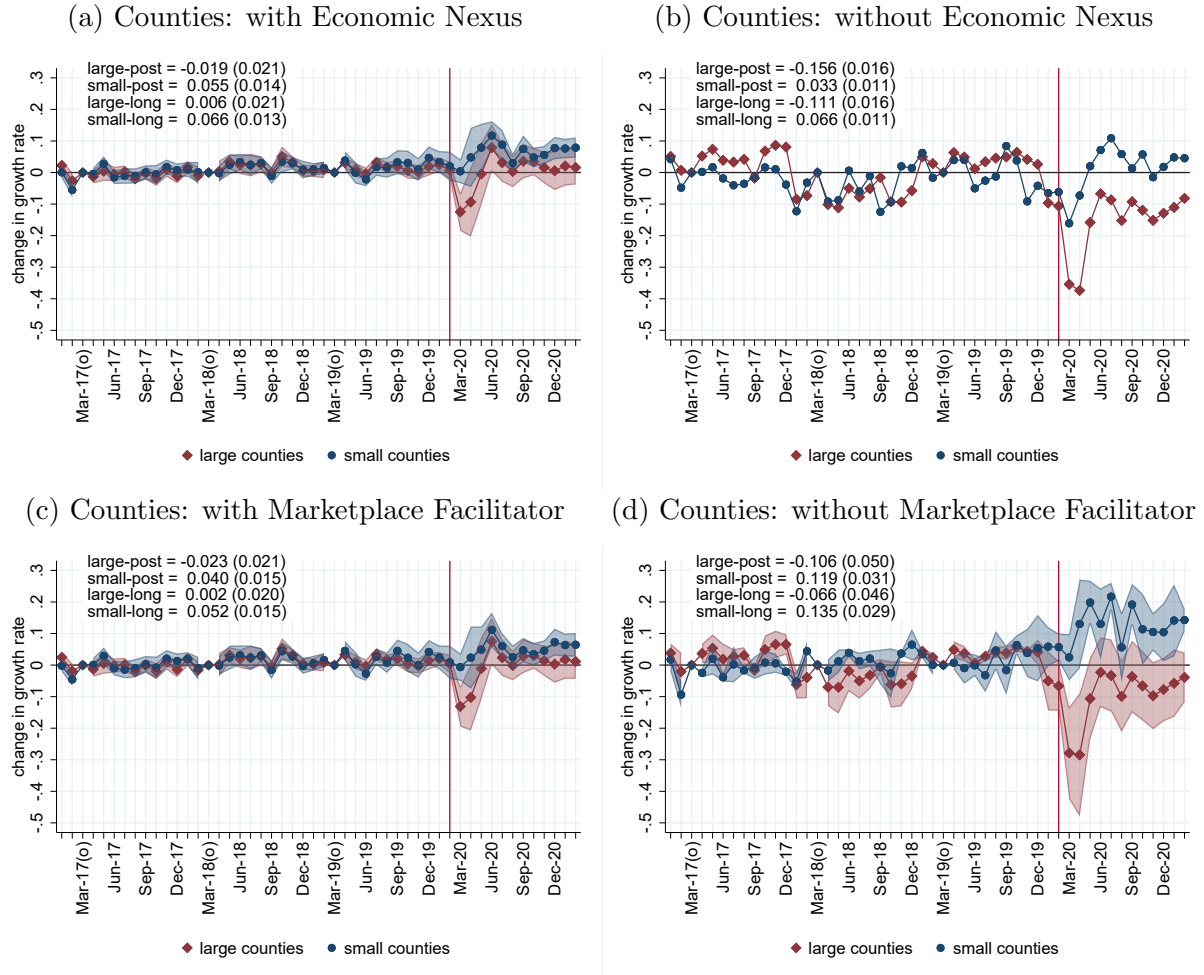
As discussed in the prior appendix, we wish to show all results are robust to using quarterly data. At the municipal level, there were not any states without economic nexus that released monthly municipal data. But, when aggregating to the quarterly level, we do have municipal data in states without economic nexus. Figure D.2 shows the results using this quarterly sample. As can be seen, the results at the county level are almost identical to those in the text. At the municipal level, the average $post_t$ effect diverges more from the “longer-term” effect in the quarterly sample. This is consistent with the quarterly sample adding states with very larger urban areas, such as New York and California. At the municipal level, no nexus states generally have lower growth rates than nexus states, consistent with economic nexus enforcing retail sales taxes on e-commerce. But, the magnitudes of these differences are much smaller than at the county level.

