Statutory Incidence and Sales Tax Compliance: Evidence from *Wayfair*

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Abstract

This paper studies the compliance, pricing, and progressivity effects of changing statutory incidence. The *Wayfair* Supreme Court decision granted U.S. states new authority to shift the statutory incidence of taxation from consumers to online and out-of-state retailers, forcing supply-side remittance of the sales tax. With state-level data, we find this increased sales tax revenues by 7.9 percent, concentrated in states with stringent compliance standards. With barcode-level scanner data, we find evidence of full pass-through of the tax to consumers. Effects are progressive however, with the reforms increasing tax liabilities principally for higher-income households.

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Keywords: sales tax; compliance; state and local public finance; statutory incidence.

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1 Introduction

Classical tax theory teaches that elasticities determine economic incidence but ignores the details of remittance requirements and other administrative issues. While the importance of enforcement and compliance to tax policy has been recognized for some time (Slemrod, 1990), economists have only in the last decade devoted significant attention to these issues in empirical analyses (Slemrod, 2019). Statutory incidence, which is the side of the market given the legal responsibility for remitting a tax or "writing the check," is of limited theoretical importance but may matter a lot in practice. In this paper we exploit the *Wayfair* judgement (details below) to estimate the advantages of retailer compliance versus individual compliance. Using state-level tax revenue data, we find that shifting tax remittance responsibilities from individual consumers to out-of-state sellers substantially increased tax revenues. Further, using micro-level barcode data, we show the additional taxes were primarily borne by high-income households. In other words, tax revenue is not invariant to the side of the market that is responsible for compliance, and economic incidence is significantly affected by the different responses to the enforcement regime across income groups.

Until recently, shopping online or through mail-order services provided consumers an easy way to avoid sales taxes on their purchases (Ellison and Ellison, 2009; Einav, Knoepfle, Levin and Sundaresan, 2014). Out-of-state sellers with no physical presence in a state had no legal duty to collect the taxes owed, and when those sellers failed to collect the tax, consumers were obligated to self-declare their untaxed purchases and remit the taxes themselves. Many consumers were unaware of their responsibilities, or chose to ignore them, and states largely refrained from imposing the kind of third-party reporting on sellers necessary to enforce use tax obligations. Consequently, the internet helped create a significant tax haven for online shoppers (Goolsbee, 2000; Agrawal, 2021).[1]

With the 2018 Supreme Court decision in *South Dakota v. Wayfair*, states gained authority to require out-of-state firms with no physical presence but sufficient economic presence, or nexus, to collect the sales tax, thereby shifting compliance responsibility from consumers and onto sellers. Legislatures quickly adopted laws requiring supply-side compliance with and remittance of the

[1] By the most recent official estimate, e-commerce comprises 14.3 percent of all retail sales (U.S. Department of Commerce, 2022). As sales taxes account for more than one-quarter (29.1 percent) of state governments' tax revenue (`http://www.taxadmin.org/assets/docs/Research/Rates/21taxdis.xls`), the online retail landscape, and the tax compliance considerations it generates, is a substantial policy issue.

sales tax by the affected firms. The legislative responses came in two waves. The first wave, which asserted an economic nexus standard,[2] applied tax collection requirements on large retailers like Wayfair or IKEA, which had no physical presence in most states. The second wave, which impacted marketplace facilitators, required platforms such as eBay or Etsy that host large numbers of smaller sellers to collect sales tax on all transactions on the platform.

We use the roll-out of these *Wayfair* laws to investigate the effects of statutory incidence on tax collection. Note that these laws apply only to out-of-state sellers. They do not shift compliance requirements for companies that have an existing bricks-and-mortar presence in a state.[3] Our main identification strategy takes advantage of this feature. The differential effect for online versus bricks-and-mortar vendors permits a triple-difference research design: treatment and non-treatment states, before and after enforcement, and online versus bricks-and-mortar vendors. The design exploits differences in the prices households pay for the same product across these retail channels. Our results are precisely estimated, and we find no evidence of spillovers between the two channels.

Using a novel panel of monthly tax revenues at the state level, we show that enforcing nexus on out-of-state vendors like Wayfair increased revenues by 1.8 percent on average and accounting for the compliance of marketplace facilitators like eBay, the total effect increases to 7.9 percent. We exploit a feature of the *Wayfair* decision to test for heterogeneous effects. Revenue gains are driven by states with more stringent enforcement criteria. The point estimates are approximately twice as large (and significant) for states with lower per capita (more stringent) thresholds. Point estimates are generally larger for the marketplace facilitator treatments than the economic nexus treatments, reinforcing the finding from Kopczuk, Marion, Muehlegger and Slemrod (2016) that moving the point-of-collection up the supply chain tends to increase revenue gains. Decomposing the results to exclude timing effects (as per Goodman-Bacon, 2021) suggests the average increase in revenue could be closer to 10 percent, though we urge caution in interpreting this result due to reduced statistical power.

Using barcode-level data on the weekly purchases of 100,000 households, we present four

[2] As discussed more fully below, this standard is met when a firm reaches a threshold level of sales (e.g. $100,000 a year) into a state.

[3] In this paper, we refer to firms with physical presence as bricks-and-mortar firms, but we are including all firms that had nexus in a state, regardless of the reason. Other factors that might trigger nexus were making deliveries into a state or deploying workers to perform repair or other services in a state.

additional empirical results at the micro-level. First, aggregate pre-tax prices did not change when states imposed statutory incidence on sellers. This is consistent with full pass-through of the tax to consumers. Second, taxes paid for online transactions increased by 5.0 percent. Third, these taxes were raised progressively. Lower income consumers saw a relatively modest 3.8 percent increase in sales taxes paid, versus 6.3 percent more in taxes for higher income consumers. Fourth, we find only limited evidence that consumers switched to lower-priced goods, consistent with theory that broad-based increases in sales taxes should not distort price ratios. Collectively these results indicate that the shift in statutory incidence results in an effective tax rate increase and the change is progressive with respect to the income distribution. Thus, the new enforcement regime reduced the overall regressivity of the sales tax and indicates that the online shopping loophole contributed to the regressivity of the sales tax.

Some recent research has recognized how administration and enforcement can impact key aspects of taxation.[4] Hansen, Miller and Weber (2020) find that tax invariance did not hold when Washington state shifted taxes on cannabis from a gross receipts tax imposed on manufacturers to a roughly revenue neutral retail sales tax. Kopczuk et al. (2016) demonstrate that diesel taxes statutorily imposed on distributors raised more revenue than equivalent taxes on retailers; and Bibler, Teltser and Tremblay (2018) show cities that entered collection agreements with Airbnb experienced higher after-tax prices and improved tax compliance.

Our paper provides a number of key contributions to the small but growing literature on the real effects of statutory incidence. This paper is the first to simultaneously show real effects of statutory incidence on both tax revenues and micro-level consumption behavior, and the first to show the impacts of statutory incidence broadly across the income distribution. We present evidence that the shift of statutory incidence to sellers lessens the overall regressivity of the sales tax. Prior research (e.g. Poterba, 1996; Ross and Lozano-Rojas, 2018; Nelson and Moran, 2019) finds the sales tax is forward shifted to consumers, but our work suggests incidence is heterogeneous and nuanced, at least with respect to income. We find that low-income consumers pay relatively less additional tax post-*Wayfair*, possibly due to the exemption of necessities from the tax base, or perhaps because they seek out smaller sellers without a compliance responsibility.

The importance of the issues addressed in this paper is highlighted by the emergence of recent

[4]See Slemrod (2019) for an extensive review of the recent literature on tax administration and enforcement.

related work; e.g. Cashin and Unayama (2016) show lower-income households were more responsive to a VAT reform in Japan; Lozano-Rojas and Ross (2019) find sales tax holidays are broadly progressive; and Harding, Leibtag and Lovenheim (2012) report less pass-through of cigarette taxes for low-income households. Similarly, Goldin and Homonoff (2013) investigate the salience of sales versus excise taxes on cigarettes; Bradley and Feldman (2020) find more prominent display of tax-inclusive fares reduced consumer-side incidence in airline ticket prices; and Hargaden and Roantree (2019) show earnings in Ireland were less responsive to taxes on employers than taxes on employees. Kacamak, Velayudan and Wilking (2020, "KVW") also uses the Nielsen Consumer Panel paper to study incidence, but there are important differences from our study. First, KVW examine Voluntary Collection Agreements (VCA) between large online retailers (like Amazon), with a single state. Our paper examines the impact of *Wayfair* laws on potentially any online firm across all sales-taxing states. Second, after Amazon and other large firms signed VCAs, consumers could still shop online and avoid sales tax at many of our nation's largest online sellers, and tens of thousands of smaller online sellers, some of them selling on Amazon's own marketplace. Our paper therefore captures a broader set of firms and examines the impact on consumers with far fewer options to avoid sales tax on their purchases. The marketplace facilitator laws enacted post-*Wayfair* are a test of compliance further up the supply chain and are consistent with the results in KVW. Third, KVW's focus is on pre-*Wayfair* environment, and therefore, there is no overlap in years for the two studies. Finally, our paper includes a substantial macro-level analysis and examines the impact of the *Wayfair* laws on state revenues.

This paper proceeds as follows: Section 2 provides background and institutional detail on sales tax compliance. Section 3 describes the tax enforcement treatments we analyze, and presents our findings on the state-level data. Section 4 shows the effects of the treatment on individual-level consumption patterns, focusing on the differences between high- and low-income households. Section 5 concludes.

2 Compliance with State Sales Taxes

For approximately 50 years prior to the *South Dakota v. Wayfair* decision,[5] two U.S. Supreme Court decisions[6] drove enforcement of the sales tax on remote vendors. The Court ruled that a firm must have physical presence in a state to create sufficient "nexus" for a state to require an out-of-state seller to collect sales taxes on sales into that state. Under the rules permitted by the Court, sales tax compliance operates through two mechanisms. First, selling firms with nexus collect and remit sales tax for the destination state for taxable business and individual sales. Second, all sales-taxing states have a companion use tax, which requires the buyer to remit the tax when the seller does not have a compliance responsibility.[7]

The 2018 *Wayfair* ruling radically altered the sales tax compliance and enforcement regime by eliminating the physical presence nexus standard and ruling that sufficient economic presence in a state is enough to subject a seller to that state's sales tax collection requirements. South Dakota imposed a minimum threshold of sales into the state (i.e., $100,000 or 200 transactions) before it requires compliance from remote vendors. By approving South Dakota's regime, the Court effectively created a safe harbor for what constitutes sufficient economic presence. Thus, *Wayfair* has been interpreted as moving the required compliance standard from one that depends only on physical presence to one that requires compliance by firms with substantial economic presence in a state, measured by sales volume or sales dollars.

There is evidence that individual- or consumer-based compliance regimes are relatively ineffective. Manzi (2015) demonstrates that individual compliance with the use tax is very poor, with only 2 percent of individual tax returns reporting nonzero use tax liability. Only $133 million was collected in 2012 across the twenty-seven states with a use tax line on the income tax return, which is a trivial sum compared to total sales tax collections for all states of $375 billion.[8]

Limited research indicates that business compliance with the use tax is also poor. For example,

[5] Kennedy, Anthony, 2018. "Opinion: *South Dakota v. Wayfair, Inc., et al.*" 585 U.S. `https://www.supremecourt.gov/opinions/17pdf/17-494_j4el.pdf`.

[6] *National Bellas Hess v. Illinois Department of Revenue* (1967) and *Quill Corp. v. North Dakota* (1992).

[7] The tax is frequently referred to as a use tax if an item is purchased outside the market state for use in the market state. We ignore the specific legal distinctions between sales and use taxes for this study. Use taxes remain after the *Wayfair* decision for other transactions where the seller fails to remit tax. See Mikesell and Ross (2019) for an excellent study that analyzes, among others, use tax versus sales tax.

[8] U.S. Census Bureau, "2012 State Tax Collections" `https://www.census.gov/content/dam/Census/newsroom/releases/2013/cb13-67_infographic.pdf`.

Washington State's most recent compliance study (2018) finds that the business use tax had the largest noncompliance of all business taxes examined, at 14.9 percent, which represents significant improvement from the estimated 21.5 percent noncompliance for 2016. But, remote business collection of the sales tax may also be weak. Post *Wayfair*, most states lack the capacity to audit out-of-state vendors to determine which firms have nexus under new state statutes, and the seller population is large and geographically dispersed. There are more than 50,000 merchants with sales exceeding $500,000 on the Amazon Marketplace alone.[9] Thus, our study examines how firms respond given the somewhat limited capacity to enforce the tax.

Slemrod (2017) argues that the standard deterrence model for taxation may not apply well to large corporations, which should be risk neutral rather than risk averse. This suggests that the larger firms to which *Wayfair* economic nexus statutes apply may be more willing to take the risk that states will not identify them as having nexus than smaller firms would be if they were required to remit.

Despite these challenges, seller-based compliance potentially offers a number of advantages. First, compliance by sellers rather than buyers is likely to reduce administrative costs as it consolidates the number of returns that must be filed.[10] Second, existing empirical evidence suggests vendors evade less than buyers (Bibler et al., 2018; Kopczuk et al., 2016). While Slemrod (2017) notes less evasion does not imply higher social welfare, greater revenues could in principle permit lower rates, reducing efficiency losses.[11] Third, compliance by both remote and in-state vendors eliminates the tax wedge that creates incentives to purchase from remote/online vendors (Fox, Luna and Schaur, 2014). This tax wedge may provide a tax-inclusive cost advantage to less efficient firms, with Zodrow (2006) describing the tax advantage afforded to online firms as "unlikely to be even close to optimal." Finally, seller compliance may facilitate better enforcement of destination taxation, which can reduce the welfare losses of tax competition (Kanbur and Keen, 1993).

