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Remittance Rules and the Distribution of Local Tax Revenue: Evidence after Wayfair

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Remittance Rules and the Distribution of Local Tax Revenue: Evidence after *Wayfair*

Abstract

Requiring firms, rather than individuals, to remit sales taxes improves tax compliance. In the U.S., this shift toward firm-based remittance rules for remote purchases occurred gradually after *South Dakota v. Wayfair*. Using comprehensive and high-frequency local sales tax revenue data, we show that due to the increased compliance after *Wayfair*, revenues increased in the average locality by 5.4% and subsequently increased 5.1% after states required platforms to pay taxes on behalf of marketplace vendors. Critically, these effects are mainly a result of substantial increases in small towns and counties, with much smaller effects in larger jurisdictions. Increases in tax compliance thus influence both the level of tax revenues as well as its distribution across places.

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

Keywords: sales tax, online shopping, e-commerce, remittance rules, tax revenue, compliance.

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1 Introduction

Remittance rules specify who must remit—or send—a tax payment to the government. Traditional public finance theory underestimates the importance of remittance rules, arguing that the statutory burden of who remits the tax is irrelevant for tax incidence. But, remittance rules are crucial for compliance in many circumstances (Slemrod, 2019). This is especially the case in the context of the retail sales tax. Until 2018, court rulings created a dichotomy where firms only remitted the retail sales tax if they had a physical presence in the state, otherwise, consumers were responsible for paying the tax. Needless to say, there was substantial tax avoidance on many online purchases (Goolsbee 2000; Ballard and Lee 2007; Ellison and Ellison 2009; Einav et al. 2014).

We study the differential effectiveness—across local governments—of firms remitting taxes instead of individuals. Prior to 2018, due to difficulty monitoring consumer purchases from firms without physical presence, consumer remittance of taxes on these out-of-state remote sales was almost nonexistent (Agrawal and Fox 2016; Agrawal and Mardan 2019). Following the Supreme Court ruling in *South Dakota v. Wayfair*, states gradually required online vendors to remit if they have an economic nexus in the state. Small vendors were exempt, but over time, states gradually adopted Marketplace Facilitator legislation that required platforms to remit on behalf of small marketplace vendors. These reforms shifted the remittance burden from the consumer to the firm.

Changes to remittance rules substantially increase tax revenues of the states (Fox et al. 2022; Mikesell and Ross 2019) or economic outcomes (Beem and Bruce, 2021). Fox et al. (2022) find that economic nexus and facilitator laws increased sales tax revenues by 7.9%. Focusing on the single state of Tennessee, Agrawal and Fox (2021) and Bruce et al. (2023) show that economic nexus also (generally) increased revenues at the county level. Other papers have analyzed more narrow remittance rule changes via voluntary collection agreements, finding that these reforms shifted some consumer purchases to tax-free sites (Baugh et al., 2018) but also increased revenues (Kaçamak et al., 2023). Bibler et al. (2021) and Bibler et al. (2024) identify the compliance effects in Airbnb markets.

Despite the evidence on overall revenue increases, the heterogeneity of these effects across places remains unknown. Depending on whether urban or rural residents were more likely to buy online, the changes in remittance rules may have unwound major inequities created by the historically tax-free nature of e-commerce. For example, changes in remittance rules may increase revenues more in larger and higher-income places where internet penetration is higher. Alternatively, revenues may rise more in smaller and lower-income places if individuals living further away from retail establishments are more likely to buy online. Depending on where e-commerce was initially more pronounced, tax-free online

shopping may have advantaged certain types of jurisdictions over others. More generally, the magnitude of the effects of firm-based remittance rules may depend on factors such as jurisdiction size, distance to retail centers, the extent of internet penetration, distance to state borders and the tax differentials there, and the sourcing rules.

We study whether remittance rules differentially benefit some jurisdictions more than others. A priori, the magnitude of the effects across place could conceivably follow many different patterns.¹ These heterogeneous effects by jurisdiction size are critical, as the “density divide” has created a perception of declining rural tax bases.

To identify these heterogeneous effects, we use comprehensive *high-frequency* county-level and municipal-level data on tax revenue and taxable sales for nearly all states. These data are assembled in Agrawal and Shybalkina (2023). Agrawal and Shybalkina (2023) study the effect of an increase in *the amount of e-commerce* holding *constant* the remittance rules. We change the remittance rules, holding constant the shopping technology.

We then exploit the adoption of remittance rules and Marketplace Facilitator laws using a staggered difference-in-differences approach. Some states took longer than others to implement the remittance rule changes after *Wayfair*. We resolve recent critiques of staggered difference-in-differences designs using Gardner (2022).

Focusing on counties, we find that economic nexus increased sales tax revenues by 5.4% and marketplace laws increased revenues by 5.1%. Small counties generally saw two to three times larger effects than big counties. First, due to relatively few large retail firms, smaller jurisdictions generally cannot tax export to nonresidents, implying that any change in enforcement will result in larger percent increases. Second, residents of smaller towns may be more likely to buy online from vendors without a physical presence in the state, perhaps because driving to specialized stores would entail substantial cost. Studying other sources of heterogeneity such as by internet penetration, income, and proximity to state borders, we conclude that size appears to be the dominant factor.

Our evidence suggests that residents of smaller jurisdictions were more likely to buy online in the pre-*Wayfair* era, thus advantaging their residents but harming their budgets. This is consistent with small jurisdictions being disproportionately affected by the lack of tax enforcement due to distance costs. Further, as small towns do not get revenues from shopping malls in other towns, if residents realized the arrival of destination taxation on online purchases meant that their home jurisdiction would receive the tax revenue, residents of there may have switched some brick-and-mortar sales to online.

Firm-based remittance rules increase tax compliance in a way that especially benefits smaller jurisdictions. Thus, we identify which places initially had more tax avoidance.

¹Using a single state case study, Bruce et al. (2023) find that *Wayfair* had a larger effect in more remote counties and a smaller effect in counties near the state border.

2 Motivation

2.1 Institutions

Figure 1 shows the rapid rise of remote sales. While the popular press focuses on business-to-consumer sales, many business-to-business sales occur online (and some are taxable).

When cross-border transactions are taxed according to the destination principle—where the buyer lives and uses the good—tax design requires the government to specify who must remit the tax. Historically, Supreme Court rulings like *Quill Corp. v. North Dakota* prohibited states from requiring remote vendors to remit state and local taxes on interstate transactions unless the vendor had a physical presence in the state. Instead, consumers were expected to file “use taxes”. Due to a lack of enforcement, online transactions from vendors without nexus were effectively tax-free, costing states and local governments substantial tax revenues.²

In 2018, *South Dakota v. Wayfair* overturned this precedent and allowed states to require remote vendors to remit state and local taxes as long as they had an economic presence in the state. Economic nexus arises, according to state law, if a firm reaches a certain number of transactions or value of sales into the destination state. This shift from consumer-based remittance rules to firm-based remittance increased compliance.

Transactions from small vendors below the sales thresholds to satisfy economic nexus escaped taxation. To address this, states gradually passed “Marketplace Facilitator” laws, which designated platforms (e.g., Amazon) as the remitting party on behalf of their small sellers. This too increased compliance with destination-based taxation.

States adopted these laws gradually (mostly over a two-year period), with some states adopting earlier than others, in ad hoc ways that depended on when the legislature was in session and the extent of legal planning occurring before *Wayfair*.

2.2 Theory

Theory can provide some predictions—while this setup makes numerous assumptions, its sole purpose is to show that the effects of remittance rules can be heterogeneous across places. Consider a large and a small jurisdiction, with capital [lowercase] letters denoting the large [small] jurisdiction. We use the standard Kanbur and Keen (1993) approach of comparing relative price incentives with perfectly inelastic demand. Let $p = P = 1$ denote the pre-tax price, (T, t) denote the tax rate, $C = c$ be the per-mile cost of driving (D, d) miles to the store with $D < d$, and (F, f) be the fixed cost of brick-and-mortar shopping. Finally, a consumer can buy goods online and incur a net fixed cost (E, e) covering the

²See Fox and Murray (1997), Bruce and Fox (2000), and Bruce et al. (2009).

internet subscription/shipping costs. Following Agrawal and Wildasin (2019), we focus on goods that can either be purchased in shopping malls located in the large jurisdiction or from online vendors, but not in the small jurisdiction.

Because of the asymmetry in retail agglomerations, the large jurisdiction tax base will be bigger than the small jurisdiction—due to origin sourcing, the large jurisdiction gets tax revenues from both residents and non-residents who drive there. Because its tax base is a priori larger due to tax exporting, any change in remittance rules will have a smaller (percent) effect on its revenues.

Prior to *Wayfair*, individuals buy online from out-of-state vendors instead of driving to a shopping mall if

$$\begin{aligned} E &< T + CD + F && \text{if large} \\ e &< T + cd + f && \text{if small.} \end{aligned} \tag{1}$$

Critically, while online transactions are not taxed, physical cross-border purchases are taxed under the origin principle, and thus subject to the large jurisdiction’s tax.

These relative-price rules indicate that, if all fixed costs were the same everywhere, tax-free online shopping would have been more common in the small jurisdiction because residents there need to incur larger distance costs. At the opposite extreme, one might believe that $E < e$ because the Internet penetration is better in the large jurisdiction, making online shopping more common there if travel costs are similar across places.

But after *Wayfair*, if the e-tailer remits the tax, an individual buys online if:

$$\begin{aligned} E &< CD + F && \text{if large} \\ t + e &< T + cd + f && \text{if small,} \end{aligned} \tag{2}$$

where taxes are sourced at destination. Taxes cancel in large jurisdictions because e-commerce faces the same rate as brick-and-mortar sales. In the large jurisdiction, notice that this condition is stricter than the pre-*Wayfair* condition, implying there will be some substitution back toward brick-and-mortar sales—though these sales are taxed at the same rate as e-commerce. The condition for the smaller jurisdiction also tightens, implying a decline in online shopping, but this shift possibly further raises revenue in the large jurisdiction.

3 Data

In this paper, we use hand-collected high-frequency data on county and local sales tax revenues and bases from state revenue departments (Agrawal and Shybalina, 2023). These data—digitized and then harmonized from state revenue websites—are either at the monthly or quarterly frequency. Some states release revenue data while others release

tax base data; we pool these data on the assumption that tax rate changes in response to the policy and any miscellaneous fees in revenue are negligible.³

Given states adopt remittance rule changes in different months over a couple of years, high-frequency data is necessary for identification. We match the tax data to Census files using name matching and harmonize all data across states. Complete details on the extent and limitations of the data are reproduced in Appendix A.1.

A limitation of the data is the different ways states release data, with some states releasing data at the monthly versus quarterly frequency. While aggregating at the quarterly frequency is possible, quarterly samples have the disadvantage of having fewer event indicators. Thus, we present results using both monthly and quarterly data.

To these data, we merge remittance rule changes from Fox et al. (2022), after adjusting some dates to account for differences in state versus local implementation (Table A.2). For example, in Illinois, economic nexus first applied only for state sales taxes; local sales taxes were not covered by these reforms until several months later.

4 Methods

The highly disaggregated data allows us to improve the clarity of identification and to conduct heterogeneity analysis relative to state-level data in Fox et al. (2022). We use temporal and cross-state variation in the adoption of economic nexus and marketplace rules to identify the revenue effects of these reforms. Our dependent variable is the year-over-year growth rate in tax revenues, $g_{i(s),t} \equiv \ln(R_{i(s),t}) - \ln(R_{i(s),t-4})$ where R is revenue for locality i in state s of quarter t (alternatively, t is adjusted for monthly data).⁴ Local tax revenues are noisy and highly seasonal. The use of the year-over-year growth rate, as in Agrawal and Shybalkina (2023), addresses this seasonality in the data.

Using a staggered difference-in-differences design, we circumvent the recent concerns with these designs (Goodman-Bacon, 2021), by using the two-step estimator of Gardner (2022).⁵ Under this approach, the outcome of interest $g_{i(s),t}$ is first regressed on the two-way fixed effects using only observations that have not yet been treated:

$$g_{i(s),t} = \zeta_i + \zeta_t + \varepsilon_{i(s),t}, \quad (3)$$

³Results are robust to using only states that release revenue data.