Finally, we address possible concerns about the limited number of states without economic nexus. We randomly change the nexus policy of a single state in our dataset. This allows us to compare our baseline results with a limited number of non-adopting states to a sample where any one other state is randomly assigned to not have nexus. Because nexus interacts with Marketplace laws, we randomly assign Marketplace Facilitator laws to the remaining states with nexus.¹⁰ After doing this five hundred times, we estimate our regression model for each random assignment. Finally, Figure D.3 shows the

¹⁰To do this, we randomly assign the policies at the state level and not at the locality level.

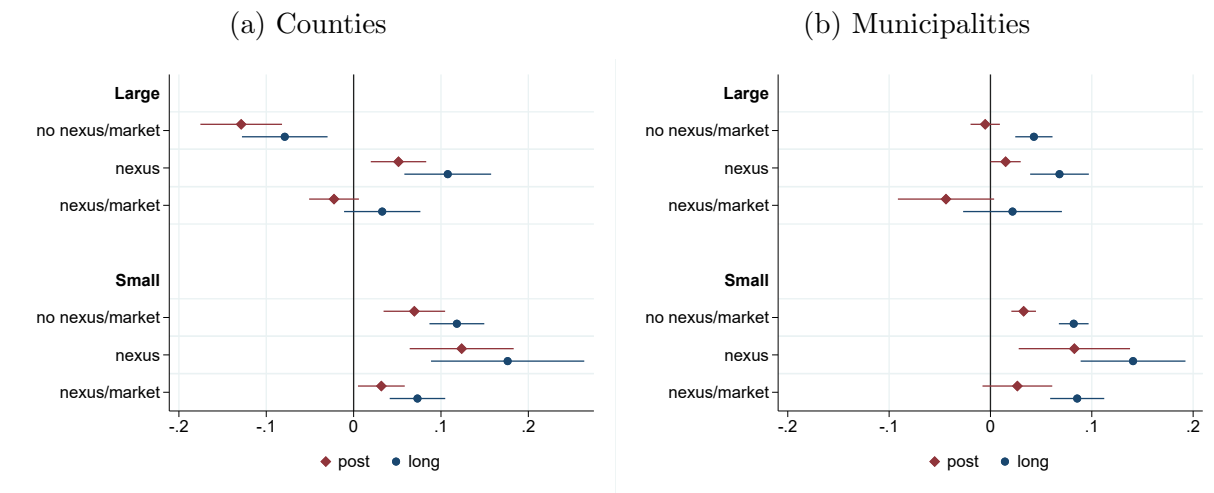
results. The figure reveals that the distribution of marginal effects for small jurisdictions is rightward-shifted relative to the distribution for large jurisdictions. This is consistent with small jurisdictions seeing revenue gains relative to large jurisdictions. Second, the effects we estimate are far in the tails of the marginal effects from those random assignments. We conclude that the effect of economic nexus is not just due to non-adopting states being different on other dimensions.