The evidence on seller versus buyer compliance suggests that consumers' behavior is consis-

[9]See "Amazon Marketplace Sellers Rake in $160 billion in 2018" Digital Commerce 360 `https://www.digitalcommerce360.com/2019/05/09/amazon-marketplace-sellers-rake-in-160-billion-in-2018/`.

[10]Relatively little is known about sales tax compliance costs. PriceWaterhouseCoopers (2007) estimates that sales tax compliance costs were 13.5 percent of tax revenues for small retailers, 5.2 percent for medium retailers, and 2.2 percent for large retailers. Presumably, compliance costs for buyers would often parallel those for small retailers.

[11]States have frequently raised their sales tax rate during the past four decades. Thirty-three states raised their sales tax rate between 2000 and 2009 and 15 states increased their rate between 2010 and 2018 (albeit some only temporarily). The revenue gains from post-*Wayfair* reforms could be used to arrest this trend of tax increases.

tent with attempts to avoid sales taxes when possible and that many buyers shifted their purchases to out-of-state e-commerce vendors who did not collect the tax (Ellison and Ellison, 2009). The same behavioral response would now require shifting towards smaller or evading remote vendors, and Baugh, Ben-David and Park (2018) find that purchases from Amazon fell significantly when Amazon began to collect sales taxes for Ohio, evidencing the type of movement away from vendors that could take place as only larger vendors collect the tax. Similarly, Einav et al. (2014) find that buyers confronted with a sales tax that they were not anticipating tend to cancel the transaction, estimating that a one percent increase in the sales tax rate increases state residents' out-of-state e-commerce purchases by almost two percent. Eliminating the tax wedge could result in significant sales increases for in-state online and bricks-and-mortar retailers. All the more so, if finding a vendor not remitting the tax is more difficult in the future.

3 Effects on Tax Revenue

3.1 Variable Definitions and Descriptions

We begin our analysis by examining how changes in the enforcement regime impact sales tax revenues. To analyze the effects of the *Wayfair* decision on state sales tax revenue, we collected a panel of state-level monthly sales and use tax revenue by month from January 2013 through March 2020.[12] Sales tax revenues are generally cash basis receipts that are collected in the month after the sale takes place.[13] *Wayfair* was decided on June 21, 2018, so August 2018 is the first time that a complete month's sales tax activity could be affected by expectations after the decision was released. Note that sales tax revenues include both traditional consumer sales and any relevant business-to-business transactions.

By ruling that South Dakota's definition of nexus was not an undue burden on interstate commerce, the Supreme Court implicitly permits other states to enact similar statutes. In effect, the Supreme Court gave states the right to enforce sales tax compliance on firms outside of their

[12]The Federation of Tax Administrators granted access to its detailed monthly data for thirty-eight of the states, which serves as the main data source. We are very grateful to Ron Alt, Research Director of FTA, for graciously providing these data. Additional data are collected from publicly available online state-level reports, and some directly from state tax authorities, resulting in monthly data on forty-five states (plus Washington, D.C.) which impose the tax. We thank John Peloquin of Minnesota Management and Budget economic analysis unit for assisting with data collection.

[13]Cash basis data generally contain any receipts during the month and can include late payments, penalties and other collections that link to sales in earlier months.

state borders and shifted the statutory burden from instate consumers to out-of-state firms. States adapted and created their own laws over the next 18 months following the decision. States typically first passed laws asserting economic nexus on remote vendors, then enacted marketplace facilitator laws (discussed below). We use the staggered adoption of these laws to identify the revenue effects of the change in statutory incidence. We present the enactment dates in Appendix Table A1. Three states (Florida, Louisiana, and Missouri) had either not enacted economic nexus legislation or were not enforcing it during our analysis period. These three states, which have a sales tax, comprise the 'never adopters' in our analysis. Section 4, which looks at pricing at the individual product level, will include states with no sales tax as a control group.

A second wave of treatment emerged when states began enforcing compliance on marketplace facilitators, in addition to thresholds for individual firms. As marketplace facilitator thresholds apply to the aggregate activity of the marketplace rather than the individual seller, many smaller firms effectively become subject to a collection responsibility for their sales on marketplace platforms. The marketplace facilitator legislation also addresses a loophole post-*Wayfair* where retailers like Amazon and Walmart were legally required to collect tax on their own sales and those of large retailers operating on their platforms but were not required to collect sales taxes for small vendors operating on the marketplaces.[14] Collections may improve both for firms with newly generated nexus but also for previously non-compliant firms which satisfied nexus but perceived the enforcement mechanisms to be weak (Tyler, 2006). Enforcing compliance on the millions of smaller vendors may have substantial effects.[15] We show these enactment dates in Appendix Table A2.

South Dakota's law created a threshold for economic activity, which levies the sales tax on out-of-state firms selling more than $100,000 into the state. All states include a minimum threshold that generally lies in the range of $100,000 to $500,000.[16] Firms with activity below the threshold are not required to collect the sales tax. We will later use per capita revenue thresholds as a measure of compliance stringency/intensity of enforcement.

[14]To understand the distinction, consider the purchase of Levi's jeans on Amazon. Economic nexus legislation ensures the jeans are taxed if sold by Amazon but not by small vendors on Amazon. Marketplace facilitator legislation ensures the jeans are taxed if sold on Amazon by any vendor.

[15]Marketplace Pulse reports that 3 million firms actively operate on the Amazon platform, and a further 3.6 million on Etsy. Internet Retailer reports eBay had 25 million vendors in 2017.

[16]The one exception is Kansas, which has not specified a minimum dollar threshold. It remains to be seen whether this is constitutional. The full list of thresholds is presented in Table 4.

Post-*Wayfair* legislation combined with state thresholds potentially create behavioral responses from buyers.[17] Consumers who were formerly shopping remotely from firms without nexus have several options to continue avoiding/evading the tax, including not purchasing the item, purchasing non-taxable items, or shifting purchases to small vendors where the collection responsibility may still not exist. On the other hand, purchases from vendors that now collect the sales tax eliminate consumer compliance burdens and, to the extent that buyers were not remitting the tax they owed, represent new revenue. Our analysis measures increased vendor compliance and represents a lower-bound estimate of previous tax evasion, including buyers who did not remit the required use tax. We cannot measure the full extent of previous evasion because some vendors, and particularly remote vendors, may still be evading, and buyers may have previously offset vendor evasion by remitting the tax.

Table 1: Summary statistics from monthly sales tax revenue data

	Mean	Std. Dev	N	Min	Max
Monthly revenue ($ million)	498.93	576.2	4,002	40	3605
Log of revenue	19.59	0.9	4,002	18	22
Population (millions)	6.84	7.3	4,002	1	40
Nexus Threshold ($/000 pop)	46.00	43.4	3,741	0	172
Economic Nexus Treatment	0.14	0.3	4,002	0	1
Marketplace Facilitator Treatment	0.08	0.3	4,002	0	1
Sales tax rate	5.65	1.0	4,002	3	8
Membership in SSTP	0.51	0.5	3,915	0	1
Pre-treatment compliance level	190.34	60.8	2,700	85	358

Table 1 displays the summary statistics of the macro-level data used in the paper. The principal research question in this section is the extent to which vendors comply better than individuals with the sales tax, and how state revenues were affected by the post-*Wayfair* legislative responses. Table 1 also includes two variables not previously discussed, membership in SSTP and pre-treatment compliance level. The first variable, Membership in SSTP, records whether a state participates in the Streamlined Sales Tax Project. This is a voluntary association of states that collaborates on sales tax administration to reduce compliance burdens and collect a limited amount of voluntary remittance. Membership in the SSTP is one of five highlighted facets of South Dakota's law that

[17] *Wayfair* is generally discussed as applying to online transactions. However, the decision supports nexus for any remote firms including mail-order firms and bricks-and-mortar sellers that ship goods across state lines.

led to the *Wayfair* outcome (Afonso, 2019). Therefore, in theory, membership in SSTP could make it easier for firms to comply with sales tax law in different states, and possibly affect likelihood of enacting nexus legislation. The second variable, pre-treatment compliance level, is a pre-*Wayfair* measure of online sales tax compliance. In particular for each state, we hand collected how many top internet firms were collecting and remitting sales taxes for that state in 2016 by 'shopping' online at 500 large retailers, entering different zip codes, and recording whether sales tax was added.[18] Aside from the obvious example of Wayfair, a sample of large firms that we believe were not remitting sales taxes in most states are HSN, Netflix, Newegg, and Overstock. We use these variables to investigate if the treatment varies across different state-level observables like population or sales tax rate. Appendix Table A3 shows the treatment and control groups do not significantly vary on either the extensive 'ever enact' or intensive 'date of enactment' margins, for relevant observables. Tables A4 and A5 show somewhat more formal balance tests. It is worth noting that SSTP membership is a statistically significant predictor of passing economic nexus legislation, but conditional on enactment, it does not significantly affect the timing of enforcement. As with all time-invariant characteristics, this will be captured by the state fixed effects.

3.2 Econometric Estimates of Statutory Enforcement

The remainder of this section analyzes the treatment effects for both the economic nexus and marketplace facilitator enforcement regimes. We test robustness by examining the intensity of the treatment and decomposing the effects of enforcement regimes. The empirical strategy is a two-way fixed effect staggered treatment design. We estimate a regression equation of the form:

$$Y_{it} = \alpha_i + \alpha_t + X'_{it}\beta_x + \delta D_{it} + \epsilon_{it}$$

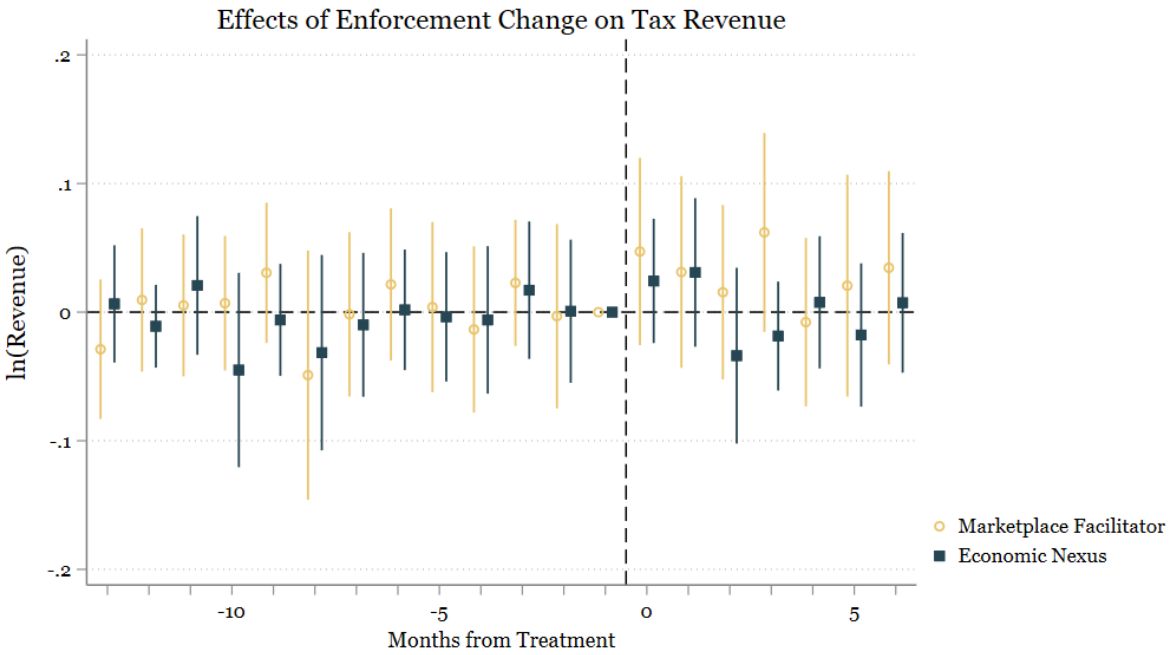
where Y_{it} is our outcome of interest (log of sales tax revenue) in state i in year-month t, and α_i and α_t are state and year-month fixed effects. We include state-specific linear time trends X_{it} to capture potentially important differences in how states might be systematically trending (Lindo, Schaller and Hansen, 2018; Ohrn, 2019).[19] Our tax enforcement treatments, shifting statutory incidence

[18] See Bruce, Fox and Luna (2015).

[19] Following Goodman-Bacon (2021), our group trends are estimated on exclusively pre-*Wayfair* dates, and predicted forward into post-*Wayfair* periods, to avoid any potential contamination with treatment. As a practical matter, exclud-

through post-*Wayfair* legislation, are denoted by D, and ϵ is the residual. Our coefficient of interest δ measures the effect of changing the collection requirements from a consumer to a seller (for economic nexus legislation) or to a platform (for marketplace facilitation legislation) on sales tax revenue. Using this log-linear specification, we can interpret δ in percent terms. To estimate percent changes in tax dollars raised correctly, we weight all results by baseline pre-treatment (2016) revenues. We two-way cluster the standard errors conservatively, at the state and year-month level.