⁴For samples studying the effect of Marketplace Facilitator laws, we use a two-year growth rate. This is because when using the year-over-year rate, $\ln(R_{i(s),t-4})$ can be affected by prior adoption of nexus. Two-year growth rates avoid this issue by ensuring the change is relative to a clean pre-period.

⁵We have explored robustness to using the estimators of de Chaisemartin and d'Haultfoeuille (2020), Borusyak et al. (2024), and Callaway and Sant'Anna (2021). Results are qualitatively and quantitatively similar in all samples except samples with very few states.

where we let ζ_i and ζ_t denote locality and time fixed effects. Then, (3) is used to residualize the outcome variable for all observations, yielding $\hat{\varepsilon}_{i(s),t} = g_{i(s),t} - \hat{\zeta}_i - \hat{\zeta}_t$. The residualized variable is then used to estimate an event study specification

$$\hat{\varepsilon}_{i(s),t} = \sum_{y=-Y_0}^{-2} \theta_y \mathbf{1}\{t - t_s^* = y\} + \sum_{y=0}^{Y_1} \beta_y \mathbf{1}\{t - t_s^* = y\} + \epsilon_{i(s),t}, \quad (4)$$

where $\mathbf{1}\{t - t_s^* = y\}$ are event time indicators relative to the state policy implementation, t_s^* (i.e., y is event time). Thus, θ_y is the differential evolution prior to the reform, while β_y represents the differential evolution after the reform. We have limited high-frequency covariates, and although suppressed above, we include unemployment rates and covariates for state data accounting changes over time. Because the procedure is a two-step procedure, standard errors are corrected for this. We cluster at the primary statistical area level, given most shopping patterns are likely linked to these regions.⁶

Many states adopt economic nexus and then adopt Marketplace laws some months later. Failing to account for this could contaminate our causal effects of each individual policy. Although the policies are similar in their goals, they affect very different firms and thus may have effects that differ in their magnitudes. We adhere to the methodology outlined in de Chaisemartin and D’Haultfoeuille (2023) for establishing our event window, $[-Y_0, Y_1]$, in a manner that ensures non-overlapping policies in both pre- and post-periods. The event windows are such that we only report balanced event dummies. In examining the nexus effect—as states always adopt this policy first—we use data back to 2016, but plot event indicators only starting 4 quarters [9 months] before adoption. Because some states then adopt Marketplace laws shortly thereafter, our analysis focuses on states that do not adopt marketplace policies until at least 3 quarters [9 months] after economic nexus. Although this includes states with more periods of post-reform data, we only report the balanced event dummies. In addition, we include states that do not adopt economic nexus until the end of our data as “never-treated” nexus units. States that adopt Marketplace laws faster than 3 quarters [9 months] are dropped from the analysis, though we have considered shorter event windows.

For the Marketplace effect, we use states that adopt Marketplace regulations at least 4 quarters [5 months] after economic nexus adoption, with the post-period limited by the balanced event dummies resulting from the end of our dataset. In addition, states that adopt nexus but do not implement Marketplace policies by the end of our data are never-treated units. Further, some states concurrently adopt both nexus and Marketplace

⁶We show in Table A.3 standard errors increase only slightly when clustering at the state level, though because of our sample restrictions discussed below, and because monthly data are only available in certain states, each estimating sample does not have a large number of clusters.

regulations, and we analyze these states separately using states that do not adopt nexus or marketplace laws until after the beginning of 2021 as never-treated units. Figure A.1 shows the states used in each sample after appropriately balancing.

To test the conditionally random assumption, we follow Bailey and Goodman-Bacon (2015) and Bailey et al. (2019) by regressing the date of adoption on economic/political characteristics of the state. Table A.4 shows no systematic correlation with the time of adoption. Additionally, both the level and trend of tax revenues prior to the reform are not significantly correlated with date of adoption. This is consistent with the timing depending on when the state legislature is in session or the state’s planning pre-*Wayfair*.

One might worry that COVID-19 might confound some of the dynamics of our effects. Several states in samples used to study the effects of Marketplace legislation and concurrent adoption of nexus/Marketplace laws were affected by COVID-19 in the later reported treated periods. However, states in samples used to study the effects of nexus are largely unaffected by COVID-19 in the reported treated periods (except for Kansas and Tennessee in county data). Nonetheless, shortening the event windows to exclude the COVID-19 period yields similar results.

5 Results

5.1 Baseline Results

Figure 2 shows the baseline results for all jurisdictions. Panels (a) and (b) focus on states that adopt nexus, while Panels (c) and (d) focus on Marketplace adoption. In all cases, the pre-reform trends are centered around zero, providing support for the plausibility of the parallel trends assumption. However, within 1-2 months after the reforms, revenues increase, usually with some dynamics. The first couple of months have smaller increases in growth rates than later months, perhaps due to uncertainty about the date of implementation or sales tax reporting not aligning perfectly with the date of adoption.⁷ The event study figures indicate these policies manifest in similar effects when using the sample of states using quarterly and monthly data.

It is interesting to note that standard errors increase in the post-reform period. Because all reported event dummies are balanced, this is not a result of changes in sample composition or size. One possible explanation is that the treatment effects are heterogeneous across different localities, an issue we return to subsequently. Of course, another possibility results from how Gardner (2022) estimates the first stage using only

⁷Some states may have also needed to establish methods/formulas for unsourced online sales; this may have resulted in delays of local revenue distributions.

not-yet-treated observations.⁸

Furthermore, as necessitated by the empirical design, all event studies document short-term effects (less than a year) immediately following the reform. Long-term effects may differ, if for example, local tax rates adjust in response.

In the upper left of each panel, we present the average effect from a specification that replaces the event indicators with a treat-by-post indicator. Focusing on counties in the quarterly sample, our results indicate that revenue grew 5.4 percentage points faster after economic nexus, 5.1 percentage points faster after Marketplace, and a similar magnitude after concurrent adoption of both policies. These effects are larger in size than the year-over-year annual growth rates prior to the reform, suggesting that increased compliance contributes more than an extra year’s growth to revenues. At the municipal level, the percentage changes are qualitatively similar in terms of percent changes, although effects following nexus adoption appear larger in the municipal than county sample. Table A.5 indicates this may be because towns there differ from the other samples on some of their observable characteristics.

Similarly, in an ideal world, the sum of adopting nexus and then adopting Marketplace Facilitator laws would be approximately equal to the effect of concurrent adoption (see Figure A.2). However, the sample of states changes across each type of treatment. Again, Table A.5 shows how the characteristics differ across each treatment sample. Interestingly, states that adopted both policies at once were likely to have higher thresholds necessary to trigger economic nexus, indicating their reforms affected fewer firms. In addition, in the county sample, these states were more likely to have quasi-origin sourcing on some e-commerce sales.⁹

While the prior event studies cleanly identify the effects of each individual policy, they restrict variation to a subset of states. Figure A.3 uses the date of first policy adoption as treatment—pooling states that concurrently/separately adopt both policies—and then follows the jurisdictions even if they subsequently adopt Marketplace laws. When interpreting the magnitudes of the effects in panels (a) and (b), the sample of treated states differs from each of the prior individual subsamples. Panels (c) and (d) indicate that about half the pooled sample are concurrent adopters and that Marketplace adoption occurs gradually over time. We conclude that the treatment effects persist over the longer event-time horizon.

How do these results compare to Fox et al. (2022)? They find nexus increased

⁸Reestimating the event studies using Borusyak et al. (2024)’s efficient estimator indicates little difference in the standard errors pre- and post-reform.

⁹While most states source all e-commerce sales at destination, some states tax intra-state sales at origin. Following economic nexus, some firms changed business models to take advantage of this. See “California cities want a slice of Amazon sales tax,” noting Amazon changed its ownership structure so that cities with distribution centers receive the tax revenue.

revenues by 1.78%, though statistically insignificant, while Marketplace laws increased revenues by 6.17%. Keeping in mind these effects are for the average state, while ours are for the average municipality/county, it appears we identify larger nexus effects but similar Marketplace effects. Part of this could be explained by the econometric design which estimates treatment effects separately, though some of it may be due to asymmetric effects we will demonstrate by size: our sample consists of many small localities that saw larger increases. We return to this point subsequently.

To gauge whether these effects are consistent with plausible magnitudes, note that e-commerce occurs in both business-to-consumer and business-to-business sales. In 2018, 10% of consumer sales were remote purchases. Given we know many vendors—including Amazon representing a large share of online sales—were already remitting taxes, our effects might seem too large. However, several points are in order. First, the 10% figure is a share of total retail sales and not taxable retail sales. While at least 70% of online purchases are likely taxable, many components of total sales are not taxable. As a result, e-commerce purchases as a share of the taxable base are likely much higher.

Second, many business-to-business (B2B) transactions are subject to sales taxes. Consistent with Bruce et al. (2009), B2B sales are approximately 93% of total e-commerce. Further, they estimate 13.0% of B2B e-commerce transactions are taxable, though estimates by Ring (1989) may even suggest a higher number. Of course tax compliance for B2B is much better than for B2C, but tax avoidance is still non-trivial. According to a 2016 Washington state audit study (Smith, 2016), given B2B is the bulk of online sales, there is a 21.5% non-compliance rate with the taxes due on B2B sales that were not collected by vendors. As a result, significant non-compliance of taxable B2B transactions will also likely be influenced by shifting the remitting party from the buyer to the seller. These two observations make our estimates consistent with recent (noncausal) GAO reports (GAO, 2022)—documenting \$23 billion in increased revenues from *Wayfair* relative to \$340 billion in state sales taxes (e.g., 6.8%).

Finally, to strengthen the case for identification, Figure 3 presents a placebo test. When passing economic nexus laws, some states first required firms to only remit state taxes, with local taxes not being subject to the reform until several quarters later. When using the date applicable for remitting only state taxes—but not local taxes—we find no effect on local tax revenues.

5.2 Heterogeneity by Size

Next we study heterogeneous effects by size. Size is long acknowledged in the commodity tax competition literature as one of the most important determinants of the revenue effects of policy interventions (Kanbur and Keen, 1993). And our theoretical framework

indicates size is important. Depending on the relative costs of buying goods online versus brick-and-mortar, pre-reform tax-free sales may differ between large and small jurisdictions. Given how size is also a critical determinant of variation in tax rates and revenues (Kanbur and Keen, 1993), it thus represents our primary focus.

To flexibly model size, we focus on the average post-reform effects rather than event studies. To do this, we replace the event dummies in (4) with a simple indicator for treat-by-post. We then include additional interactions of this term with indicators for deciles of jurisdiction size. Our preferred metric for size is the number of establishments in the retail or wholesale sector, using the Census County Business Patterns, as this proxies for retail agglomerations. We include wholesale operations to capture taxable B2B transactions discussed previously. Unfortunately, because establishment counts are not available at the municipal level, we use municipal population as a proxy for the extent of the retail operations in the municipality.

Large retail agglomeration and population jurisdictions raise most local sales tax revenue. In order to interpret the number of establishments as a measure of retail agglomeration or urbanization, some assumptions are necessary. U.S. counties vary dramatically in size by land area. In very large counties with many remote areas, the number of retail establishments may be large just by virtue of jurisdiction area. As county size differs by state, using national thresholds to split the sample may pick up heterogeneity across states. We have explored using the number of counties per square mile, defining large/small based on state-specific rather than national thresholds, and directly using the USDA urban-rural continuum codes. Cross-tabulations indicate that regardless, most counties remain classified in the same manner.¹⁰ The number of establishments as a measure of retail agglomeration is policy relevant for thinking about the size of sales tax base and interjurisdictional revenue capacity—and thus our preferred metric.