Figure D.1: Growth in Local Revenue: Effect of Online Sourcing Rules by Month



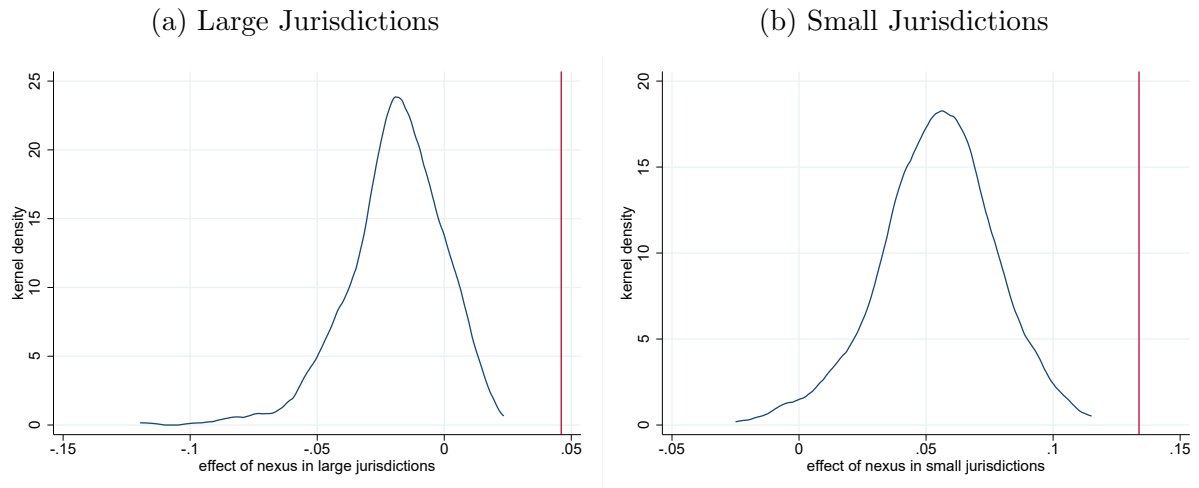
Panel (a) shows the change in the growth rate for county tax revenue or taxable sales in states that have economic nexus, Panel (b) shows the change in the growth rate in states that do not have economic nexus, Panel (c) shows the change in the growth rate for county tax revenue or taxable sales in states that have Marketplace Facilitator legislation, while panel (d) shows the change in the growth rate for county tax revenue or taxable sales in states that do not have Marketplace Facilitator legislation. All growth rate changes are relative to the omitted periods (denoted by “o” on the graphs). Shaded areas denote 95% confidence intervals with standard errors clustered at the state-by-size level. Confidence intervals are omitted from panel (b) because in monthly data we only have a small number of states without nexus and the event study confidence bands become large when estimating so many event indicators. Text boxes present the results of (1) with a single $post_t$ (one if March 2020 - February 2021) or $long_t$ (one if May 2020 - February 2021) indicators. In the latter expression, we include a $short_t$ indicator, so the interpretation on $long_t$ is relative to pre-COVID-19. The changes in growth rates for small and large jurisdictions are reported alongside standard errors clustered at the state-by-size level.

Figure D.2: Nexus Effects, Quarterly Data



This figure shows the change in the year-over-year growth rate in county and municipal sales tax revenue or taxable sales in the post period (2020) separately by size in states that do not have economic nexus or Marketplace Facilitator legislation, states that only have economic nexus, and states that have both institutions. The only difference from Figure 5 is that it uses quarterly data instead of monthly data. The thresholds for small and large jurisdictions are defined in the text. Spikes denote 95% confidence intervals with standard errors clustered at the state-by-size level.

Figure D.3: Random Assignment of Nexus Policies



Notes: The left panel focuses on large counties while the right panel focuses on small counties. The red vertical line in the graph shows the estimated effect in nexus states as estimated in Figure 5. The density function shows the distribution of the same estimated effects after we randomly assign a single state to not have economic nexus and then randomly assign states with nexus to have Marketplace Facilitator laws. The distribution is from approximately 500 random assignments. Because the small jurisdiction distribution is shifted to the right, this indicates that during the pandemic, the growth of sales tax revenue was higher in small jurisdictions.