Figure 1: Event study graph of both nexus and marketplace treatments



We start with an event study analysis, investigating the trends in revenue prior to treatment. This is depicted in Figure 1. Event studies for the two treatments separately are included as Figures B1 and B2 in Appendix B. Initial inspection of the event study graph shows no divergence in the trends in the pre-period. The event study is a regression of log revenue on a series of lags and leads around treatment. We partial-out month-year indicators, so the coefficients are estimated off within-month variation. Consequently a point estimate can be interpreted as the percent increase in revenues in a given month near treatment, relative to the same month in a state that is not treated. Due to the staggered nature of the design, post-period for the economic nexus treatment

ing time trends substantially reduces the precision of our estimates.

may be pre-period observations for the marketplace facilitator treatment. We will discuss these 'timing effects' later in the context of a Goodman-Bacon (2021) decomposition. Treatment effects appear weak for the economic nexus treatment, and possibly stronger for the marketplace facilitator treatment. Of course, month-by-month analyses will generally have larger standard errors than looking at overall effects. Table 2 shows the results from a regression framework.

Table 2: Effect of enforcement on states' tax revenue

	ln(Sales Tax Revenue)	
	(1)	(2)
Economic Nexus × Enforcement	4.22	1.78
	(2.55)	(2.07)
Marketplace Facilitator × Enforcement		6.17***
		(1.78)
State and Time FE	Yes	Yes
State-specific (pre-treatment) trends	Yes	Yes
Observations	4,002	4,002

Table shows effects of enforcement of economic nexus and marketplace facilitator legislation. Coefficients are multiplied by 100 for interpretation. Results weighted by baseline tax revenue, and standard errors are two-way clustered at the state and year-month level. The linear combination of both treatments sums to 7.9 percent, $p < 0.01$.

Column 1 shows that passage and enforcement of economic nexus legislation increases sales tax revenues by 4.2 percent. As a number of states subsequently passed marketplace facilitator legislation, it is important to interpret these results in conjunction with Column 2. This shows that the economic nexus legislation effect is substantially dampened (by more than one-half, to 1.8 percent) when accounting for marketplace facilitator enforcement. We see substantial gains from marketplace facilitator legislation, increasing revenues by about 6.2 percent. The combined effect is 7.9 percent.[20]

These results are best understood in the context of an actual example of state revenues. We obtained the specific figures for Tennessee through a data access request. We depict this graphically in Figure 2. In June 2018, the month of the *Wayfair* decision, Tennessee collected approximately $875,000 in state sales tax revenues from out-of-state/remote vendors. Two years later, after the

[20] A recent Government Accountability Office report (GAO-22-106016) documents $23 billion in revenue from *Wayfair* legislation. This represents 6.8% of the total $340 billion collected in state sales taxes in 2020. These data are based on 33 states reporting to the GAO.

Figure 2: Growth in Tennessee tax revenue since the *Wayfair* case

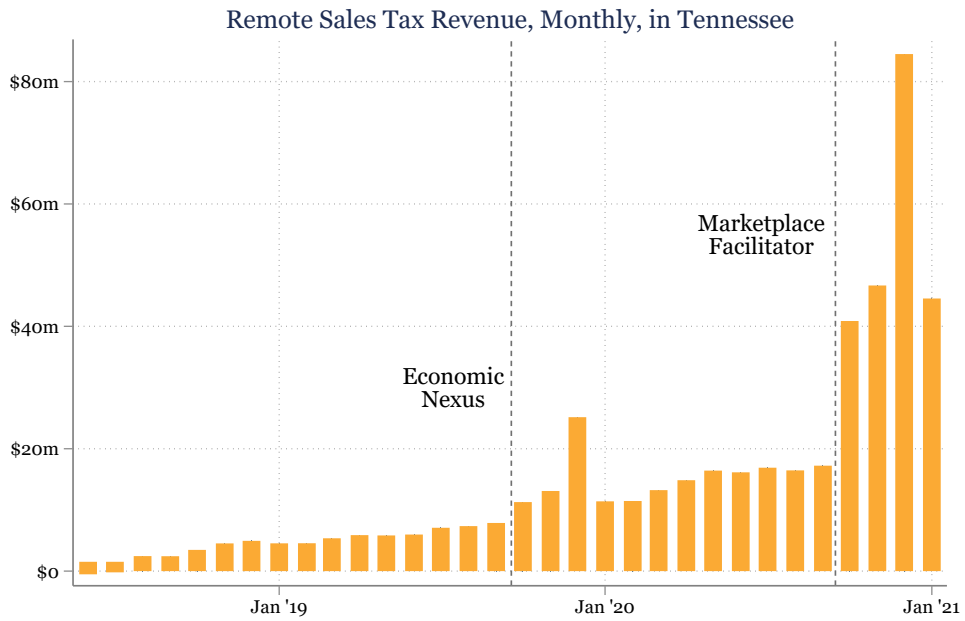


Figure shows Tennessee's monthly sales tax revenue from 'remote sellers' each month since *Wayfair* was decided. The dashed vertical bars represent when the state began enforcing collection requirements. *Data: TN Dept of Revenue.*

passage of nexus legislation, the number for the corresponding month had risen to nearly $15 million. While the seventeen-fold increase in remote sales is dramatic, $15 million represents about 2 percent of the state's overall monthly revenue stream. Following the passage of marketplace facilitation laws that figure more than doubled, to more than $40 million in a typical month — with revenue gains concentrated after enforcement for marketplace facilitator law. For a sense of scale, the Metro Nashville Public Schools budget is about $76 million per month.[21] This figure provides a concrete example underpinning our regression results, and these results should be considered in this context.

Staggered difference-in-differences (DD) designs have attracted renewed econometric attention recently (e.g. de Chaisemartin and d'Haultfoeuille, 2020). New methods have been developed to disentangle an overall point estimate into subcomponents (e.g. Goodman-Bacon, 2021). These developments have been motivated in part by the realization that staggered DD estimates incorporate already-treated units as a control group. To the extent that already-treated units are inflated relative to untreated units, incorporating already-treated units as a control group will push point

[21] Based on a total operating budget of $914m for FY2019-2020.

estimates towards zero. While not strictly a source of bias, this seems unsatisfactory. This is particularly relevant in our setting, where nexus treatment effects may bias the marketplace facilitator effects towards zero. Minimally, it is desirable to disentangle these timing effects from a purer treatment and control group, and compare the magnitude of the effects. The Goodman-Bacon decomposition does this, showing how an overall point estimate (e.g. 1.78 in our economic nexus case) is the weighted sum of timing effects and never-versus-treated groups. Table 3 shows this decomposition.

Table 3: Goodman-Bacon Decompositions

Treatment	Timing Groups (weight)	Never v. Timing	Within	**Overall Estimate**
Economic Nexus	−0.6 (64%)	15.0 (23%)	−9.6 (13%)	1.78
Marketplace	5.8 (56%)	12.1 (32%)	−7.7 (12%)	6.17

Table shows the Goodman-Bacon decomposition for the staggered difference-in-difference point estimates for economic nexus and marketplace facilitator treatments. The Never v. Timing estimates suggest treatment effects in excess of 10%.

Both economic nexus and marketplace facilitator point estimates are principally driven by timing groups, with timing groups representing more than fifty percent of the weight in both cases. Some of these timing groups use already-treated units as control groups and are thus potentially downward-biased. The never-versus-timing groups, which is perhaps the theoretically cleanest estimand but comes with disclaimers about statistical power,[22] finds shifting statutory incidence resulted in excess of 10 percent additional revenue for both economic nexus and marketplace facilitator treatments. This suggests our headline results in Table 2 may be conservative. Further, the Within component (which measures the impact of control variables like our state-specific trends) enters with a negative coefficient, suggesting that these controls deflate the headline result. Appendix Figures B3 and B4 present the Goodman-Bacon decompositions graphically. Appendix C provides results using the estimator of de Chaisemartin and d'Haultfoeuille (2020).

An intuitive robustness test of the effect of shifting statutory incidence on tax revenues is possible because of a unique feature of the *Wayfair* decision. In declaring South Dakota's revenue threshold of $100,000 of sales into the state as constitutional, the Court effectively endorsed a fixed-cost view of tax compliance. This $100,000 per state threshold generates implicit *per capita* variation. A revenue threshold of $100,000 is less likely to be satisfied in South Dakota (popula-

[22] Decomposing an overall result into subcomponents necessarily reduces inferential confidence.

tion 885,000) than in Illinois (population 12.7 million). Consequently, *Wayfair* and the subsequent legislative responses, create exploitable variation in the legal threshold/intensity of treatment: hypothetical firms with equivalent sales are more likely to be treated when selling into states with low per capita thresholds.

Table 4: Revenue thresholds firms must exceed to satisfy post-*Wayfair* nexus requirements

State	**Sales Threshold ($)**
FL, LA, MO	N/A
KS	0
CO, DC, HI, IA, ID, IL, IN, KY, MA, MD, ME, MI, MN, NC, ND, NE, NJ, NM, NV, OH, PA, RI, SC, SD, UT, VI, VT, WA, WI, WV, WY	100,000
AZ, OK	200,000
AL CT, GA, MS	250,000
CA, NY, TN, TX	500,000

States chose different dollar valued thresholds as shown in Table 4. A majority of states followed South Dakota's lead in adopting a $100,000 revenue threshold for asserting economic nexus for sales tax purposes. Ten states adopted higher thresholds, and this generates more variation to test if the stringency of standards affected tax revenue. We use the per capita threshold as a measure of stringency, or intensity, of treatment. Figure B6 depicts this variation.

As the stringency measure is a function of state population, it is not possible to disentangle it from a state fixed effect.[23] Using the median value we split the sample into two groups, Low Threshold per capita (Stringent Enforcement) and High Threshold per capita (Lenient Enforcement) and compare results. We otherwise retain the same specification as in Table 2. Table 5 shows the results, demarcated by per capita stringency.

The results conform with our expectations. Imposing a high per capita compliance threshold, namely a lenient enforcement regime, is associated with smaller revenue gains. Most of the revenue gains are concentrated in states with low per capita thresholds. For states with lenient enforcement, neither of the results are statistically distinguishable from zero — though some of that may be a consequence of reduced sample size. Testing the linear combination of both economic nexus and marketplace facilitator treatments implies a 3.6 percent revenue boost in strin-

[23] Any regression including a stringency measure with a state fixed effect will be estimating off changes in population, which is unlikely to capture the desired estimand.

gent states, but only 1.3 percent in states with lenient thresholds.[24]

Table 5: Effect of enforcement on states' ln(sales tax revenue), by treatment intensity

	Low Threshold per cap		**High Threshold per cap**	
	(1)	(2)	(3)	(4)
Economic Nexus × Enforcement	-0.10	-1.67	0.0085	-0.71
	(1.16)	(1.25)	(2.77)	(2.79)
Marketplace Facilitator × Enforcement		5.27**		2.00
		(1.93)		(3.42)
Joint Effect		3.61**		1.29
S.E.		1.72		3.93
State and Time FE	Yes	Yes	Yes	Yes
State-specific trends	Yes	Yes	Yes	Yes
Observations	1,914	1,914	1,827	1,827

Table shows effects of enforcement of economic nexus and marketplace facilitator legislation on revenue. The joint effect shows the linear combination of both. Regression coefficients are multiplied by 100 for interpretation. Results weighted by baseline tax revenue, and standard errors are clustered at the state and year-month level.

While the state-level findings should be considered supplementary to the subsequent analysis in Section 4, we do not wish to downplay the significance of the results. We find that shifting statutory incidence from resident consumers to out-of-state firms increased revenues. This is a broad-based rejection of the irrelevance of statutory incidence, supplementing the findings on diesel taxes (Kopczuk et al., 2016) and Airbnb (Bibler et al., 2018). The results are larger when applying the Goodman-Bacon (2021) decomposition and are strongest for states with more stringent compliance regimes. The results emphasize that shifting the enforcement regime to sellers increases revenues, and the degree to which revenue rises grows when the compliance responsibility is spread across a wider range of small firms. To investigate exactly how this macro-level effect on revenues affects micro-level behavior, we now shift our attention to scanner-level data.

4 Effects on Prices and Individual Tax Liabilities

The tax revenue effects identified in the previous section are an important policy concern, but state-level data do not lend themselves to determining the mechanisms driving the results. This section unpacks the macro-level results by examining consumption at the barcode-transaction

[24]Note that these regression coefficients are lower than our headline results because of the non-inclusion of 'Never adopters' in the high- versus low-threshold analysis.

level. This permits us to investigate the degree to which the tax is borne by buyers versus sellers, and the impact for buyers with different levels of income. The available window for the micro data extends through 2019, which permits analysis until just before the arrival of COVID-19 in the United States.

4.1 Data

We use Nielsen Consumer Panel scanner data to investigate the effects of economic nexus and marketplace facilitator laws on prices paid, quantities bought, and taxes borne by consumers.[25] These data have become a staple of the public finance literature (see e.g. Goolsbee, Lovenheim and Slemrod, 2010; Harding et al., 2012; Baggio, Chong and Kwon, 2018; Rozema, 2018; Conlon and Rao, 2020; Baker, Johnson and Kueng, 2021; Kroft, Laliberté, Leal-Vizcaíno and Notowidigdo, 2020), and permit separately estimating effects for online versus bricks-and-mortar channels. Einav, Leibtag and Nevo (2010) encourage caution over some measurement error in the Nielsen data but found it reliable overall.[26] See Dubois, Griffith and O'Connell (2022) for a discussion on the rise of barcode data in economics.