Figures 4 and A.5 show the effects by deciles of size. In almost every specification, larger counties see smaller increases in the growth of revenue than smaller jurisdictions. Following nexus and Marketplace adoption, for many deciles of the size distribution, the effect sizes are almost monotonically decreasing in size. However, the effects are always positive—as should be the case—because enforcement increases everywhere in the state.

The size effects at the municipality level are qualitatively similar, except that the population gradients in Figure 4 are flatter over the bottom four deciles. Part of the reason is that the bottom deciles in the population distribution contain very small and remote municipalities with populations less than 678 people. These municipalities may not have good broadband access and so residents there may be less reliant on e-commerce.

¹⁰Table A.6 shows the cross-tabulation of establishments and urban-rural continuum codes, while Figure A.4 shows the correlation with population.

But there is a clear decline in the magnitude of the effects over the top six deciles. The one main exception to this nearly monotonic gradient in size is the municipal-monthly sample, perhaps a result of no never-treated states releasing monthly data.

In terms of magnitudes, focusing on the quarterly sample of counties, counties in the bottom decile see growth increase 6.6 [7.3] percentage points, while counties in the top decile see growth increase 2 [1.0] percentage points after the adoption of economic nexus [Marketplace Facilitator laws]. The bottom decile contains counties with less than 31 retail establishments, while the top decile are those with more than 1,299 establishments. Pooling the bottom three and top three deciles indicates growth increases 7.3 [6.3] percentage points in the smallest jurisdictions, while increasing 2.4 [3.2] percentage points in the largest jurisdictions following economic nexus [Marketplace Facilitator laws]. The p-values on a test of differences between large and small jurisdictions in the quarterly sample is 0.006 [0.195]; in the monthly samples, the p-values are 0.022 [≈ 0] for nexus [Marketplace].¹¹ In percent terms, small and large jurisdictions are differentially impacted by enforcement policies. This helps close the revenue gap across places.

To gain an appreciation of the economic magnitude of these effects, we translate one year’s worth of growth from these causal effects into the number of additional teachers that could be hired at the median state’s 2017 teacher salary. In the bottom three deciles, each *one* percentage point of additional growth would result in 0.13 new teachers, while at the larger three deciles each *one* percentage point of growth results in 10.9 teachers. These effects can then be multiplied by the causal effects presented in the prior paragraph in order to determine the total annual effect of the change in remittance rules. Although the levels in larger jurisdictions are bigger, so too are their demand for teachers.

This result suggests that smaller jurisdictions disproportionately benefited—in percent terms—from the change in remittance rules. This is consistent with *Wayfair* not benefiting localities as much if they were able to export lots of their tax burden to nonresident cross-border shoppers. Further, the result appears to be consistent with the tax increase from compliance not inducing more rural residents to shift their purchases back toward brick-and-mortar sales in larger jurisdictions. Although prior tax elasticities of e-commerce are large (Einav et al. 2014; Goolsbee 2000), indicating that people were price sensitive to sales taxes, by 2018 it appears e-commerce became well established enough that this offsetting channel was not qualitatively important.

The relative costs of online shopping and brick-and-mortar purchases matter. Our results shed light on which jurisdictions benefited from lax sales and use tax enforcement in the pre-*Wayfair* era. It appears that small jurisdictions’ residents enjoyed substantial tax-free benefits, perhaps because they were more likely to shop online due to high costs of

¹¹Tables A.7 and A.8 present the p-values on tests of differences for all samples.

driving to shopping malls. As a result, small jurisdictions appear to have disproportionately enjoyed tax-free online shopping relative to their larger neighbors. Governments in these same rural jurisdictions were thus losing considerable revenue to tax-free online sales and losing revenue to nearby urban centers due to the origin taxation of brick-and-mortar sales. Their residents also may have been more willing to switch cross-border sales to e-commerce if they understood the arrival of destination taxation on e-commerce meant their home jurisdiction would receive the tax revenue from the sale—better matching taxes with their public service benefits. There are many diverse equity effects of *Wayfair*.

Remittance rules that facilitate enforcement thus provide a boom in revenues in smaller and more rural jurisdictions. It is important to compare this result to Agrawal and Shybalkina (2023), who find using simple before-and-after comparisons that COVID-19 increased rural tax revenues at the expense of retail centers. That result operated through an entirely different mechanism, identifying the effect of an exogenous increase in *the amount of e-commerce*, perhaps resulting from shocks to the relative cost of online versus brick-and-mortar sales. In that setting, one jurisdiction’s gain was another jurisdiction’s loss. This is because that paper holds *constant* the remittance rules and instead studies a technological shock. As a result, Agrawal and Shybalkina (2023) sheds light on how technological change can erode or enhance the tax base.

In this paper, we consider the opposite scenario: we hold constant the extent of online shopping (except for any endogenous change in it due to the tax rules) and instead change the *policies* in place. Thus, we study the effect of an increase in tax enforcement via remittance rules rather than an expansion of e-commerce due to technological change. In this setting, the enforcement shock benefits all jurisdictions, but benefits some more than others depending on the initial conditions. As a result, this paper is about how tax *policies* that facilitate enforcement can be more beneficial to some jurisdictions than others. This allows us to determine which jurisdictions were most harmed, in percentage terms, by tax evasion.

5.3 Heterogeneity by Other Characteristics

Other factors such as proximity to state borders, internet penetration, income, urbanization, the state sourcing rules, and whether the state sets a high/low sales threshold to trigger economic nexus may influence the magnitude of the effects (see Appendix A.2). While the thresholds to establish nexus might be a measure of intensity of treatment, there are many challenges to identifying the causal effects of these thresholds: states often have separate sales and transaction thresholds, thresholds do not vary within states, and state thresholds might be correlated with unobservables such as in-state purchasing preferences. That said, adopting economic nexus or combined nexus and marketplace rules

shows lower effects under high thresholds in some municipal subsamples, but, overall, the relationship between thresholds and effect magnitudes is weak.

As for other factors, aside from a few exceptions, we do not notice them providing significant differential effects once already conditioning based on jurisdiction size. In some subsamples, border counties and municipalities on the low-tax side of neighboring states observe higher effects, possibly suggesting newly emerged cross-border shopping due to rule changes. We conclude that size appears to be the dominant factor. This result is consistent with the tax competition literature, which shows that with decentralized taxation, the primary source of heterogeneous responses is jurisdiction size.

6 Conclusion

Kopczuk et al. (2016), Bibler et al. (2021), Hansen et al. (2021) and Fox et al. (2022) show that the remittance system matters for the extent of evasion, tax incidence and tax revenues. However, how tax enforcement increases revenues depends critically on the amount of initial evasion, the existence of other avoidance opportunities, the breadth of the tax base, and the strength of remittance rules. These factors can differ across place, meaning that the distributional effects of remittance regimes may be heterogeneous.

Although noncompliance with sales and use taxes was historically high, other taxes also have high noncompliance. Thus, other state-wide reforms to these taxes may have heterogeneous effects across localities. For example, excise taxes on cigarettes are evaded by bootlegging—reselling packs purchased in lower-tax states after smuggling them into high-tax states (Wang et al., 2019). And states have recently discussed noncompliance with local personal property taxes on vehicles via improper registrations.¹² These evasion strategies affect state and local tax revenues. Policy responses to them may have disparate impacts on different local communities. Thus, changes in enforcement tools that increase revenue efficiency may also create a more equal distribution of revenues across communities.

Tax evasion and avoidance are not randomly distributed across space. Lax enforcement of sales and use taxes on online transactions appears to have disproportionately harmed tax revenues in smaller jurisdictions lacking many brick-and-mortar retailers. These revenue effects of effectively tax-free e-commerce confounded pre-existing losses in revenue to nearby towns with retail centers as a result of origin taxation on in-person sales. As a result, policies that enforce destination-based sales taxes on e-commerce can potentially have heterogeneous effects across space, helping to better link the taxes paid

¹²See “Evasion of Property Taxes on Motor Vehicles”, State of Connecticut Office of Legislative Research Report, 2014.

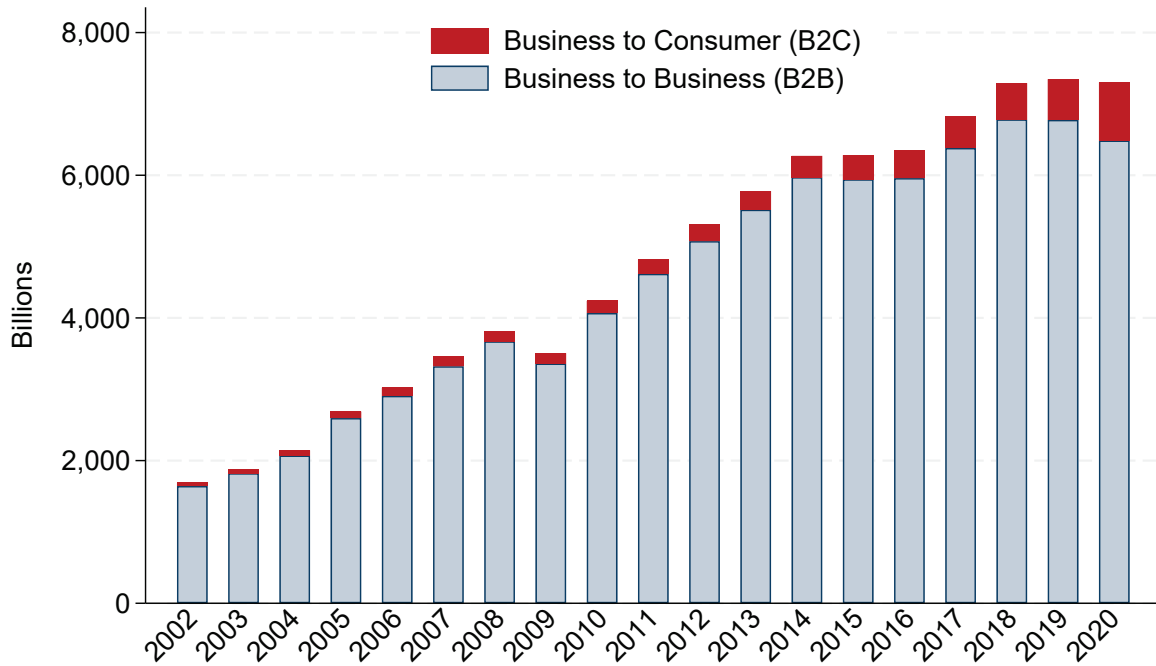
to the services provided to local residents. Even if changes in remittance rules and enforcement policies are uniformly set by states or the federal government, the revenue effects on local governments depend on how tax avoidance/evasion vary across places.