The Consumer Panel Data records purchases from approximately 60,000 U.S. households at any one time, and our coverage window is January 2015 through December 2019. Our sample includes approximately 100,000 total households who appear at least once. The data include basic demographic information on the household (income, age bracket, geographic location, etc.). Previously named Homescan, the data collection method involves households scanning the barcode of any purchase, inputting the retailer and price paid, and recording total tax-inclusive cost. Linking the transaction with the retailer ID allows us to see if the sale was from an online or mail-order retailer and if the recipient was living in a state where economic nexus laws had been enacted. The

[25]Our analyses are derived based in part on data from The Nielsen Company (US), LLC and marketing databases provided through the Nielsen Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the Nielsen data are those of the researchers and do not reflect the views of Nielsen. Nielsen is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

[26]The authors cite a recorded quantity as 11 instead of 1 as an example of measurement error in the data. This error would understate the pre-tax price of the good by a factor of eleven. Similarly, households that forget to scan an item will overstate the amount of taxes paid. Our identification strategy is not affected by these errors unless they are systematically correlated with treatment. Nonetheless, we further mitigate against these concerns by omitting observations we consider more susceptible to error. Specifically, we drop households with incomes less than $15,000 per year, products that cost less than $1, transactions with more than 200 individual items or where taxes are more than 25 percent of the total cost, and household-months with more than 200 trips or more than $5,000 in expenditure.

income data are grouped into bins of several thousand dollars each. Our 'High Income' measure includes respondents reporting household income in one of the two highest bins, namely at least $70,000 per year.[27] We define 'Low Income' as reporting an income below $70,000.[28]

The data are comprehensive, recording approximately one million separate Universal Product Codes (UPCs, e.g. a six-pack of 500ml Coca-Cola bottles) over the five years we analyze. These UPCs/barcodes are categorized by product category, e.g. 'Frozen Foods' or 'General Merchandise'. The results we present in Section 4.3 are robust to analysis on the subsample of goods that are typically taxable, e.g. excluding food and necessities. These results are reported in Appendix H.

We observe the pre-tax prices of goods in the data. While we do not observe the specific tax amount, we can infer the tax charged in a transaction as the difference between the pre-tax cost of the items and the total amount paid. In our analysis, we estimate taxes using both an indicator variable (whether total amount paid exceeds the pre-tax costs) and as a continuous measure (percent of total cost). While magnitudes vary across these two measures, the results are consistent.

4.2 Empirical Specification

The empirical strategy employed in Section 3 is a staggered difference-in-difference design, looking at changes between treatment and non-treatment states before-and-after enforcement. The strategy is extended here as the micro-level data allow us to exploit a feature of the post-*Wayfair* legislative environment: changes in enforcement were restricted to out-of-state firms. Bricks-and-mortar retailers have a physical presence in a state and therefore face no change in statutory incidence. This facilitates a triple-difference design, comparing the behavior of online versus bricks-and-mortar consumption within an enforcement state, before and after *Wayfair*.[29] Indeed, using barcode-level data allows us to observe changes in pricing for specific goods when sold across

[27] The median household income in the United States was approximately $67,500 in 2020. Appendix E shows our results are qualitatively robust to focusing on the single highest income bin, with household incomes in excess of $100,000, though the estimates are noisier.

[28] This is perhaps more accurately described as 'Moderate Income'. All results are reweighted to accurately reflect the U.S. population.

[29] Because we do not know the identity of the vendor, we cannot separate online firms that were voluntarily collecting the sales tax prior to *Wayfair*. However, our specification measures newly complying firms because of the change of economic nexus relative to physical nexus. Our results for the *Online* interaction, which includes both firms formerly complying and those induced to comply, are thus likely conservative because we are picking up the average increase in taxes. The denominator is inflated because some firms were already complying.

these different retail channels, after controlling for household-level fixed effects.

The empirical specification is quite saturated. In addition to time fixed effects, state fixed effects, and state-specific linear time trends, we now include 100,000 household and over one million product fixed effects. In general, our specification takes the form:

$$Y_{ihstr} = \alpha_i + \alpha_h + \alpha_s + \alpha_t + X'_{st}\beta_x + \beta_r Online_r + \beta_D D_{st} + \delta(Online_r \times D_{st}) + \epsilon_{ihstr}$$

where Y is the outcome for product i, purchased by household h, in state s, in year-month t, by retailer r. The α's are fixed effects for the corresponding subscripts, X is a set of control variables (namely pre-treatment group-specific linear time trends as discussed above), Online_r is an indicator for whether the retailer is online, mail-order, or otherwise remote, D_{st} is a treatment indicator for enforcement of post-*Wayfair* legislation (economic nexus and/or marketplace facilitator) in state s at time t, and δ is our triple-difference coefficient of interest. It measures the excess effect of treatment on online firms relative to bricks-and-mortar firms.[30] Aggregating up from the product level to the transaction level follows the same specification, but without product-specific terms. We weight all results with household sampling weights that are designed to match the U.S. population, and we two-way clustered at the state and year-month level.

We note that spillovers to bricks-and-mortar retailers are a potential threat to our identification strategy. Our exclusion restriction requires that the difference between online and bricks-and-mortar pricing would be parallel in states absent the statutory incidence-changing legislation. The validity of the counterfactual is not directly testable, but to investigate this, in Appendix D we analyze consumption behavior in bricks-and-mortar stores and find no significant evidence of spillovers. Table D1 shows there is no detectable increase in the quantity of items purchased in bricks-and-mortar stores.[31] Table D2 shows a comparable table for online stores, finding households do purchase fewer quantities online after treatment. We supplement the quantities-based regressions with analysis of monthly expenditures. The decline in quantities purchased online appear to be offset by an increase in taxes: Table D3 shows that households' monthly online spending

[30] An alternative specification, outlined in Pischke (2015), further saturates the specification by including additional fixed effects and interaction terms but does not separately estimate Online or Enforce $\times$ Post effects. These results are reported in Appendix G. We thank Keith Teltser for this suggestion.

[31] Appendix Tables D1 and D2 are Poisson FE regressions, with coefficients expressed as incidence rate ratios, and so the relevant baseline is $\beta = 1$ rather than $\beta = 0$.

is approximately unaffected by treatment. Table D4 shows a very modest (0.1 percent) overall decrease in dollars spent in offline stores, but of differing signs by household income, and without any statistical significance for either group, evidence against general equilibrium spillovers.

4.3 Results

Table 6 shows the results of the triple-difference specification for the key outcomes of pre-tax unit prices (in logs), an indicator variable for whether a transaction triggers any tax liability, and the amount of tax paid in a transaction as a percentage of the total price. The first row of Table 6 shows the effect of buying from an online retailer. Relative to bricks-and-mortar stores, online stores have higher prices and have lower tax amounts paid on purchases. The second and third row shows the effect of the economic nexus and marketplace facilitator treatments on bricks-and-mortar stores. Corroborating the evidence presented in Appendix Tables D1 and D2, we see no significant effects of treatment on 'untreated' bricks-and-mortar stores.

Table 6: Effects of nexus and marketplace facilitator treatments on key outcomes

	ln(Unit Price)		Tax Indicator		Percent Taxes	
	(1)	(2)	(3)	(4)	(5)	(6)
Online	10.18***	10.18***	-9.35***	-9.40***	-9.56***	-9.59***
	(0.59)	(0.59)	(0.99)	(0.99)	(0.97)	(0.97)
Nexus Treatment	0.05	-0.03	-0.08	0.03	0.13	0.20
	(0.12)	(0.14)	(0.20)	(0.20)	(0.21)	(0.23)
Marketplace Treatment		0.24		-0.31		-0.18
		(0.19)		(0.19)		(0.23)
Nexus × Online	0.11	0.27	5.08***	2.78***	3.30***	1.86**
	(0.39)	(0.54)	(0.83)	(0.66)	(0.70)	(0.76)
Marketplace × Online		-0.35		5.04***		3.14***
		(0.54)		(0.85)		(0.79)
Unit of observation	Unit-transaction	Unit-transaction	Transaction	Transaction	Transaction	Transaction
Level of FE	UPC	UPC	Household	Household	Household	Household
N	268,524,485	268,524,485	26,850,238	26,850,238	26,850,238	26,850,238

Table shows the effects of economic nexus and remote seller tax enforcement on the log of unit prices, an any tax indicator variable, and the tax as a percent of overall price. Coefficients are multiplied by 100 for interpretation. All regressions include household fixed effects, year-month fixed effects, and state-specific (pre-treatment) linear time trends. Standard errors are two-way clustered at the state and year-month level.

The fourth and fifth rows, those near the bottom of the table with the treatment names in italics, show the triple-difference coefficients of interest. They represent the effect of economic nexus

and/or marketplace facilitation legislation on online purchases in treated states, over and above offline purchases in treated states. Our first result is that there is essentially no evidence of firms reducing their prices in response to the treatment. This is consistent with full pass-through of the tax onto the consumer. If anything, a plurality of the point estimates in Columns 1 and 2 suggests excess pass-through of the tax, with pre-tax prices increasing for products bought online. Although excess pass-through has been observed in other studies (Besley and Rosen, 1999; Poterba, 1996; Ross and Lozano-Rojas, 2018; Nelson and Moran, 2019), the unit price results in Table 6 are not significantly different from zero. Our robustness analysis in Appendix F shows the excess pass-through result is largely driven by very low-priced products. Moderate restrictions on the sample, e.g. to goods that cost at least $10, reduces this point estimate to –0.96 percent.

However, we do see significant effects in the tax outcomes. Let us start with the Tax Indicator variable, a binary measure of whether the consumer paid any tax. This indicator ignores variation in the amount of taxes paid, but is robust to potential behavioral responses like purchasing fewer items per trip. With a binary outcome we can interpret the coefficients as probabilities. In isolation, Column 3 suggests that economic nexus legislation increases the probability that an online transaction incurs any taxation by 5 percentage points. When accounting for subsequent enforcement on marketplace facilitators, the effect of nexus alone decline to 2.78 percentage points, and we see the combined effect of these treatments sum to $2.78 + 5.08 = 7.82$ percentage points. This goes a considerable way to eliminating the 'internet tax haven' effect indicated by the first row where online transactions are about 9.4 percentage points less likely to trigger taxes than similar purchases offline.

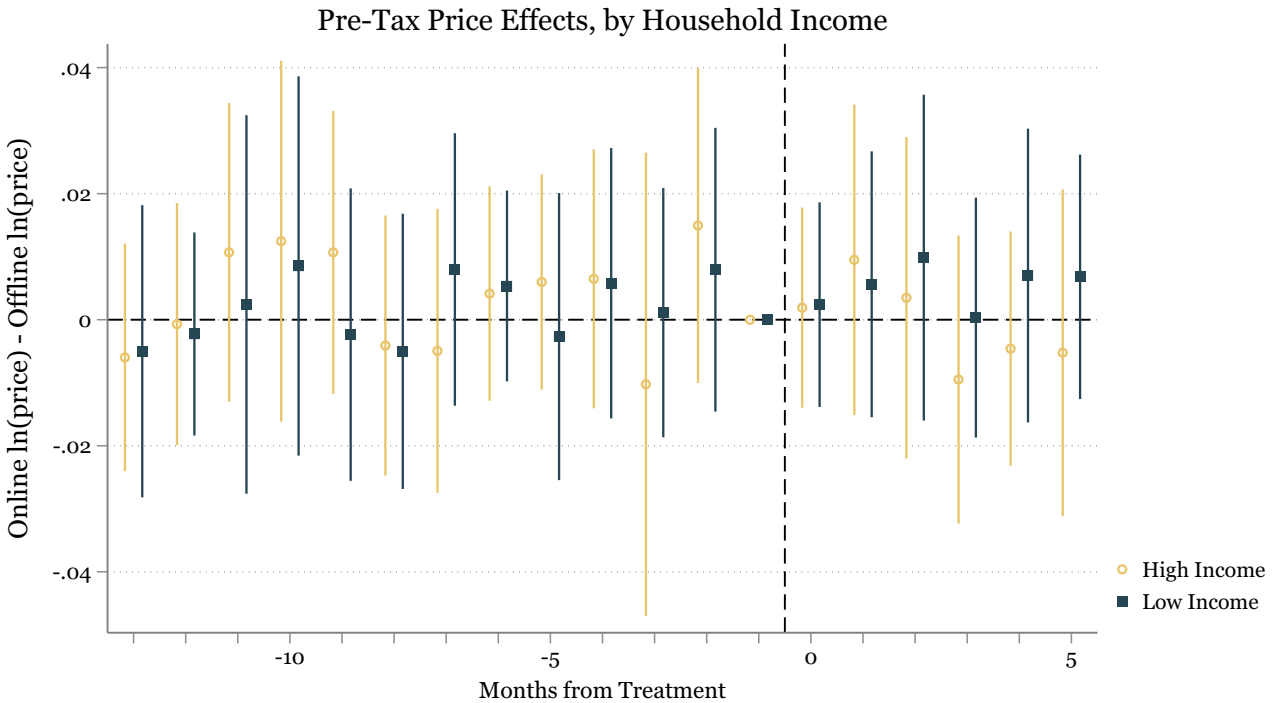
The third outcome variable is an intensive margin effect of taxes, namely a Percent Taxes variable. This follows the same qualitative pattern as the tax indicator variable, albeit with point estimates attenuated. The linear combination of the treatments in the final column of the table indicates that consumers paid 5 percent more in taxes for online purchases after treatment. This is one of the main results of our paper.