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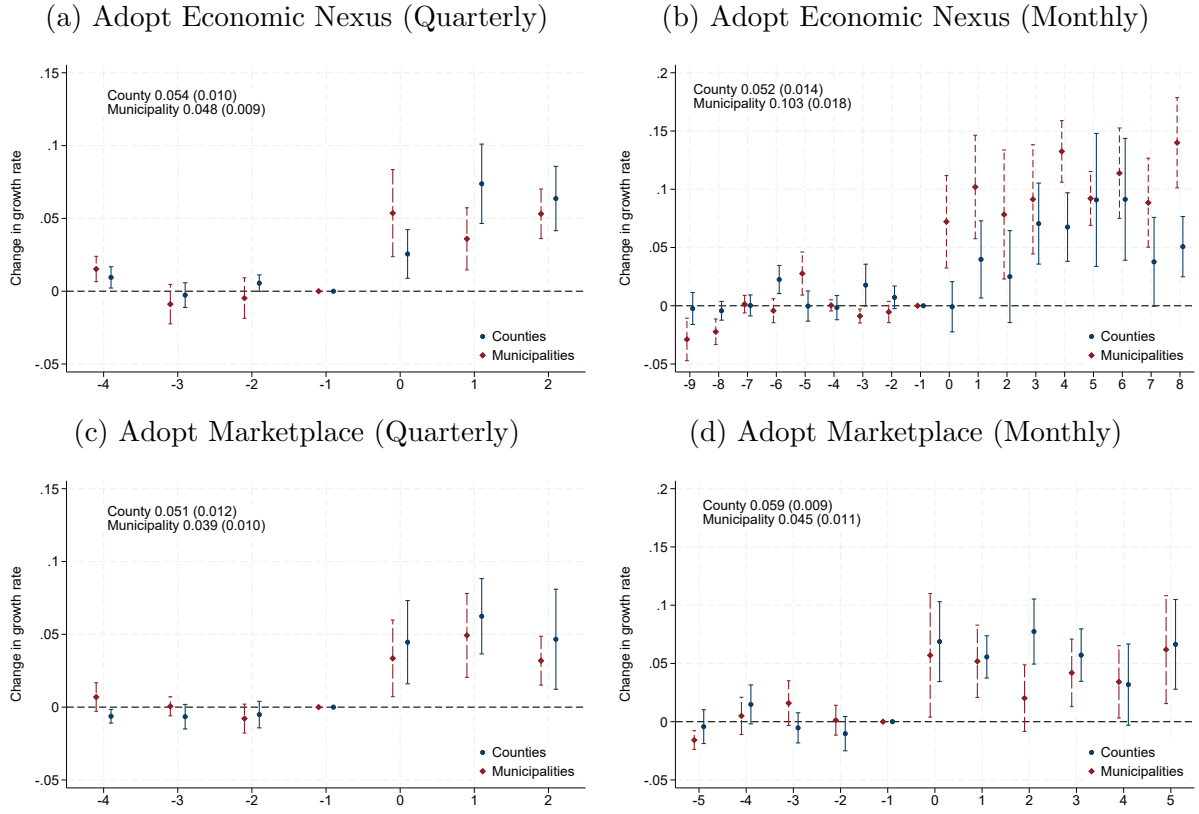
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Figure 1: E-Commerce Trends



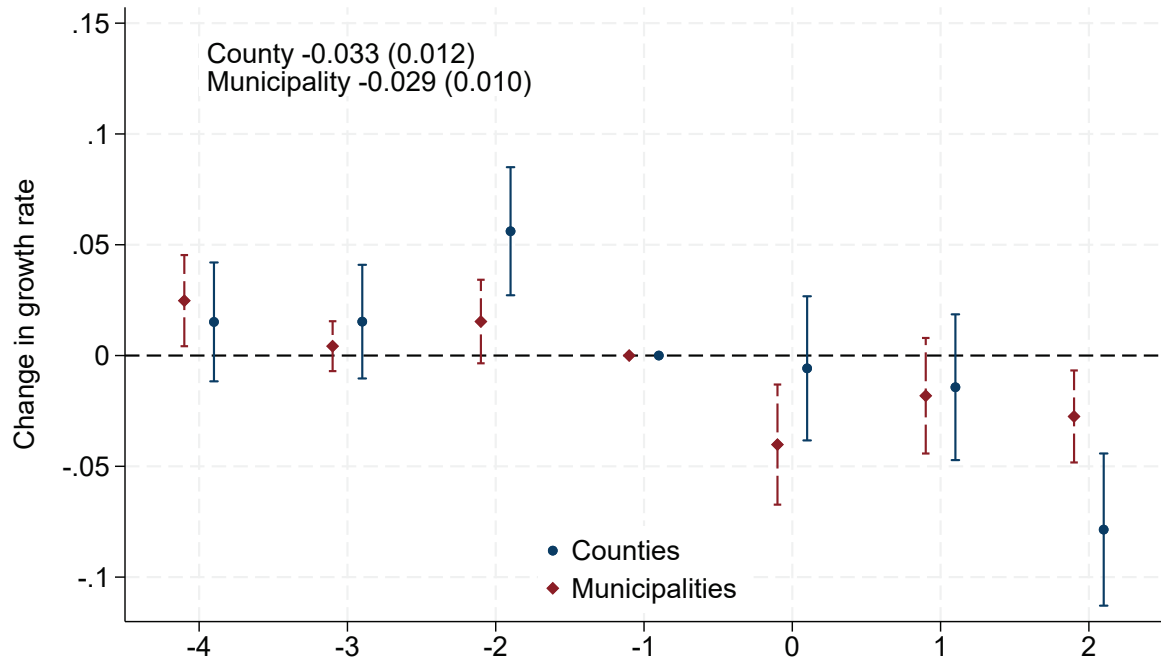
This figure shows e-commerce sales over time, broken down by whether they are B2C or B2B. Business to Consumer (B2C) encompasses retail trade direct to consumer, while Business to Business (B2B) covers manufacturing and merchant wholesalers. In 2018, B2B was 93.0% of total e-commerce (\$507, \$3,970, and \$2,813 billion in 2018, respectively for B2C retail, B2B manufacturing, and B2B wholesalers). In 2018, e-commerce sales were 9.7% of total B2C sales and 46.7% of B2B sales (67.4% for manufacturing and 32.6% for wholesalers). The source of data is US Census E-Commerce Statistics.

Figure 2: Event Study: Change in Remittance Rules



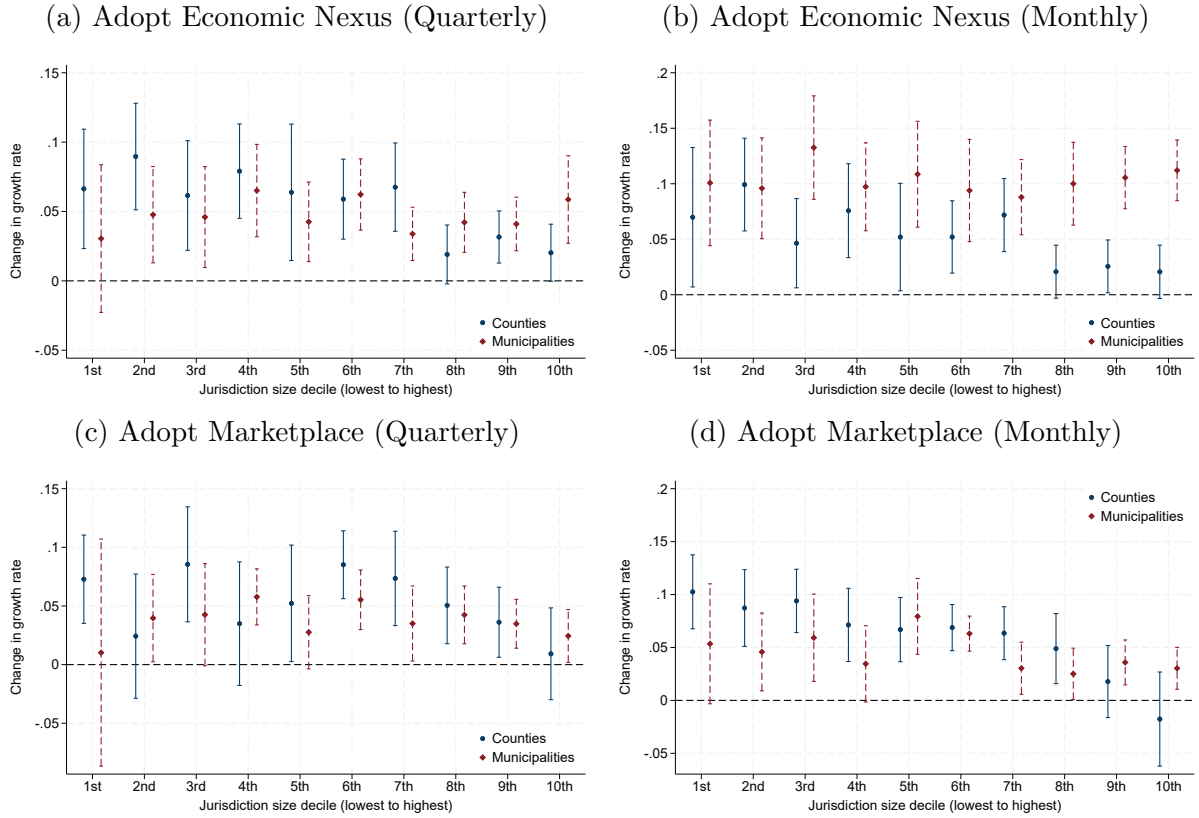
This figure shows an event study where the dependent variable is the year-over-year growth rate in county or municipal sales tax revenue or taxable sales. Panels (a) and (c) use states that release quarterly data but also include states with monthly data aggregated to the quarterly level; panels (b) and (d) only use states that release monthly data. Each figure plots the effect separately for counties and municipalities. Panels (a) and (b) show the change in the growth rate for tax revenue relative to the omitted month after a state adopts economic nexus, while panels (c) and (d) plot the change in growth after a change in Marketplace Facilitator laws. Text boxes present the results with a single $treat_i \cdot post_t$ indicator. 95 percent confidence intervals are based on standard errors that are clustered at the primary statistical area, which includes all combined statistical areas and core-based statistical areas not part of a combined statistical area. Areas outside primary statistical areas are grouped within each state.

Figure 3: Placebo Test: State Nexus Adoptions Not Applying to Local Revenues



This figure presents a placebo test by using states where the date of adoption for economic nexus differed for state sales tax collections and local sales tax collections. Using the date when firms were required to remit state taxes, but not local taxes, we study the effect on local tax revenues using our event study design. The comparison group is never-treated states or very late adopters. 95 percent confidence intervals are based on standard errors that are clustered at the primary statistical area level, which includes all combined statistical areas and core-based statistical areas not part of a combined statistical area. Areas outside primary statistical areas are grouped within each state.

Figure 4: Heterogeneity by Jurisdiction Size Due to a Change in Remittance Rules



This figure shows the policy effects by deciles of jurisdiction size. All effects are based on the overall post-reform effects given by a single $treat_i \cdot post_t$ indicator. Panels (a) and (c) use states that release quarterly data but also include states with monthly data aggregated to the quarterly level; and panels (b) and (d) use states that release monthly data. At the county level, deciles of jurisdiction size are determined by the number of retail establishments. At the municipal level, size is determined by the population of the municipality. 95 percent confidence intervals are based on standard errors that are clustered at the primary statistical area level, which includes all combined statistical areas and core-based statistical areas not part of a combined statistical area. Areas outside primary statistical areas are grouped within each state.

Online Appendix for “Remittance Rules and the Distribution of Local Tax Revenue: Evidence After *Wayfair*”

David R. Agrawal and Iuliia Shybalkina

A Appendix

A.1 Tax Revenue Data

This entire section quotes verbatim from the online appendix of Agrawal and Shybalkina (2023) after deleting information unnecessary for this paper. New text relevant for this paper is added in italics. We include this for readers who want to understand our data in detail without accessing the prior paper.

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.

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 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.

Most states provide some but not all data types (i.e., revenue data versus transaction data or monthly data versus quarterly data). *Table A.1* presents

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.

available data types by state by taxing jurisdiction (municipal versus county), type of data (revenue versus sales), and the time frequency (monthly versus quarterly). 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.² 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 have cleaned and harmonized revenue data beginning as far back as records allow. However, some states commenced reporting later than others. For instance, Oklahoma and Utah's data commence from 2017 onwards. Nebraska's taxable sales data for counties and municipalities start in 2016. Kansas county taxable sales begin in 2018, with municipal revenue starting in 2019. Municipal taxable sales in Colorado begin 2016. Quarterly data span until 2020, while monthly data extend up to February 2021, except for Connecticut (no data available beyond 2019) and Illinois (no data available beyond the 3rd quarter of 2020).

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

²Quarterly 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.

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 of growth rates 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 munic-

ipalities 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.³ 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.

In this paper, we have limited our analysis to 26 states (Figure A.1) compared to 32 in Agrawal and Shybalkina (2023). Wisconsin is excluded due to concerns about data reliability in the pre-*Wayfair* period and Wyoming due to intermittent reporting. Four states—California, Ohio, South Dakota, and West Virginia—implemented Marketplace very shortly after enacting economic nexus, which does not meet our criterion of allowing at least an 8-month (2-quarter) gap between the two policies. For instance, Ohio adopted nexus in August 2019 and Marketplace in September 2019. Similarly, California enacted nexus in April 2019 (2nd quarter) and Marketplace in October 2019 (4th quarter). Excluding these states is critical to getting “clean” pre- and post-periods for each event study. These four states are included in a robustness check in which we pool states adopting nexus individually or adopting both policies concurrently, bringing the total number of states to 30. This check, covering data from 2016 to early 2021, plots balanced event indicators from 4 quarters [9 months] before to 4 quarters [12 months] after adoption, with states not adopting economic nexus by the end of the data period

³In Texas, some counties participate in metropolitan transit authorities instead of using county sales taxes.

included as "never-treated" units.