We consider the results in Table 6 to be quite important. They provide substantial support for the macro-level results that found increased tax revenues. Table 6 supplements this to suggest the tax was accompanied by full pass-through to consumers, with very little evidence of changes in pre-tax prices. The findings on tax effects in Table 6 are of a similar magnitude (but smaller, 5.0

percent versus 7.9 percent) to those found in macro-data. One potential avenue for the difference is the tax increase on business-to-business transactions, which will increase tax revenues but would not be captured in the household-focused micro data.

While it cannot speak to business-to-business sales, the household-focused data do permit analysis of heterogeneous effects by household income. As Table 6 provides evidence of full pass-through of the tax to consumers, any differences actual in tax liabilities are of particular interest from an equity point of view. We start this heterogeneity analysis with the event study Figure 3.

Figure 3: Event study analysis of the economic nexus treatment



The figure presents results from two separate event study designs for the economic nexus treatment. The outcome on the vertical axis represents increases in pre-tax prices for online purchases relative to bricks-and-mortar prices, after partialing out UPC fixed effects. We see the results for high-income and low-income households follow very similar trends in the pre-period. Furthermore, the principal finding from this figure, consistent with Table 6 is that price effects are small overall. A keen eye might note that the estimates for low-income households tend to depart from the results for high-income households in the post-treatment period, namely that post-treatment

effects are uniformly positive for low-income households but mixed for high-income households. These results are more formally depicted in Table 7.

The analysis in Table 7 confirms the event study, as it shows that nexus' 0.27 percent price increase in Table 6 is the weighted average of differential effects across households by income. High-income households experience a small (but insignificant) price decrease (-0.59%) and low-income households have a large, significant price increase (1.68%). The -2.23 coefficient in Column 3 of Table 7 is the interaction effect, confirming the difference between high-income and low-income households is statistically significant. Moving from low- to high-income reduces the effect on pre-tax prices. This is consistent with low-income households seeking sellers who do not collect the tax, and being willing to pay a higher retailer price to do so. If this represents shifting to higher profitability firms, this is a transfer. If this is a shift to higher cost firms, then this represents a welfare loss.

The evidence on heterogenous effects is weaker for the marketplace facilitator treatments. Further, the results lean in the opposite direction to those from nexus: higher income households see a small price increase (0.24%) and low income households see a weakly significant price decrease. Thus, in aggregate, there is insufficient evidence to make any particularly strong claims about heterogeneity in pass-through effects. To a first approximation, taxes are fully passed-through to consumers, with perhaps some evidence of excess pass-through to low-income consumers.

The pattern of the pooled sample masking household-level heterogeneity is more pronounced with the tax variables. Table 8 shows the results for the tax indicator variable. Pre-treatment, the probability of an online transaction incurring any taxation is lower for both high- (9.33 percentage points) and low-income (9.44 percentage points) households.[32] Table 6 revealed that the probability increased by 7.82 percentage points after the nexus and marketplace treatments, largely eliminating the online tax haven loophole. However Table 8 is suggestive of a progressive pattern in the distribution of these probabilities. High-income households see an increase of 8.7 percent and low-income households see an increase of 6.8 percent. Both numbers are positive, but the increase for high-income households was $(8.7 - 6.8)/6.8 = 28\%$ greater than that for low-income households. This evidence is suggestive, however, as the test of the differences in treatment magnitudes for high- and low-income (defined here as the sum of the last two interaction effects in

[32]For reference, about two-thirds of transactions in the data have positive tax liability.

Table 7: Heterogenous effects on log of listed price by consumer type

	(1) High Income	(2) Low Income	(3) Pooled
Online	10.02***	10.67***	10.02***
	(0.72)	(0.56)	(0.53)
Highest Income HH			0.09
			(0.05)
Nexus Treatment	-0.13	0.06	-0.09
	(0.13)	(0.14)	(0.14)
Marketplace Treatment	0.31*	0.17	0.15
	(0.17)	(0.21)	(0.20)
Nexus × Online	-0.59	1.68***	1.43**
	(0.63)	(0.63)	(0.57)
Marketplace × Online	0.24	-1.27*	-1.19*
	(0.78)	(0.73)	(0.67)
Online × High			0.33
			(0.50)
Nexus × High			0.13**
			(0.06)
Marketplace × High			0.16**
			(0.07)
Nexus × Online × High			-2.23***
			(0.68)
Marketplace × Online × High			1.59
			(1.08)
Joint Effect	-0.35	0.41	-0.63
S.E.	0.69	0.62	1.08
Unit of observation	Unit-transaction	Unit-transaction	Unit-transaction
Level of FE	UPC	UPC	UPC
Group	High Income	Low Income	Interaction
N	118,997,439	149,255,483	268,524,485

Table shows the effects of economic nexus and remote seller tax enforcement on pre-tax prices, by consumer income band, for goods that cost at least $1. The joint effect row refers to the sum of the final two coefficients in each column. The highest income group is households with incomes in excess of $70,000. Coefficients are multiplied by 100 for interpretation. All regressions include household fixed effects, year-month fixed effects, and state-specific (pre-treatment) linear time trends. Standard errors are two-way clustered at the state and year-month level.

Table 8: Heterogenous effects on tax indicator by consumer type

	(1) High Income	(2) Low Income	(3) Pooled
Online	-9.33***	-9.44***	-9.45***
	(1.08)	(1.05)	(1.05)
Highest Income HH			0.42*
			(0.21)
Nexus Treatment	0.09	0.02	0.13
	(0.25)	(0.22)	(0.26)
Marketplace Treatment	-0.43**	-0.19	-0.13
	(0.21)	(0.25)	(0.24)
Nexus × Online	4.21***	1.17	1.13
	(1.12)	(0.86)	(0.85)
Marketplace × Online	4.49***	5.62***	5.66***
	(1.19)	(1.33)	(1.32)
Online × High			0.11
			(0.78)
Nexus × High			-0.20
			(0.24)
Marketplace × High			-0.36*
			(0.21)
Nexus × Online × High			3.24**
			(1.50)
Marketplace × Online × High			-1.32
			(1.87)
Joint Effect	8.70***	6.80***	1.92
S.E.	0.91	1.20	1.40
Unit of observation	Transaction	Transaction	Transaction
Level of FE	Household	Household	Household
Group	High Income	Low Income	Interaction
N	11,263,962	15,586,276	26,850,238

Table shows the effects of economic nexus and remote seller tax enforcement on an any-tax indicator, by consumer income band. The joint effect row refers to the sum of the final two coefficients in each column. The highest income group is households with incomes in excess of $70,000. Coefficients are multiplied by 100 for interpretation. All regressions include household fixed effects, year-month fixed effects, and state-specific (pre-treatment) linear time trends. Standard errors are two-way clustered at the state and year-month level.

Table 9: Heterogenous effects on percent tax by consumer type

	(1) High Income	(2) Low Income	(3) Pooled
Online	-10.44***	-8.83***	-8.83***
	(1.11)	(1.06)	(1.06)
Highest Income HH			0.59**
			(0.25)
Nexus Treatment	0.15	0.29	0.32
	(0.25)	(0.26)	(0.30)
Marketplace Treatment	-0.31	-0.13	-0.05
	(0.20)	(0.31)	(0.28)
Nexus × Online	3.59***	0.07	0.07
	(1.28)	(0.72)	(0.71)
Marketplace × Online	2.70**	3.76***	3.77***
	(1.07)	(1.14)	(1.13)
Online × High			-1.61*
			(0.90)
Nexus × High			-0.25
			(0.24)
Marketplace × High			-0.26
			(0.19)
Nexus × Online × High			3.65**
			(1.42)
Marketplace × Online × High			-1.26
			(1.57)
Joint Effect	6.29***	3.84***	2.39**
S.E.	0.82	0.88	1.12
Unit of observation	Transaction	Transaction	Transaction
Level of FE	Household	Household	Household
Group	High Income	Low Income	Interaction
N	11,263,962	15,586,276	26,850,238

Table shows the effects of economic nexus and remote seller tax enforcement on tax as a percent of overall price, by consumer income band. The joint effect row refers to the sum of the final two coefficients in each column. The highest income group is households with incomes in excess of $70,000. Coefficients are multiplied by 100 for interpretation. All regressions include household fixed effects, year-month fixed effects, and state-specific (pre-treatment) linear time trends. Standard errors are two-way clustered at the state and year-month level.

Column 3) is not statistically distinguishable from zero at any conventional level.

These heterogenous effects are statistically different in Table 9 when we use the percent tax variable. The results in this table follow an identical pattern as in Table 8, but the greater variation permitted by the continuity of the outcome variable allows for more precise inference. Table 9 shows both household types pay lower taxes per transaction online pre-treatment, and that the joint effect of treatment is either 6.29 percent (high-income) or 3.84 percent (low-income). The implication is strongly progressive, with high-income households experiencing a $(6.29 - 3.84)/3.84 = 64\%$ greater increase in taxation than low-income households. The joint effect in Column 3 confirms the statistical significance of the difference in treatment intensity between high- and low-income when the sample is pooled.

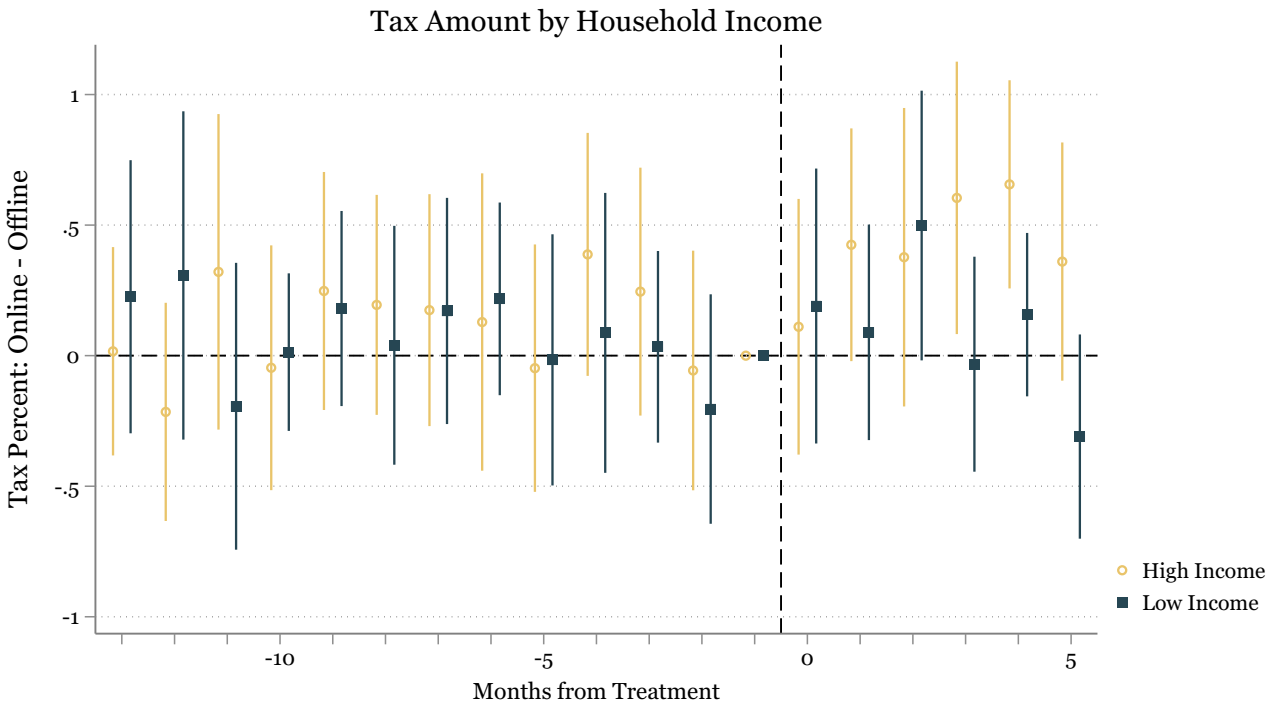
On the other hand, we find that high-income households faced lower prices (Table 7), suggesting that sellers paid some of the additional tax through lower pre-tax prices and evidencing that the entire tax was not forward shifted to buyers. Low-income households see a much smaller and statistically insignificant 3.8 percent tax increase, but they did not see the price reduction experienced by high-income buyers. This suggests that low-income households are likely to identify sellers that do not collect the tax[33] but which also offer prices that are higher than those offered by firms that collected the tax.[34] Shifting purchases to non-compliant vendors (and not remitting the tax) effectively continues the tax evasion that was already occurring. In this sense, non-compliance with the tax made the sales tax less regressive, but some of this effect is perhaps offset by low-income households experiencing higher prices. The joint effect in Column 3 of Table 9 (2.39 percent) is significant both statistically and substantively, again confirming high-income households bear a disproportionately larger tax increase than low-income households after treatment.

Figure 4 depicts the event study of treatment on tax liabilities. The trends for high- and low-income appear broadly parallel in the pre-period. There is some evidence of nonzero gains (more positive than negatives), albeit insignificant, in the variable pre-treatment. This supports the anec-

[33]No evasion occurs if the switch to noncompliant retailers is accompanied by a use tax declaration, which we think is unlikely because the change suggests the buyers were not making use tax payments before *Wayfair*.