A.2 Heterogeneity by Characteristics

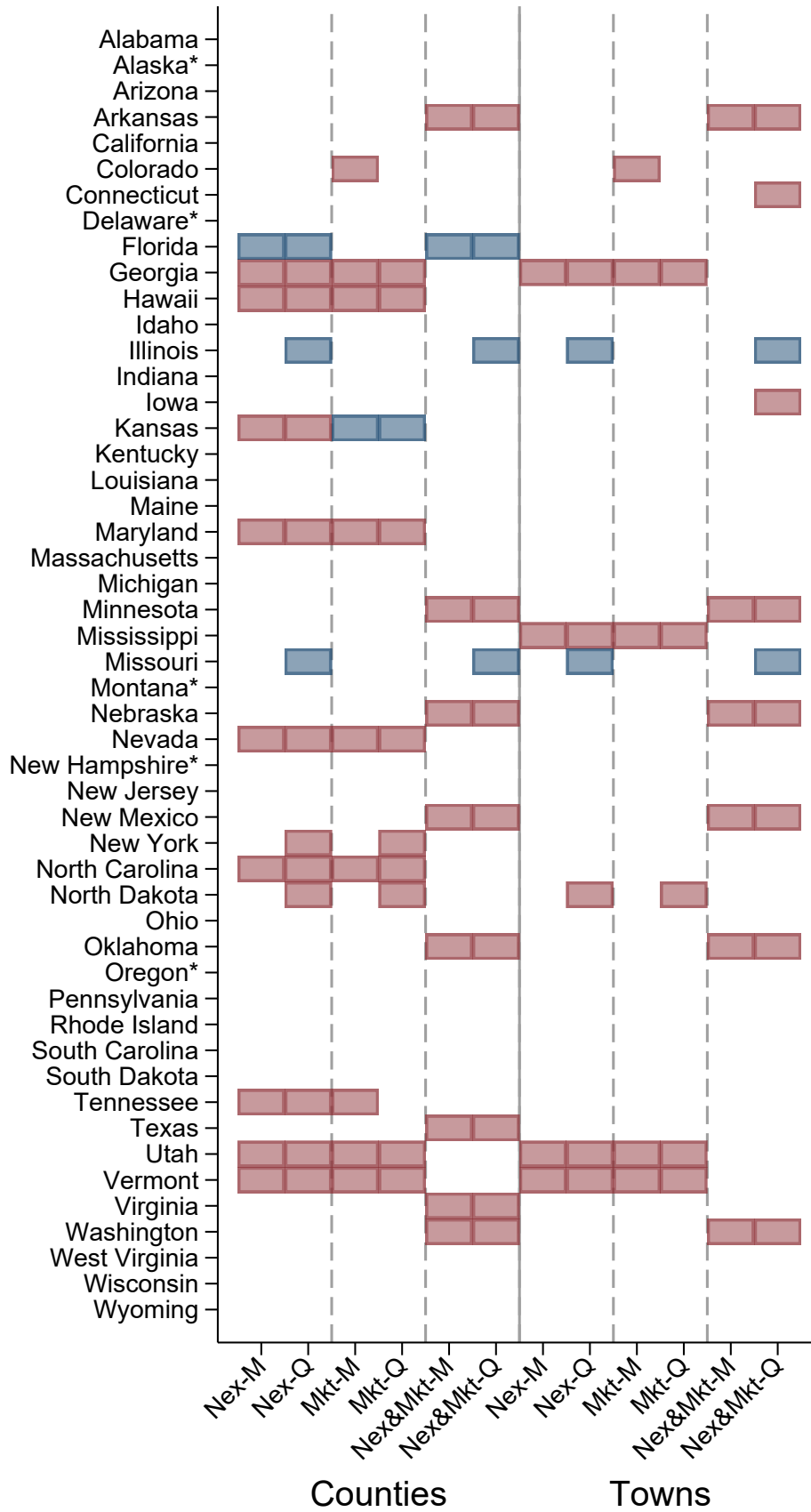
In addition to the effect of size, we study whether these effects differ by other observable characteristics of the locality or state policy. We focus on whether the jurisdiction is high income (American Community Survey), has high broadband penetration (American Community Survey), metropolitan areas status (USDA urban-rural continuum), is proximate to a border and whether that border is higher or lower tax (Agrawal, 2021), is in a state that has pure destination sourcing or sources some online sales at origin (Avalara),⁴ and by the size of the per capita dollar threshold of sales at the time of policy adoption for a firm to establish economic nexus (Agrawal and Fox, 2021). Given the initial asymmetry based on size, we focus on small jurisdictions (bottom 30 percent) and large jurisdictions (top 30 percent) because these places appear to have systematically different effects based on our non-parametric size estimates above.⁵ Splitting the sample by size, we then interact the standard treat-by-post indicator with indicators for these characteristics, including all of them in the regression simultaneously.

Overall, comparing across Tables A.9-A.14, we conclude that size appears to be the dominant factor. In many samples, large places appear to have smaller effects if state taxes are destination-based, suggesting that some online vendors may create in-state subsidiaries located in smaller locations in quasi-origin sourcing states. We sometimes notice smaller effects in states with high sales thresholds to trigger economic nexus—suggesting that the fewer firms that need to remit taxes dampen the effect. There is no clear pattern near borders, but the effects are generally larger if in low-tax states. This latter effect might be puzzling if higher taxes lead to more e-commerce tax evasion, but is justified if low-tax places benefit from added cross-border shopping resulting from consumers looking for alternative evasion strategies following the remittance rule change.

⁴In destination sourcing states, remote sales are taxed by the locality to which the product is shipped. In quasi-origin source states, e-commerce from vendors with a physical presence are taxed from the warehouse where the good is shipped, while destination sourcing only arises if the vendor has no physical location in the state.

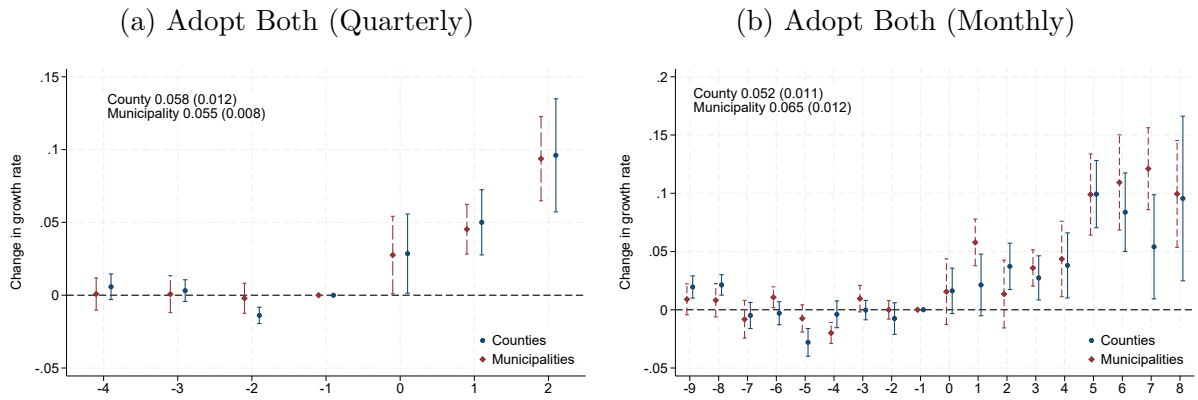
⁵We have explored using interactions in jurisdiction size as well rather than a split sample approach. We omit medium sized jurisdiction effects for simplicity.

Figure A.1: States Used in Each Sample



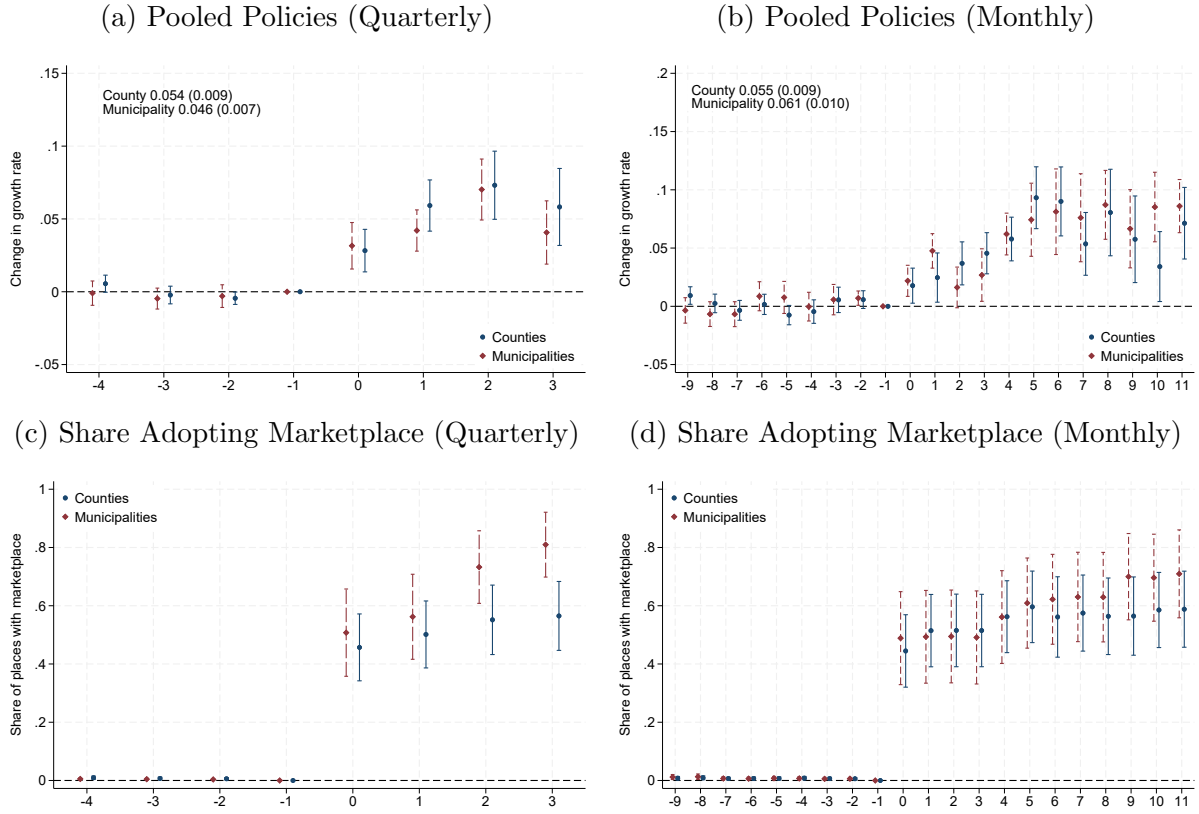
This figure shows which states are included in each estimating sample after restricting to states where treatments are not contaminated. Blue color signifies never-treated states. * indicates states without sales taxes.