[34]Low-income households may avoid a greater propensity to pay sales tax through a composition effect (i.e. choosing to buy more tax-free products) or substitution towards untaxed alternatives (such as purchasing from vendors not remitting the sales tax). We investigate the composition hypothesis in Appendix H, where we restrict the sample to goods and transactions that are typically taxable. Results persist in this subsample, suggesting differences may be driven by substitution rather than composition.

Figure 4: Event study analysis of tax liability



dotal evidence claiming some online firms implemented post-*Wayfair* tax compliance changes all at once rather than timing them in response to the state-level deadlines. To the extent that online firms are responding to the treatment early and implementing tax compliance regimes in the months ahead of actual enforcement, we will miss these effects and this will bias our point estimate towards zero. While we cannot quantitatively evaluate the magnitude of any downward bias without strong assumptions about its nature, the evidence from the macro-level effects of Section 3 may be instructive: the 'never versus treated' effect from Goodman-Bacon decompositions suggest the increases in taxation may be in the range of 12 to 15 percent. Our estimated effects in both micro and macro data are more modest; in the range of 5 to 8 percent. We leave investigating the effects of pre-treatment compliance to future research.

Given our results that lower-income consumers experience relatively lower treatment effects than higher-income consumers, potential shifting to higher- or lower-quality goods is an important avenue to investigate. Sales taxes may affect the choice of consumers in at least two ways. First, policymakers generally exempt necessities from the tax base and therefore introducing a

sales tax may shift consumption towards basic goods. We can consider this a substitution effect. Second, income effects may also affect the choices of consumers. While scaling up all prices by some constant τ will not create any substitution effects, income effects may induce consumers to switch to lower-quality goods rather than to adjust quantities.

To analyze if the tax induces consumers to switch to lower-quality goods, we focus on categories where the tax is most likely to be applicable.[35] Harding et al. (2012) investigated a similar question in the context of taxes on cigarettes. They use a UPC-level measure of average price as a proxy for quality, and estimate the effect of an increase in excise taxes on this quality index. They conclude that increasing the excise tax, which on a percent basis disproportionately increases the price of lower-cost items, caused a shift towards higher-quality goods.

We adapt this technique[36] and use product sub-category fixed effects to generalize it to the multi-good case: rather than just cigarettes, there are approximately fifty product sub-categories (e.g. skin care products, laundry detergents, candles) within our broader definition of taxable goods. We create the good-specific price to proxy for quality, and then regress this on enforcement to test for evidence of switching. We include fixed effects for each of these sub-categories, and thus estimate coefficients from within-category differences in price. Negative coefficients indicate a reduction in average price of goods, measured in dollars, within a product sub-category. Table 10 shows the results.

We find very limited evidence of consumers switching to lower price goods in response to treatment. Even though we focus our attention on goods most likely affected by the tax, results are generally insignificant. In the bottom row, we see treatment is associated with a 1.2 cents ($\pm$3.9 cents) reduction in the quality index of goods in the pooled sample. The average unit price across all the included products is around \$9, and so this change is substantively small as well as statistically insignificant. In the pooled samples, the estimated coefficients range from $+1.7$ cents (base \$15) for Non-Food Grocery to -5.2 cents (base \$8) in Health and Beauty Aids. Considering the results in full, there is limited evidence of substantial consumer switching in either quality direction.

Table 10 also includes results across households of different incomes. There is some evidence

[35] These categories are General Merchandise, Health and Beauty products, and Non-Food Grocery, the same definition of taxable goods used in Appendix H.

[36] We thank Mike Lovenheim for useful discussions in implementing this procedure.

Table 10: Product quality index regression results

Department	High Income	Low Income	Pooled
Health and Beauty Aids	-0.046	-0.053	-0.052*
	(0.038)	(0.035)	(0.026)
	N = 11,234,758	N =7,955,583	N =19,190,341
Non-Food Grocery	0.004	0.033	0.017
	(0.046)	(0.039)	(0.035)
	N =12,253,629	N =7,972,893	N =20,226,522
General Merchandise	0.002	0.007	0.000
	(0.053)	(0.073)	(0.056)
	N =8,658,883	N =6,070,815	N =14,729,698
All taxable goods	-0.014	-0.004	-0.012
	(0.025)	(0.026)	(0.019)
	N =32,147,270	N =21,999,291	N =54,146,561

Table shows the results of twelve separate regressions. The outcome variable is a UPC-level measure of within product-category quality. Point estimates, standard errors, and sample sizes are shown. A negative coefficient indicates nexus enforcement shifted consumer choices towards lower-quality goods.

of lower-income consumers switching to lower quality Health and Beauty products (base $7.73) but the decline of 5.3 cents is again quite small on a percentage basis and statistically insignificant. This is consistent with limited substitution effects in the presence of proportional increases in prices. Overall there is modest reason to believe income effects are switching consumer choices to lower quality goods, perhaps slight evidence of that in Health and Beauty products, but no particularly strong claims can be made.

5 Conclusion

While standard public finance theory predicts the irrelevance of statutory incidence, prior empirical literature has questioned this finding (Chetty, Looney and Kroft, 2009; Goldin and Homonoff, 2013; Bradley and Feldman, 2020). Existing empirical evidence suggests vendors evade less than buyers, particularly when higher up in the supply chain (Bibler et al., 2018; Kopczuk et al., 2016). We extend this empirical literature, providing evidence that statutory incidence can affect both individual behavior and state tax collections, and moving statutory incidence earlier in the distribution process results in better compliance, higher tax revenues, and less tax evasion.

The 2018 *Wayfair* Supreme Court decision combined with state legislative changes granted tax authorities additional enforcement powers. Prior to *Wayfair*, states could not enforce sales tax compliance on many of the rapidly increasing e-commerce retailers. Rather, compliance depended on consumers 'signing the check' and personally remitting the tax to the revenue authority. *Wayfair* profoundly changed this, declaring that a shift in incidence to out-of-state retailers is constitutional and allowing states new powers to enforce tax compliance on firms not physically located inside their borders. This precipitated one of the largest shifts in statutory incidence in U.S. history. We exploit the rollout of these changes in enforcement to investigate the relevance of statutory incidence.

Using a novel panel of monthly sales tax revenues, we show that shifting incidence and enforcing compliance on remote vendors increased revenues by 7.9 percent on average. These revenue gains are driven by revenue from so-called marketplace facilitators: more than three-quarters of the effect is from websites like eBay or Etsy who provide platforms for small retailers rather than sell directly. These revenue gains could — if states wished — be used to fund tax rate reductions. With sales taxes exceeding 9 percent in many jurisdictions, back of the envelope calculations suggest that states could reduce rates by as much as a percentage point and remain revenue-neutral.

For a more nuanced understanding of the macro-level results, we use the Nielsen consumer panel scanner data to analyze effects at the individual level. We find limited evidence of reduced pre-tax prices, implying that increased tax liabilities are fully passed through to consumers, at least in the short run. Prior research finds the sales tax is forward shifted to consumers, but our work suggests incidence is heterogeneous and nuanced, at least with respect to income (Poterba, 1996; Ross and Lozano-Rojas, 2018; Nelson and Moran, 2019). Enforcement increased online tax liabilities by about 5 percent, but this increase was concentrated in high-income households: the tax is borne by higher income taxpayers but less so by lower income consumers who often find the means to avoid paying more tax. Specifically the tax increase point estimates are about two-thirds higher for households with incomes in excess of $70,000 (6.3 percent increase) than for households with incomes below (3.8 percent increase). This may be caused by compositional differences in the baskets of goods consumed by these households, by a greater tendency of lower-income households to shift to non-compliant retailers, or a combination of both. The net effect is the increased tax revenues appear to have been raised progressively. On the other hand, lower-

income households may experience higher pre-tax prices, perhaps because they seek out sellers without a compliance responsibility. This is an avenue for future research. To the extent that consumers are switching to higher-cost producers who do not collect the tax, this increase in prices represents a 'quasi-tax' for lower income consumers that reduces welfare.

A series of policy implications follow. First, the progressivity or regressivity of a tax must be evaluated against actual consumption behavior and not theoretical taxes. Second, shifting the point of collection to retailers increases revenues, and the degree of increase rises with more stringent enforcement criteria. Our results are consistent with reduced evasion by centralization of the compliance responsibility. Expecting the gig economy to grow in the coming years, consider a tax on ride-sharing services like Uber. Our results suggest that collection responsibility is best placed at the level of the platform (Uber) rather than the service provider (the driver), and should certainly not be placed on the final consumer. This offers a clear message for state tax authorities. Internationally, countries should consider robust nexus and/or marketplace facilitator legislation as an untapped source of revenue growth, and U.S. states that opted for high per capita compliance thresholds should consider more stringent enforcement. Third, shifting statutory incidence can improve compliance, reduce evasion, increase tax revenues, and do so in an economically progressive manner. These advantages are likely more pronounced if online shopping has increased because of the COVID-19 pandemic.

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A Additional State-Level Tables

Table A1: Dates of economic nexus enforcement at the state-level

State	Effective Date
NY	June 2018
HI, ME, VT	July 2018
MS	September 2018
AL, IL, IN, KY, MD, MI, MN, ND, NV, WA, WI	October 2018
NC, NJ, SC, SD	November 2018
CT	December 2018
DC, GA, IA, UT, WV	January 2019
WY	February 2019
CA, NE	April 2019
CO	May 2019
ID	June 2019
AR, NM, PA, RI, VA	July 2019
OH	August 2019
AZ, KS, MA, TN, TX	October 2019
OK	November 2019

Table A2: Dates of marketplace facilitator enforcement

State	**Effective Date**
MN, WA	October 2018
NJ	November 2018
CT	December 2018
AL, IA	January 2019
SD	March 2019
DC, NE	April 2019
ID, NY, VT	June 2019
AR, IN, KY, NM, PA, RI, VA, WV, WY	July 2019
OH	September 2019
AZ, CA, CO, MA, MD, ME, ND, NV, TX, UT	October 2019
OK	November 2019
HI, IL, MI, WI	January 2020
NC	February 2020

Table A3: The determinants of if and when post-*Wayfair* enforcement commenced

	Ever Enacted		***Date Enacted***	
	(1)	(2)	(3)	(4)
log(Revenue)	-0.036	-0.11	2.16	-0.49
	(0.094)	(0.11)	(1.72)	(1.48)
Population (millions)	-0.0042	0.0067	0.054	0.10
	(0.0058)	(0.0094)	(0.14)	(0.13)
Sales tax rate	0.054	0.054	0.31	-0.45
	(0.037)	(0.061)	(0.87)	(0.88)
Membership in SSTP	0.11	0.049	-1.09	-1.38
	(0.066)	(0.12)	(1.77)	(2.06)
Pre-*Wayfair* nexus measure	0.0014	-0.00011	-0.061	0.0030
	(0.0025)	(0.0029)	(0.051)	(0.046)
Treatment	Nexus	Marketplace	Nexus	Marketplace
Adjusted R^2	0.016	-0.052	-0.035	-0.085
Obs	45	45	42	39

Table shows results from regressions on whether a state ever enacts legislation in our data and also results from a continuous measure of enactment: months since the SCOTUS case.

Table A4: Balance table for economic nexus legislation

Variable	(1) Control	(2) Treatment	(3) Differences
log(Revenue)	20	19.6	-.439
	(1.11)	(.888)	(.658)
Sales tax rate	4.86	5.7	.845*
	(.847)	(1.04)	(.498)
Population (millions)	10.4	6.59	-3.86
	(7.22)	(7.28)	(4.35)
Membership in SSTP	0	.548	.548***
	(0)	(.498)	(.0777)
Pre-treatment compliance level	193	190	-3.13
	(46)	(61.7)	(12.5)
Observations	261	3,741	4,002

Table A5: Balance table for marketplace facilitator legislation

Variable	(1) Control	(2) Treatment	(3) Differences
log(Revenue)	19.9	19.5	-.32
	(.816)	(.919)	(.34)
Sales tax rate	5.56	5.67	.1
	(1.25)	(1.01)	(.499)
Population (millions)	7.74	6.68	-1.07
	(5.76)	(7.58)	(2.52)
Membership in SSTP	.286	.553	.267
	(.452)	(.497)	(.191)
Pre-treatment compliance level	191	190	-.576
	(50.2)	(62.5)	(8.68)
Observations	609	3,393	4,002