Figure A.2: Event Study: Change in Remittance Rules for Concurrent Adoption



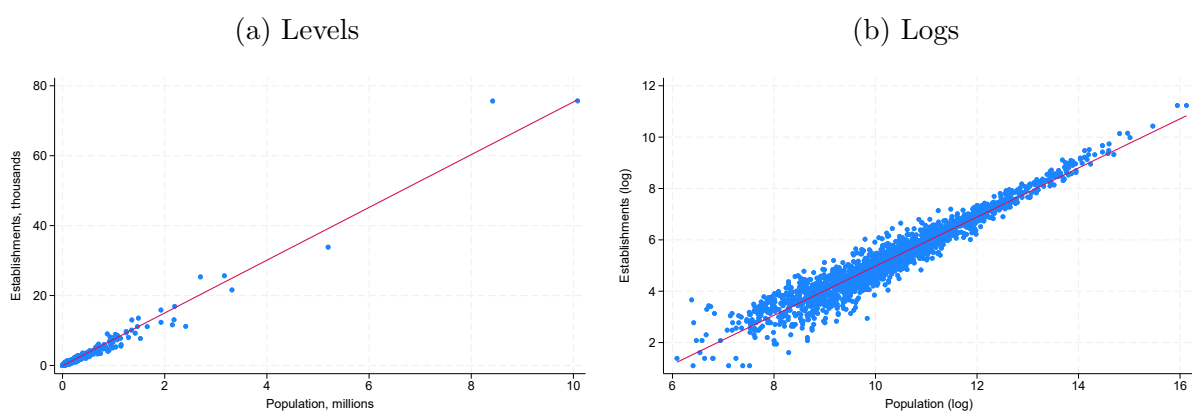
This figure shows an event study where the dependent variable is the year-over-year growth rate in county or municipal sales tax revenue or taxable sales. Unlike the figures in the text, treatment here is defined by states that concurrently adopt nexus and facilitator laws. Panel (a) uses states that release quarterly data but also includes states with monthly data aggregated to the quarterly level; and panel (b) only uses states that release monthly data. Each figure plots the effect separately for counties and municipalities. The figure shows the change in the growth rate in places that adopted both at the same time. Text boxes present the results with a single $treat_i \cdot post_t$ indicator. 95 percent confidence intervals are based on standard errors that are clustered at the primary statistical area, which includes all combined statistical areas and core-based statistical areas not part of a combined statistical area. Areas outside primary statistical areas are grouped within each state.

Figure A.3: Effects of Remittance Rules When Pooling All Treated States with Single or Concurrent Adoption



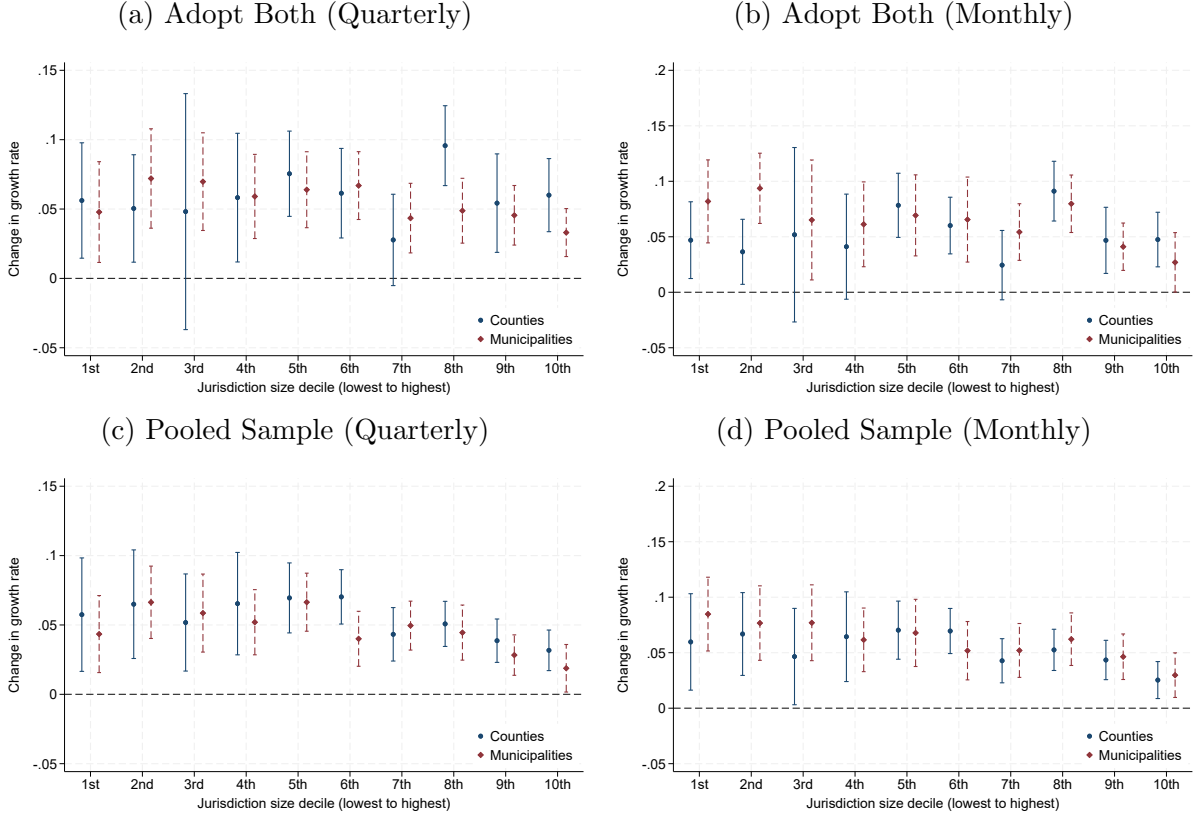
This figure shows the effect of treatment when we pool states adopting economic nexus only and concurrent adopters of nexus and Marketplace laws. Panels (a) and (b) show estimation of an event study where the dependent variable is the year-over-year growth rate in county or municipal sales tax revenue or taxable sales. Panels (c) and (d) replace the dependent variable with an indicator that equals one in the jurisdiction has adopted Marketplace laws. Panels (a) and (c) use quarterly data, while panels (b) and (d) use monthly data. Text boxes present the results with a single $treat_i \cdot post_t$ indicator. 95 percent confidence intervals are based on standard errors that are clustered at the primary statistical area, which includes all combined statistical areas and core-based statistical areas not part of a combined statistical area. Areas outside primary statistical areas are grouped within each state.

Figure A.4: Correlation of Number of Vendors and Population



This figure shows the correlation between the number of establishments and county population. Panel (a) is in levels while Panel (b) is in logs.

Figure A.5: Heterogeneity by Jurisdiction Size: Change in Remittance Rules for Concurrent Adoptions and Pooled Samples



This figure shows the overall post-reform effects with a single $treat_i \cdot post_t$ indicator by jurisdiction size. Unlike the figures in the text, treatment here is defined by states that concurrently adopt nexus and Facilitator laws. Panels (a) and (c) use states that release quarterly data but also include states with monthly data aggregated to the quarterly level; and panels (b) and (d) only use states that release monthly data. Panels (a) and (b) focus on jurisdictions that adopt nexus and marketplace laws concurrently, while panels (c) and (d) pool concurrent adopters and jurisdictions that adopt the policies separately. At the county level, deciles of jurisdiction size are determined by the number of retail establishments. At the municipal level, size is determined by the population of the municipality. 95 percent confidence intervals are based on standard errors that are clustered at the primary statistical area level, which includes all combined statistical areas and core-based statistical areas not part of a combined statistical area. Areas outside primary statistical areas are grouped within each state.

Table A.1: Data Types Released by Each State

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

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. This table is reproduced from Agrawal and Shybalkina (2023).

Table A.2: Critical Dates of States Adopting New Sourcing Rules

State	Economic Nexus	Marketplace Facilitator
Alabama	Oct-18	Jan-19
Alaska*	NA	NA
Arizona	Oct-19	Oct-19
Arkansas	Jul-19	Jul-19
California	Apr-19	Oct-19
Colorado	May-19	Oct-19
Connecticut	Dec-18	Dec-18
Delaware*	NA	NA
Florida		
Georgia	Jan-19	Apr-20
Hawaii	Jul-18	Jan-20
Idaho	Jun-19	Jun-19
Illinois	Oct-18 (state) Jan-21 (local)	Jan-20 (state) Jan-21 (local)
Indiana	Oct-18	Jul-19
Iowa	Jan-19	Jan-19
Kansas	Oct-19	
Kentucky	Oct-18	Jul-19
Louisiana	Jul-20	Jul-20
Maine	Jul-18	Oct-19
Maryland	Oct-18	Oct-19
Massachusetts	Oct-19	Oct-19
Michigan	Oct-18	Jan-20
Minnesota	Oct-18	Oct-18
Mississippi	Sep-18	Jul-20
Missouri		
Montana*	NA	NA
Nebraska	Apr-19	Apr-19
Nevada	Oct-18	Oct-19
New Hampshire*	NA	NA
New Jersey	Nov-18	Nov-18
New Mexico	Jul-19	Jul-19
New York	Jun-18	Jun-19
North Carolina	Nov-18	Feb-20
North Dakota	Oct-18	Oct-19
Ohio	Aug-19	Sep-19
Oklahoma	Nov-19	Nov-19
Oregon*	NA	NA
Pennsylvania	Jul-19	Jul-19
Rhode Island	Jul-19	Jul-19
South Carolina	Nov-18	Nov-18
South Dakota	Nov-18	Mar-19
Tennessee	Oct-19	Oct-20
Texas	Oct-19	Oct-19 0
Utah	Jan-19	Oct-19
Vermont	Jul-18	Jun-19 0
Virginia	Jul-19	Jul-19
Washington	Oct-18	Oct-18
West Virginia	Jan-19	Jul-19
Wisconsin	Oct-18	Jan-20
Wyoming	Feb-19	Jul-19

Source: Nexus and Facilitator dates are from Fox et al. (2022) and then cross-checked and updated (until February 2021), including in states where different dates were set to apply to local governments. NA denotes states without a state sales tax.

Table A.3: Clustering

	Nexus		Marketplace		Both	
	County	Municipal	County	Municipal	County	Municipal
County	0.054*** (0.005)	0.048*** (0.007)	0.051*** (0.010)	0.039*** (0.010)	0.058*** (0.008)	0.055*** (0.007)
Commuting zone	0.054*** (0.007)	0.048*** (0.009)	0.051*** (0.010)	0.039*** (0.010)	0.058*** (0.010)	0.055*** (0.008)
Labor market area	0.054*** (0.008)	0.048*** (0.009)	0.051*** (0.015)	0.039*** (0.011)	0.058*** (0.011)	0.055*** (0.009)
Core SA	0.054*** (0.010)	0.048*** (0.009)	0.051*** (0.011)	0.039*** (0.010)	0.058*** (0.012)	0.055*** (0.008)
Primary SA	0.054*** (0.010)	0.048*** (0.009)	0.051*** (0.012)	0.039*** (0.010)	0.058*** (0.012)	0.055*** (0.008)
Combined SA	0.054*** (0.011)	0.048*** (0.013)	0.051*** (0.010)	0.039*** (0.010)	0.058*** (0.014)	0.055*** (0.009)
State by year	0.054*** (0.020)	0.048*** (0.020)	0.051*** (0.016)	0.039*** (0.015)	0.058*** (0.018)	0.055*** (0.013)
State	0.054*** (0.020)	0.048*** (0.019)	0.051*** (0.014)	0.039*** (0.012)	0.058*** (0.021)	0.055*** (0.011)
N	16,271	59,182	5,006	10,723	17,127	83,016

In the main analysis, we cluster at the primary statistical area level. This table shows how standard errors change when clustering at different levels. Primary statistical area includes all combined statistical areas and core-based statistical areas not part of a combined statistical area. When clustering at core, primary, and combined statistical areas, places outside these areas are grouped within each state. This table uses states that release quarterly data but also includes states with monthly data aggregated to the quarterly level. Commuting zone and labor market area data are from the USDA, while statistical area data are from Census Bureau/NBER. When clustering at the state level, we do not have the usually recommended large number of clusters. *** 99%, ** 95%, * 90%

Table A.4: Determinants of Nexus and Marketplace Adoption Timing

	Nexus		Marketplace	
Population	0.968 (1.196)	2.869 (2.763)	1.013 (0.671)	-1.239 (3.178)
Population density	0.136 (1.034)	-0.287 (1.148)	0.124 (0.756)	0.558 (0.908)
Income	-8.221 (12.185)	-0.219 (16.991)	-2.339 (11.776)	-6.020 (13.565)
Broadband share	0.206 (0.545)	-0.014 (0.678)	-0.271 (0.465)	-0.197 (0.517)
Republican	2.239 (2.120)	2.964 (2.672)	1.178 (1.834)	0.998 (2.178)
State sales tax rate		0.523 (0.923)		-1.096 (1.034)
State sales tax revenue		-2.138 (2.736)		2.277 (3.201)
State sales tax revenue dif. (2012-2017)		7.598 (10.102)		0.593 (6.737)
N	43	43	42	42
R-sq	0.111	0.136	0.115	0.146

This specification follows Bailey and Goodman-Bacon (2015) to explore determinants of adoption timing. All variables are measured in 2017 unless specified otherwise. Population, density, income, and state sales tax revenue are logged. Population, density, income, and broadband data are from the 1-year ACS 2017. Republican is defined based on the results of the 2016 presidential election using data from MIT Election Lab. The state sales tax rate is from Tax Foundation. State sales tax revenue is from the Annual Survey of State and Local Government Finances. Robust standard errors are in parentheses. *** 99%, ** 95%, * 90%.