B Additional Figures

Figure B1: Event study graph of economic nexus treatment

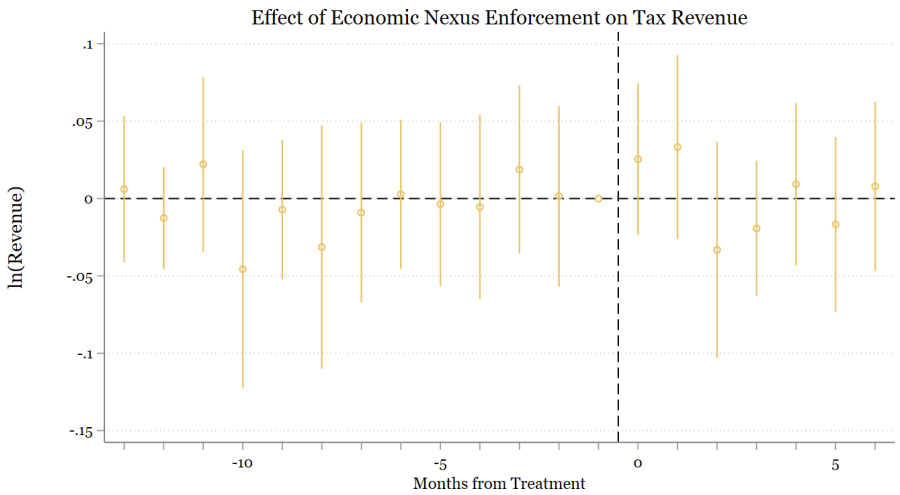


Figure B2: Event study graph of marketplace facilitator treatment

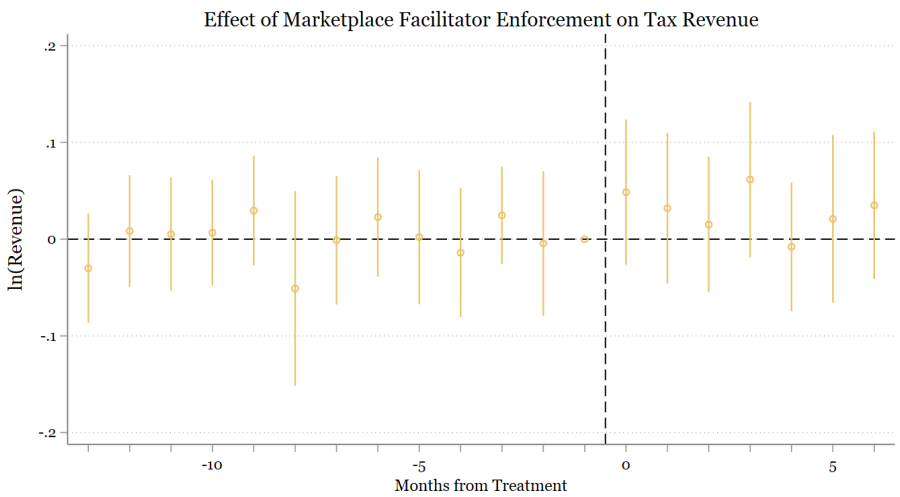


Figure B3: Distribution of point estimates across observation pairs

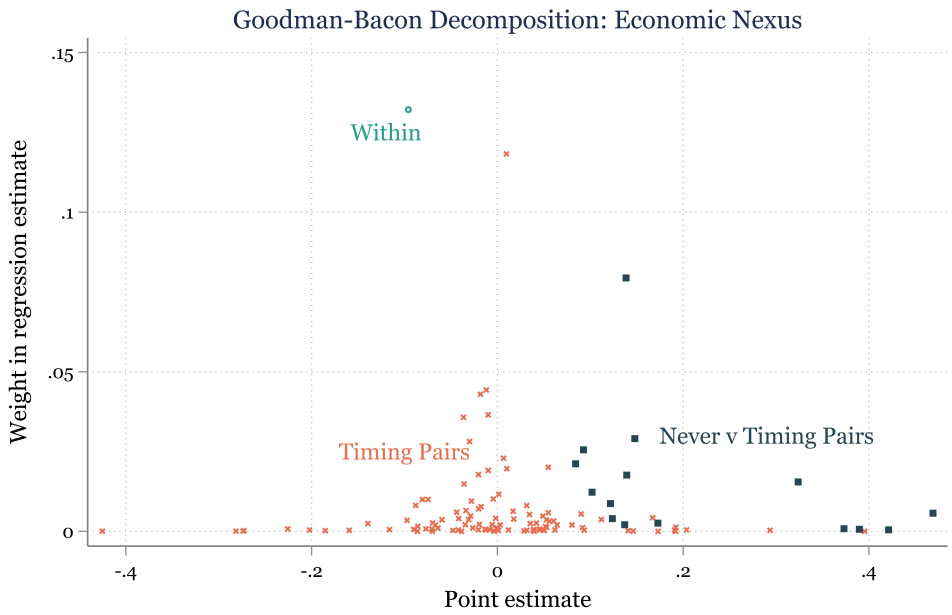


Figure depicts the distribution of estimates from the Goodman-Bacon decomposition, by comparison (or cohort) group. Each observation represents a comparison of states which received treatment in a given month with other states not treated in that month. The Never v Timing Pairs, which are a theoretically clean estimand, are strongly positive.

Figure B4: Distribution of point estimates across observation pairs

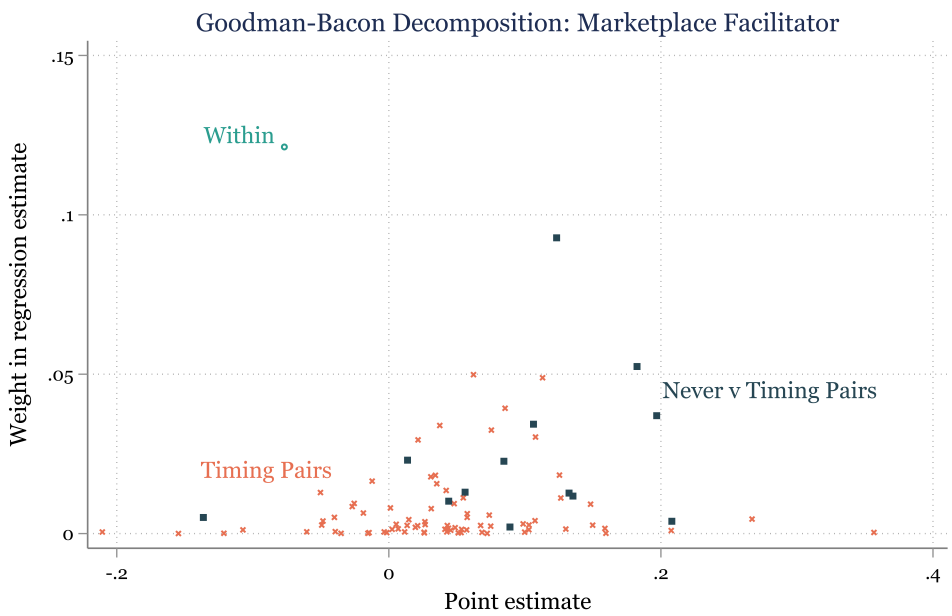


Figure depicts the distribution of estimates from the Goodman-Bacon decomposition, by comparison (or cohort) group.

Figure B5: Event study analysis of the tax indicator

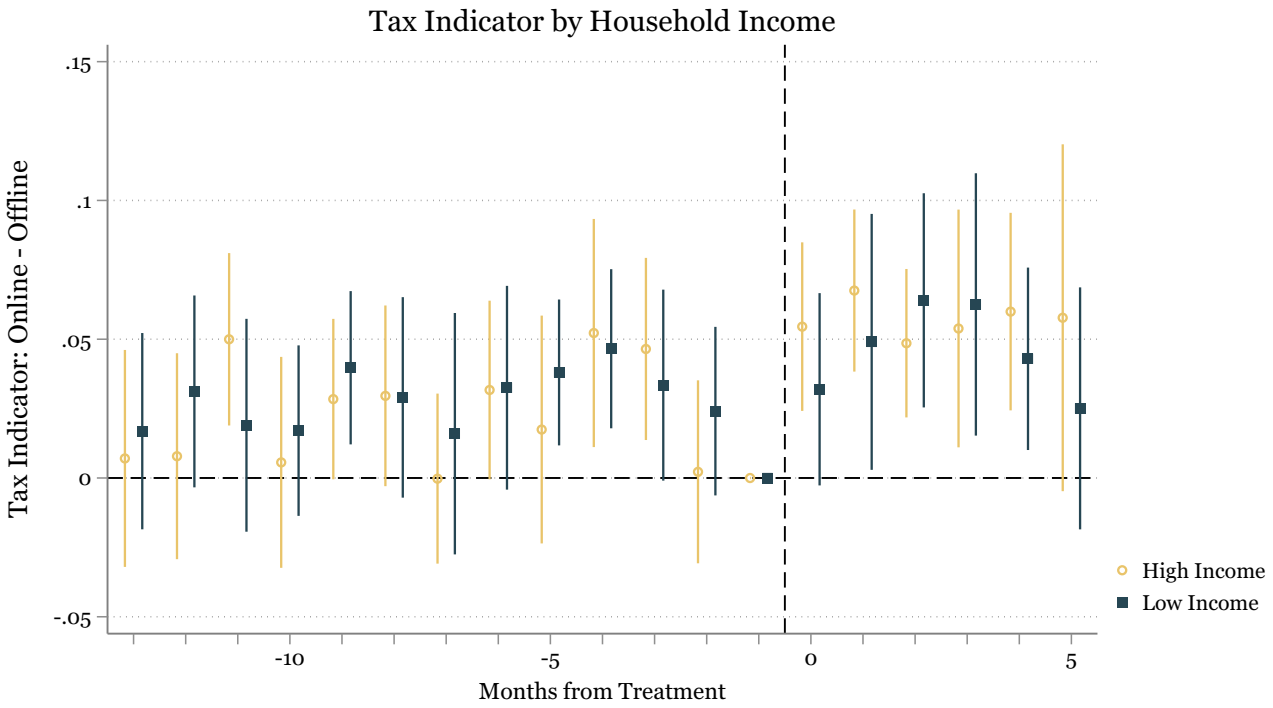
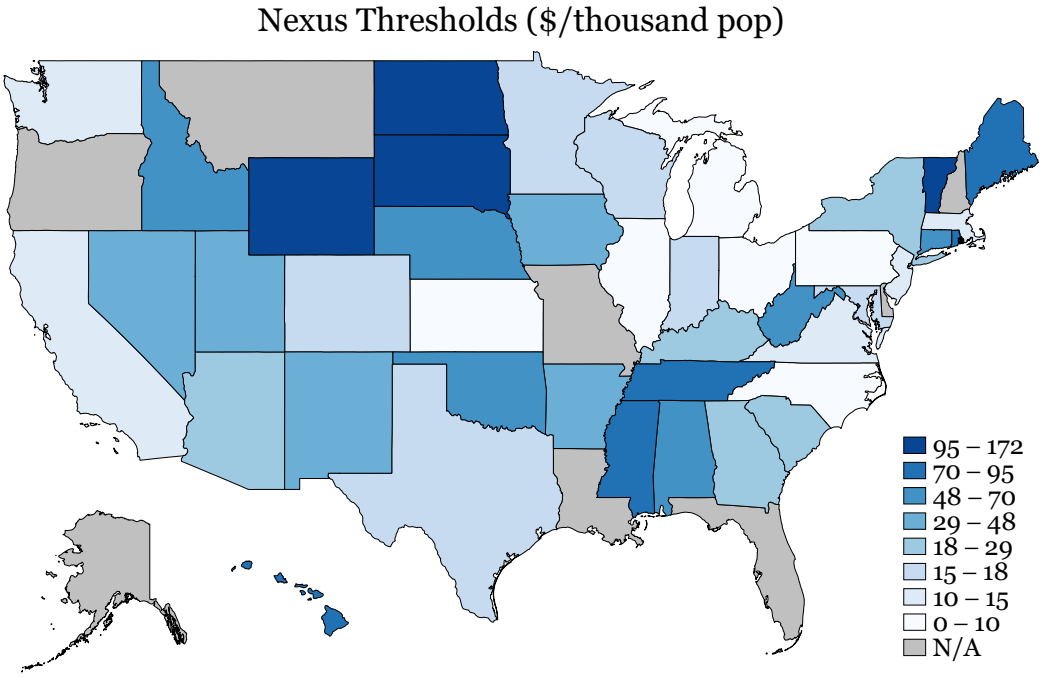


Figure B6: Figure shows each state's nexus threshold divided by the state's population (in thousands). A revenue threshold of $100,000 is less likely to be satisfied in SD than in IL.



C de Chaisemartin and d'Haultfoeuille Estimates

de Chaisemartin and d'Haultfoeuille (2020) introduce a new estimator for staggered difference-in-difference designs like ours, and applying their estimator to our setting finds treatment effects that envelop our principal 1.78% economic nexus and 6.17% marketplace facilitator estimates. Similar to the Goodman-Bacon decomposition approach, de Chaisemartin and d'Haultfoeuille emphasize that two-way fixed effect designs identify weighted sums of average treatment effects, but that the attributable weights can be negative. The approach can be implemented on a fixed effect (FE) or first-difference (FD) basis.

For economic nexus, using the FE-basis estimates a treatment effect of 1.5%, although the SE of 2.7% is wide. Decomposing the weights that constitute the point estimate shows 415 of the potential treatment pairs are positive. While a reasonably large number (150) of the weights are negative, their sum of -0.07 indicates that negative weights are perhaps not a major concern. The FD-basis indicates a substantially larger point estimate of 4.4% (again insignificant, se=5.7%) but a much larger number (291) of the weights are negative, and substantially large (summing to -0.51). Similarly, the FE estimate on marketplace facilitator legislation generates a small amount of negative weights (actually zero) with a point estimate of 4.1% (se=3.0%), whereas the FD approach gives a substantially higher estimate of 11.6% with a standard error of 7.3%.