Table A.5: Summary Statistics

	Panel A: County					
	Monthly			Quarterly		
	Nexus	Market	Both	Nexus	Market	Both
N of establishments	529	610	395	699	866	395
Income, \$	51635	52285	53720	53433	54863	53720
Broadband	0.55	0.55	0.51	0.57	0.58	0.51
Metro	0.40	0.43	0.36	0.39	0.43	0.36
Metro adjacent	0.33	0.34	0.32	0.32	0.33	0.32
Not metro adjacent	0.27	0.24	0.32	0.29	0.24	0.32
Lower tax interior	0.25	0.36	0.25	0.29	0.41	0.25
Higher tax interior	0.25	0.14	0.43	0.24	0.09	0.43
Lower tax border	0.13	0.18	0.13	0.14	0.21	0.13
Higher tax border	0.20	0.13	0.15	0.18	0.10	0.15
Destination	0.77	0.75	0.53	0.81	0.94	0.53
High nexus threshold	0.29	0.31	0.45	0.32	0.26	0.45
Destination (state)	0.78	0.78	0.63	0.82	0.89	0.63
High nexus threshold (state)	0.56	0.56	0.50	0.55	0.56	0.50
N of counties	547	506	619	658	458	619
N of states	9	9	8	11	9	8

	Panel B: Municipal					
	Monthly			Quarterly		
	Nexus	Market	Both	Nexus	Market	Both
Population	7587	9454	9272	7664	7664	7947
Income, \$	48557	50964	49326	49136	49136	52078
Broadband	0.52	0.54	0.50	0.53	0.53	0.54
Metro	0.41	0.42	0.37	0.40	0.40	0.36
Metro adjacent	0.30	0.27	0.29	0.30	0.30	0.31
Not metro adjacent	0.29	0.31	0.34	0.30	0.30	0.33
Lower tax interior	0.22	0.30	0.35	0.22	0.22	0.29
Higher tax interior	0.28	0.23	0.27	0.29	0.29	0.30
Lower tax border	0.11	0.16	0.19	0.12	0.12	0.16
Higher tax border	0.12	0.10	0.16	0.13	0.13	0.18
Destination	0.48	0.58	0.93	0.51	0.51	0.96
High nexus threshold	0.57	0.46	0.79	0.59	0.59	0.86
Destination (state)	0.50	0.60	0.83	0.60	0.60	0.88
High nexus threshold (state)	0.75	0.60	0.67	0.80	0.80	0.75
N of municipalities	1065	1315	1489	1123	1123	2308
N of states	4	5	6	5	5	8

This table shows the summary statistics for counties (Panel A) and municipalities (Panel B) for the monthly and quarterly samples, depending on the policy treatment.

Table A.6: Number of Establishments and Metro Status

		Metro status		
		not metro adjacent	metro adjacent	metro
Number	small	251	139	81
	medium	157	295	166
	large	42	75	348

This table shows a cross-tabulation of our preferred metric of size at the county level using the number of establishments and the urban-rural continuum codes published by the USDA.

Table A.7: County Tests of Differences

	Small	Middle	Large	Small vs Large p	Middle vs Large p
Quarterly					
Nexus	0.073*** (0.016)	0.067*** (0.014)	0.024*** (0.008)	0.006***	0.003***
Market	0.063*** (0.020)	0.063*** (0.015)	0.032** (0.013)	0.195	0.107
Both	0.051** (0.021)	0.055*** (0.012)	0.072*** (0.011)	0.349	0.198
Nexus + Both	0.058*** (0.016)	0.062*** (0.010)	0.040*** (0.006)	0.293	0.027**
Monthly					
Nexus	0.072*** (0.021)	0.062*** (0.015)	0.023** (0.009)	0.022**	0.009***
Market	0.095*** (0.011)	0.068*** (0.011)	0.017 (0.013)	0.000***	0.000***
Both	0.045** (0.018)	0.050*** (0.012)	0.065*** (0.011)	0.337	0.253
Nexus + Both	0.058*** (0.017)	0.062*** (0.010)	0.041*** (0.007)	0.351	0.053*

This table shows formal tests of differences of the effects over the size distribution. Small jurisdictions are those in the bottom three deciles while larger jurisdictions are in the top three deciles. The first three columns show the marginal effects. The last two columns give the p-values for a test of differences of the effects. Standard errors in parentheses are clustered as in our main specifications. *** 99%, ** 95%, * 90%

Table A.8: Municipality Tests of Differences

	Small	Middle	Large	Small vs Large p	Middle vs Large p
Quarterly					
Nexus	0.043*** (0.013)	0.050*** (0.010)	0.047*** (0.010)	0.723	0.696
Market	0.035 (0.021)	0.044*** (0.010)	0.035*** (0.010)	0.979	0.286
Both	0.064*** (0.012)	0.059*** (0.009)	0.042*** (0.008)	0.042**	0.096*
Nexus + Both	0.058*** (0.011)	0.052*** (0.008)	0.030*** (0.008)	0.012**	0.010**
Monthly					
Nexus	0.110*** (0.023)	0.096*** (0.018)	0.105*** (0.015)	0.742	0.255
Market	0.053*** (0.019)	0.051*** (0.011)	0.030*** (0.009)	0.146	0.027**
Both	0.080*** (0.016)	0.063*** (0.013)	0.048*** (0.011)	0.040*	0.152
Nexus + Both	0.079*** (0.014)	0.058*** (0.012)	0.046*** (0.010)	0.009***	0.202

This table shows formal tests of differences of the effects over the size distribution. Small jurisdictions are those in the bottom three deciles while larger jurisdictions are in the top three deciles. The first three columns show the marginal effects. The last two columns give the p-values for a test of differences of the effects. Standard errors in parentheses are clustered as in our main specifications. *** 99%, ** 95%, * 90%

Table A.9: Heterogeneity for Places Adopting Economic Nexus (Quarterly)

	Small counties	Small towns	Large counties	Large towns
Post	0.121*** (0.041)	0.024 (0.050)	0.177*** (0.035)	0.088*** (0.020)
High incomeXPost	-0.015 (0.023)	-0.003 (0.029)	-0.011 (0.012)	0.005 (0.014)
High internetXPost	-0.037** (0.018)	0.013 (0.026)	-0.035** (0.015)	-0.004 (0.011)
Metro adjacentXPost	-0.006 (0.017)	-0.02 (0.033)	-0.027** (0.011)	-0.003 (0.013)
Not metro adjacentXPost	-0.015 (0.020)	-0.016 (0.030)	0.02 (0.021)	0.024* (0.014)
Lower tax interiorXPost	-0.046 (0.033)	-0.011 (0.032)	-0.008 (0.016)	-0.003 (0.021)
Higher tax interiorXPost	0.01 (0.026)	-0.011 (0.043)	0.018 (0.025)	0.002 (0.009)
Lower tax borderXPost	-0.070*** (0.025)	-0.002 (0.029)	-0.017 (0.017)	0.013 (0.009)
Higher tax borderXPost	0.029 (0.030)	-0.018 (0.054)	-0.013 (0.021)	0.012 (0.011)
DestinationXPost	-0.045 (0.042)	0.069** (0.033)	-0.104*** (0.031)	-0.006 (0.020)
High nexus thresholdXPost	0.003 (0.022)		0.009 (0.022)	-0.078*** (0.019)
N	4704	17008	5286	17986

This table shows heterogeneous effects by characteristics of the jurisdiction and characteristics of the state that the jurisdiction is in. Here we focus on states that adopt economic nexus legislation, using the sample of states releasing quarterly data, including states with monthly data aggregated to the quarterly level. High-income indicates the jurisdiction has an above median household income; high Internet means the jurisdiction has an above median amount of broadband usage as measured in the ACS; metro adjacent and not metro adjacent are as defined by USDA (with metropolitan counties as the omitted category); low-tax [high-tax] side of the border means the jurisdiction is located on the low-tax [high-tax] side relative to its nearest neighboring state; a county is adjacent to the state border if it is contiguous to the state border and has a major road crossing (Agrawal, 2015) into the neighboring state, otherwise the county is interior; a town is adjacent to the state border if it is in an adjacent county, otherwise it is interior; destination means the jurisdiction is in a state that uses destination-based sourcing on remote sales (a small number of states use a quasi-origin basis); a jurisdiction has a high nexus threshold if the state has a dollar value of total sales per capita being shipped into the state that is above the median state. If a variable coefficient is missing it is because that state characteristic is colinear with other state characteristics. Standard errors are clustered at the primary statistical area, which includes all combined statistical areas and core-based statistical areas not part of a combined statistical area. Areas outside primary statistical areas are grouped within each state. *** 99%, ** 95%, * 90%.

Table A.10: Heterogeneity for Places Adopting Marketplace (Quarterly)

	Small counties	Small towns	Large counties	Large towns
Post	0.068 (0.048)	-0.071 (0.048)	0.212*** (0.065)	0.148*** (0.033)
High incomeXPost	-0.05 (0.035)	0.054 (0.045)	-0.065*** (0.020)	-0.024 (0.017)
High internetXPost	-0.031 (0.041)	0.051* (0.029)	-0.029 (0.022)	-0.022* (0.013)
Metro adjacentXPost	-0.047 (0.029)	0.045 (0.030)	-0.014 (0.017)	0.01 (0.015)
Not metro adjacentXPost	-0.03 (0.032)	0.055 (0.040)	-0.113* (0.059)	0.000 (0.022)
Lower tax interiorXPost	0.015 (0.033)	0.059 (0.037)	0.079*** (0.025)	0.041*** (0.015)
Higher tax interiorXPost	-0.065 (0.041)	-0.057 (0.066)	0.064 (0.054)	-0.002 (0.013)
Lower tax borderXPost	0.019 (0.028)	0.03 (0.036)	0.058** (0.029)	0.030** (0.012)
Higher tax borderXPost	-0.057 (0.038)	0.075 (0.091)	0.046 (0.052)	0.006 (0.017)
DestinationXPost	0.047 (0.032)	0.009 (0.035)	-0.115** (0.052)	-0.103*** (0.027)
High nexus thresholdXPost	0.015 (0.046)		-0.075 (0.060)	-0.055* (0.031)
N	1383	2019	1955	3945

This table shows heterogeneous effects by characteristics of the jurisdiction and characteristics of the state that the jurisdiction is in. Here we focus on states that adopt Marketplace Facilitator legislation, using the sample of states releasing quarterly data, including states with monthly data aggregated to the quarterly level. High-income indicates the jurisdiction has an above median household income; high Internet means the jurisdiction has an above median amount of broadband usage as measured in the ACS; metro adjacent and not metro adjacent are as defined by USDA (with metropolitan counties as the omitted category); low-tax [high-tax] side of the border means the jurisdiction is located on the low-tax [high-tax] side relative to its nearest neighboring state; a county is adjacent to the state border if it is contiguous to the state border and has a major road crossing (Agrawal, 2015) into the neighboring state, otherwise the county is interior; a town is adjacent to the state border if it is in an adjacent county, otherwise it is interior; destination means the jurisdiction is in a state that uses destination-based sourcing on remote sales (a small number of states use a quasi-origin basis); a jurisdiction has a high nexus threshold if the state has a dollar value of total sales per capita being shipped into the state that is above the median state. If a variable coefficient is missing it is because that state characteristic is colinear with other state characteristics. Standard errors are clustered at the primary statistical area, which includes all combined statistical areas and core-based statistical areas not part of a combined statistical area. Areas outside primary statistical areas are grouped within each state. *** 99%, ** 95%, * 90%.

Table A.11: Heterogeneity for Places Adopting Both Nexus and Marketplace (Quarterly)

	Small counties	Small towns	Large counties	Large towns
Post	0.082** (0.038)	0.139* (0.078)	0.161*** (0.025)	0.060* (0.031)
High incomeXPost	-0.02 (0.022)	-0.021 (0.022)	-0.007 (0.015)	-0.004 (0.011)
High internetXPost	0.029 (0.033)	-0.001 (0.023)	-0.021 (0.015)	0.007 (0.012)
Metro adjacentXPost	-0.018 (0.038)	-0.015 (0.028)	-0.023 (0.020)	-0.006 (0.017)
Not metro adjacentXPost	-0.044 (0.049)	-0.026 (0.027)	0.001 (0.021)	0.014 (0.017)
Lower tax interiorXPost	-0.05 (0.043)	0.002 (0.044)	-0.047*** (0.016)	0.009 (0.021)
Higher tax interiorXPost	-0.038 (0.042)	0.03 (0.043)	-0.044*** (0.013)	0.009 (0.014)
Lower tax borderXPost	-0.023 (0.042)	0.029 (0.051)	-0.02 (0.031)	0.039* (0.020)
Higher tax borderXPost	-0.005 (0.041)	-0.017 (0.044)	-0.029* (0.017)	0.016 (0.022)
DestinationXPost	-0.005 (0.056)	0.012 (0.058)	-0.044* (0.025)	-0.041** (0.018)
High nexus thresholdXPost	0.026 (0.046)	-0.077*** (0.026)	0.032 (0.024)	0.003 (0.018)
N	4988	23412	4784	24183

This table shows heterogeneous effects by characteristics of the jurisdiction and characteristics of the state that the jurisdiction is in. Here we focus on states that adopt economic nexus and Marketplace Facilitator legislation concurrently, using the sample of states releasing quarterly data, including states with monthly data aggregated to the quarterly level. High-income indicates the jurisdiction has an above median household income; high Internet means the jurisdiction has an above median amount of broadband usage as measured in the ACS; metro adjacent and not metro adjacent are as defined by USDA (with metropolitan counties as the omitted category); low-tax [high-tax] side of the border means the jurisdiction is located on the low-tax [high-tax] side relative to its nearest neighboring state; a county is adjacent to the state border if it is contiguous to the state border and has a major road crossing (Agrawal, 2015) into the neighboring state, otherwise the county is interior; a town is adjacent to the state border if it is in an adjacent county, otherwise it is interior; destination means the jurisdiction is in a state that uses destination-based sourcing on remote sales (a small number of states use a quasi-origin basis); a jurisdiction has a high nexus threshold if the state has a dollar value of total sales per capita being shipped into the state that is above the median state. Standard errors are clustered at the primary statistical area, which includes all combined statistical areas and core-based statistical areas not part of a combined statistical area. Areas outside primary statistical areas are grouped within each state. *** 99%, ** 95%, * 90%.

Table A.12: Heterogeneity for Places Adopting Economic Nexus (Monthly)

	Small counties	Small towns	Large counties	Large towns
Post	0.129** (0.055)	0.111*** (0.023)	0.123*** (0.040)	0.137*** (0.028)
High incomeXPost	-0.025 (0.036)	-0.006 (0.019)	-0.008 (0.013)	0.008 (0.009)
High broadbandXPost	-0.025 (0.023)	0.028* (0.015)	-0.032* (0.017)	0.008 (0.012)
Metro adjacentXPost	-0.011 (0.016)	-0.017 (0.021)	-0.027* (0.015)	0.007 (0.012)
Not metro adjacentXPost	-0.028 (0.023)	-0.025 (0.019)	0.016 (0.028)	0.021 (0.016)
Lower tax interiorXPost	-0.035 (0.032)	0.023 (0.015)	0.011 (0.018)	0.031 (0.030)
Higher tax interiorXPost	0.029 (0.029)	-0.040* (0.022)	0.028 (0.028)	0.001 (0.009)
Lower tax borderXPost	-0.073*** (0.023)	0.038* (0.021)	-0.015 (0.020)	0.027* (0.015)
Higher tax borderXPost	0.058** (0.029)	0.006 (0.039)	-0.002 (0.023)	-0.014 (0.012)
DestinationXPost	-0.048 (0.053)	0.070*** (0.019)	-0.080** (0.037)	-0.037** (0.016)
High nexus thresholdXPost	-0.021 (0.044)		0.021 (0.030)	-0.092*** (0.021)
N	8887	12512	11033	14106

This table shows heterogeneous effects by characteristics of the jurisdiction and characteristics of the state that the jurisdiction is in. Here we focus on states that adopt economic nexus legislation, using the sample of states releasing monthly data. High-income indicates the jurisdiction has an above median household income; high Internet means the jurisdiction has an above median amount of broadband usage as measured in the ACS; metro adjacent and not metro adjacent are as defined by USDA (with metropolitan counties as the omitted category); low-tax [high-tax] side of the border means the jurisdiction is located on the low-tax [high-tax] side relative to its nearest neighboring state; a county is adjacent to the state border if it is contiguous to the state border and has a major road crossing (Agrawal, 2015) into the neighboring state, otherwise the county is interior; a town is adjacent to the state border if it is in an adjacent county, otherwise it is interior; destination means the jurisdiction is in a state that uses destination-based sourcing on remote sales (a small number of states use a quasi-origin basis); a jurisdiction has a high nexus threshold if the state has a dollar value of total sales per capita being shipped into the state that is above the median state. If a variable coefficient is missing it is because that state characteristic is colinear with other state characteristics. Standard errors are clustered at the primary statistical area, which includes all combined statistical areas and core-based statistical areas not part of a combined statistical area. Areas outside primary statistical areas are grouped within each state. *** 99%, ** 95%, * 90%.

Table A.13: Heterogeneity for Places Adopting Marketplace (Monthly)

	Small counties	Small towns	Large counties	Large towns
Post	0.128** (0.050)	-0.024 (0.050)	0.150* (0.090)	0.070* (0.040)
High incomeXPost	-0.067*** (0.020)	0.021 (0.020)	-0.015 (0.020)	0.011 (0.020)
High broadbandXPost	0.046 (0.030)	0.022 (0.020)	-0.028 (0.020)	-0.004 (0.010)
Metro adjacentXPost	-0.024 (0.020)	0.005 (0.030)	0.025 (0.020)	0.019 (0.010)
Not metro adjacentXPost	0.006 (0.030)	0.025 (0.030)	-0.102* (0.050)	0.006 (0.020)
Lower tax interiorXPost	0.013 (0.030)	0.055* (0.030)	0.110*** (0.030)	0.075*** (0.020)
Higher tax interiorXPost	-0.033 (0.050)	-0.016 (0.070)	0.033 (0.040)	0.008 (0.020)
Lower tax borderXPost	-0.02 (0.030)	0.065* (0.040)	0.085** (0.030)	0.034* (0.020)
Higher tax borderXPost	-0.002 (0.060)	0.064 (0.080)	0.062 (0.050)	0.014 (0.020)
DestinationXPost	-0.034 (0.040)	0.045 (0.050)	-0.124* (0.070)	-0.097*** (0.030)
High nexus thresholdXPost	-0.009 (0.060)		-0.087 (0.080)	-0.031 (0.040)
N	4113	9210	4991	11657

This table shows heterogeneous effects by characteristics of the jurisdiction and characteristics of the state that the jurisdiction is in. Here we focus on states that adopt Marketplace Facilitator legislation, using the sample of states releasing monthly data. High-income indicates the jurisdiction has an above median household income; high Internet means the jurisdiction has an above median amount of broadband usage as measured in the ACS; metro adjacent and not metro adjacent are as defined by USDA (with metropolitan counties as the omitted category); low-tax [high-tax] side of the border means the jurisdiction is located on the low-tax [high-tax] side relative to its nearest neighboring state; a county is adjacent to the state border if it is contiguous to the state border and has a major road crossing (Agrawal, 2015) into the neighboring state, otherwise the county is interior; a town is adjacent to the state border if it is in an adjacent county, otherwise it is interior; destination means the jurisdiction is in a state that uses destination-based sourcing on remote sales (a small number of states use a quasi-origin basis); a jurisdiction has a high nexus threshold if the state has a dollar value of total sales per capita being shipped into the state that is above the median state. If a variable coefficient is missing it is because that state characteristic is colinear with other state characteristics. Standard errors are clustered at the primary statistical area, which includes all combined statistical areas and core-based statistical areas not part of a combined statistical area. Areas outside primary statistical areas are grouped within each state. *** 99%, ** 95%, * 90%.

Table A.14: Heterogeneity for Places Adopting Both Nexus and Marketplace (Monthly)

	Small counties	Small towns	Large counties	Large towns
Post	0.119*** (0.045)	0.176*** (0.052)	0.129*** (0.026)	0.024 (0.031)
High incomeXPost	-0.005 (0.024)	-0.004 (0.021)	0.003 (0.015)	-0.007 (0.016)
High broadbandXPost	0.009 (0.029)	0.032 (0.030)	-0.037** (0.017)	-0.005 (0.016)
Metro adjacentXPost	-0.02 (0.036)	-0.050* (0.028)	0 (0.018)	-0.002 (0.016)
Not metro adjacentXPost	-0.046 (0.048)	-0.043 (0.030)	0.001 (0.022)	0.001 (0.017)
Lower tax interiorXPost	-0.052 (0.052)	-0.116*** (0.033)	-0.048*** (0.017)	-0.01 (0.029)
Higher tax interiorXPost	-0.037 (0.051)	-0.077** (0.039)	-0.042*** (0.014)	-0.005 (0.028)
Lower tax borderXPost	-0.026 (0.052)	-0.078* (0.041)	-0.01 (0.037)	0.014 (0.032)
Higher tax borderXPost	0.001 (0.052)	-0.089** (0.041)	-0.026 (0.018)	-0.006 (0.032)
DestinationXPost	-0.043 (0.062)	0.025 (0.042)	-0.022 (0.022)	-0.004 (0.018)
High nexus thresholdXPost	0.049 (0.048)	-0.003 (0.027)	0.013 (0.023)	0.051** (0.022)
N	11164	21459	11377	22290

This table shows heterogeneous effects by characteristics of the jurisdiction and characteristics of the state that the jurisdiction is in. Here we focus on states that adopt economic nexus and Marketplace Facilitator legislation concurrently, using the sample of states releasing monthly data. High-income indicates the jurisdiction has an above median household income; high Internet means the jurisdiction has an above median amount of broadband usage as measured in the ACS; metro adjacent and not metro adjacent are as defined by USDA (with metropolitan counties as the omitted category); low-tax [high-tax] side of the border means the jurisdiction is located on the low-tax [high-tax] side relative to its nearest neighboring state; a county is adjacent to the state border if it is contiguous to the state border and has a major road crossing (Agrawal, 2015) into the neighboring state, otherwise the county is interior; a town is adjacent to the state border if it is in an adjacent county, otherwise it is interior; destination means the jurisdiction is in a state that uses destination-based sourcing on remote sales (a small number of states use a quasi-origin basis); a jurisdiction has a high nexus threshold if the state has a dollar value of total sales per capita being shipped into the state that is above the median state. Standard errors are clustered at the primary statistical area, which includes all combined statistical areas and core-based statistical areas not part of a combined statistical area. Areas outside primary statistical areas are grouped within each state. *** 99%, ** 95%, * 90%.