D Difference-in-difference estimates at the household-month level

Table D1: Effect of economic nexus and marketplace facilitator laws on offline purchases

	Offline Quantity		
	(1)	(2)	(3)
Offline Items			
Ever enforce × Post	0.997	0.996	0.997
	(0.00)	(0.00)	(0.00)
Unit of observation	Household-Month	Household-Month	Household-Month
Household FE	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes
Group	Pooled	High Income	Low Income
N	3,370,252	1,438,263	1,931,977

Table shows FE Poisson estimates of treatment on the number of offline items purchased by month, by consumer income band. The highest income group is households with incomes in excess of $70,000. Coefficients are reported as incidence rate ratios for interpretation.

Table D2: Effect of economic nexus and marketplace facilitator on online purchases

	Online Quantity		
	(1)	(2)	(3)
Online Items			
Ever enforce × Post	0.908***	0.909*	0.928*
	(0.03)	(0.05)	(0.04)
Unit of observation	Household-Month	Household-Month	Household-Month
Household FE	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes
Group	Pooled	High Income	Low Income
N	1,918,205	808,886	1,052,277

Table shows FE Poisson estimates of treatment on the number of items purchased by month, by consumer income band. The highest income group is households with incomes in excess of $70,000. Coefficients are reported as incidence rate ratios for interpretation.

Table D3: Effect of economic nexus and marketplace facilitator laws on online purchases

	ln(Online Spending)		
	(1)	(2)	(3)
Ever enforce × *Post*	0.131	0.004	0.349
	(0.652)	(0.887)	(0.953)
Unit of observation	Household-Month	Household-Month	Household-Month
Household FE	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes
Group	Pooled	High Income	Low Income
N	3,370,337	1,438,315	1,932,022

Table shows effect of treatment on log monthly online expenditure, by consumer income band. The highest income group is households with incomes in excess of $70,000. Coefficients are reported as Incidence Rate Ratios for interpretation.

Table D4: Effect of treatment on offline purchases

	ln(Offline Spending)		
	(1)	(2)	(3)
Ever enforce × *Post*	-0.11	-0.21	0.28
	(0.86)	(1.25)	(0.69)
Unit of observation	Household-Month	Household-Month	Household-Month
Household FE	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes
Group	Pooled	High Income	Low Income
N	3,370,337	1,438,315	1,932,022

Table shows effect of treatment on log monthly online expenditure, by consumer income band. The highest income group is households with incomes in excess of $70,000. Coefficients are multiplied by 100 for interpretation.

E Effects for high income households

Table E1: Effects of nexus and marketplace facilitator treatments on key outcomes

	ln(Unit Price)	Tax Indicator	Percent Taxes
	(1)	(2)	(3)
Online	10.18***	-9.40***	-9.59***
	(0.59)	(0.99)	(0.97)
Nexus Treatment	-0.03	0.03	0.20
	(0.14)	(0.20)	(0.23)
Marketplace Treatment	0.24	-0.31	-0.18
	(0.19)	(0.19)	(0.23)
Nexus Treatment × Online	0.27	2.78***	1.86**
	(0.54)	(0.66)	(0.76)
Marketplace Treatment × Online	-0.35	5.04***	3.14***
	(0.54)	(0.85)	(0.79)
Joint Effect	-0.08	7.83***	5.01***
S.E.	0.37	0.79	0.62
Unit of observation	Unit-transaction	Transaction	Transaction
Level of FE	UPC	Household	Household
N	268,524,485	26,850,238	26,850,238

Table shows the effects of economic nexus and remote seller tax enforcement on the log of unit prices, an any tax indicator variable, and the tax as a percent of overall price. The highest income group is households with incomes in excess of $100,000. Coefficients are multiplied by 100 for interpretation. All regressions include household fixed effects, year-month fixed effects, and state-specific (pre-treatment) linear time trends. Standard errors are two-way clustered at the state and year-month level.

Table E2: Heterogenous effects on listed price by consumer type

	(1) ln(Unit Price)	(2) ln(Unit Price)	(3) ln(Unit Price)
Online	10.06***	10.51***	10.12***
	(0.99)	(0.48)	(0.48)
Highest Income HH			0.25***
			(0.07)
Nexus Treatment	-0.14	0.01	-0.06
	(0.13)	(0.14)	(0.14)
Marketplace Treatment	0.33**	0.18	0.20
	(0.16)	(0.20)	(0.20)
Nexus × Online	-0.65	1.16*	1.02*
	(0.71)	(0.58)	(0.55)
Marketplace × Online	0.19	-0.82	-0.80
	(0.90)	(0.62)	(0.58)
Online × High			0.21
			(0.72)
Nexus × High			0.10*
			(0.05)
Marketplace × High			0.12
			(0.09)
Nexus × Online × High			-2.01***
			(0.73)
Marketplace × Online × High			1.18
			(1.10)
Joint Effect	-0.47	0.34	-0.83
S.E.	0.73	0.43	0.90
Unit of observation	Unit-transaction	Unit-transaction	Unit-transaction
Level of FE	UPC	UPC	UPC
Group	High Income	Low Income	Interaction
N	56,377,218	211,885,307	268,524,485

Table shows the effects of economic nexus and remote seller tax enforcement on pre-tax prices, by consumer income band, for goods that cost at least $1. The highest income group is households with incomes in excess of $100,000. Coefficients are multiplied by 100 for interpretation. All regressions include household fixed effects, year-month fixed effects, and state-specific (pre-treatment) linear time trends. Standard errors are two-way clustered at the state and year-month level.

Table E3: Heterogenous effects on tax indicator by consumer type

	(1) Tax Indicator	(2) Tax Indicator	(3) Tax Indicator
Online	-8.88***	-9.60***	-9.61***
	(1.30)	(0.93)	(0.93)
Highest Income HH			0.18
			(0.19)
Nexus Treatment	0.04	0.08	0.12
	(0.24)	(0.20)	(0.22)
Marketplace Treatment	-0.55**	-0.19	-0.12
	(0.25)	(0.21)	(0.21)
Nexus × Online	4.01**	2.06***	2.07***
	(1.55)	(0.63)	(0.63)
Marketplace × Online	4.67**	5.29***	5.32***
	(1.80)	(1.20)	(1.19)
Online × High			0.68
			(0.83)
Nexus × High			-0.29**
			(0.14)
Marketplace × High			-0.57***
			(0.17)
Nexus × Online × High			1.96
			(1.72)
Marketplace × Online × High			-0.86
			(2.43)
Joint Effect	8.68***	7.35***	1.10
S.E.	0.91	1.19	1.50
Unit of observation	Transaction	Transaction	Transaction
Level of FE	Household	Household	Household
Group	High Income	Low Income	Interaction
N	5,291,056	21,559,181	26,850,238

Table shows the effects of economic nexus and remote seller tax enforcement on an any-tax indicator, by consumer income band. The highest income group is households with incomes in excess of $100,000. Coefficients are multiplied by 100 for interpretation. All regressions include household fixed effects, year-month fixed effects, and state-specific (pre-treatment) linear time trends. Standard errors are two-way clustered at the state and year-month level.

Table E4: Heterogenous effects on percent tax by consumer type

	(1) Percent Taxes	(2) Percent Taxes	(3) Percent Taxes
Online	-10.22***	-9.28***	-9.28***
	(1.37)	(0.90)	(0.91)
Highest Income HH			0.17
			(0.19)
Nexus Treatment	0.32	0.26	0.24
	(0.25)	(0.26)	(0.26)
Marketplace Treatment	-0.49**	-0.04	0.02
	(0.24)	(0.27)	(0.25)
Nexus × Online	3.31*	1.12*	1.14*
	(1.75)	(0.59)	(0.59)
Marketplace × Online	2.63	3.59***	3.59***
	(1.68)	(1.15)	(1.14)
Online × High			-0.98
			(0.92)
Nexus × High			-0.15
			(0.21)
Marketplace × High			-0.59***
			(0.19)
Nexus × Online × High			2.18
			(1.77)
Marketplace × Online × High			-1.21
			(2.34)
Joint Effect	5.94***	4.71***	0.97
S.E.	0.70	0.96	1.27
Unit of observation	Transaction	Transaction	Transaction
Level of FE	Household	Household	Household
Group	High Income	Low Income	Interaction
N	5,291,056	21,559,181	26,850,238

Table shows the effects of economic nexus and remote seller tax enforcement on tax as a percent of overall price, by consumer income band. The highest income group are households with incomes in excess of $100,000. Coefficients are multiplied by 100 for interpretation. All regressions include household fixed effects, year-month fixed effects, and state-specific (pre-treatment) linear time trends. Standard errors are two-way clustered at the state and year-month level.

F Effect on prices when goods cost at least $10

Table F1: Effects of nexus and marketplace facilitator treatments on key outcomes

	ln(Unit Price)	Tax Indicator	Percent Taxes
	(1)	(2)	(3)
Online	2.62***	-9.40***	-9.59***
	(0.52)	(0.99)	(0.97)
Nexus Treatment	0.12	0.03	0.20
	(0.24)	(0.20)	(0.23)
Marketplace Treatment	0.29	-0.31	-0.18
	(0.19)	(0.19)	(0.23)
Nexus Treatment × Online	-0.93*	2.78***	1.86**
	(0.50)	(0.66)	(0.76)
Marketplace Treatment × Online	-0.03	5.04***	3.14***
	(0.64)	(0.85)	(0.79)
Joint Effect	-0.96	7.83***	5.01***
S.E.	0.72	0.79	0.62
Unit of observation	Unit-transaction	Transaction	Transaction
Level of FE	UPC	Household	Household
N	22,826,547	26,850,238	26,850,238

Table shows the effects of economic nexus and remote seller tax enforcement on the log of unit prices, an any tax indicator variable, and the tax as a percent of overall price, for goods that cost at least $10. Coefficients are multiplied by 100 for interpretation. All regressions include household fixed effects, year-month fixed effects, and state-specific (pre-treatment) linear time trends. Standard errors are two-way clustered at the state and year-month level.

Table F2: Heterogenous effects on listed price by consumer type

	(1) ln(Unit Price)	(2) ln(Unit Price)	(3) ln(Unit Price)
Online	3.05***	2.41***	3.12***
	(0.74)	(0.48)	(0.48)
Highest Income HH			0.37**
			(0.17)
Nexus Treatment	-0.03	0.32	0.17
	(0.23)	(0.29)	(0.25)
Marketplace Treatment	0.47**	-0.03	-0.05
	(0.21)	(0.20)	(0.21)
Nexus × Online	-0.16	-1.42**	-1.53**
	(0.90)	(0.65)	(0.61)
Marketplace × Online	-0.40	0.31	0.29
	(1.03)	(0.67)	(0.66)
Online × High			-1.00*
			(0.56)
Nexus × High			-0.09
			(0.12)
Marketplace × High			0.57***
			(0.16)
Nexus × Online × High			1.19
			(1.05)
Marketplace × Online × High			-0.55
			(1.17)
Joint Effect	-0.56	-1.11	0.63
S.E.	1.18	0.75	1.31
Unit of observation	Unit-transaction	Unit-transaction	Unit-transaction
Level of FE	UPC	UPC	UPC
Group	High Income	Low Income	Interaction
N	11,121,542	11,624,927	22,826,547

Table shows the effects of economic nexus and remote seller tax enforcement on pre-tax prices, by consumer income band, for goods that cost at least $10. The highest income group is households with incomes in excess of $70,000. Coefficients are multiplied by 100 for interpretation. All regressions include household fixed effects, year-month fixed effects, and state-specific (pre-treatment) linear time trends. Standard errors are two-way clustered at the state and year-month level.

G Effects with saturated state, year-month, and online FE specification

Table G1: Effects of nexus and marketplace facilitator treatments on key outcomes

	ln(Unit Price)	Tax Indicator	Percent Taxes
Nexus × Online	5.77**	-1.45*	-1.17
	(2.69)	(0.85)	(0.88)
Marketplace × Online	-2.49	5.04***	3.66***
	(3.46)	(1.05)	(0.99)
Joint Effect	3.27	3.59**	2.49**
S.E.	3.88	1.41	1.24
Unit of observation	Unit-transaction	Transaction	Transaction
Level of FE	UPC	Household	Household
N	268,798,409	26,850,238	26,850,238

Table shows the effects of economic nexus and remote seller tax enforcement on an any-tax indicator, by consumer income band. The highest income group is households with incomes in excess of $70,000. Coefficients are multiplied by 100 for interpretation. All regressions include state fixed effects, year-month fixed effects, state-specific (pre-treatment) linear time trends, and year-month, state, and online interaction effects. Standard errors are two-way clustered at the state and year-month level.

H Effects for subsample of goods that are typically taxable

Table H1: Effects of nexus and marketplace facilitator treatments on key outcomes

	ln(Unit Price)	Tax Indicator	Percent Taxes
	(1)	(2)	(3)
Online	15.67***	-9.89***	-10.44***
	(0.71)	(0.79)	(0.74)
Nexus Treatment	0.35	-0.10	0.05
	(0.26)	(0.24)	(0.22)
Marketplace Treatment	0.52	-0.16	0.01
	(0.37)	(0.20)	(0.25)
Nexus Treatment × Online	0.31	3.47***	2.30***
	(0.90)	(0.66)	(0.69)
Marketplace Treatment × Online	-0.31	6.10***	3.71***
	(0.96)	(0.82)	(0.95)
Joint Effect	-0.00	9.57***	6.02***
S.E.	0.66	0.87	0.80
Unit of observation	Unit-transaction	Transaction	Transaction
Level of FE	UPC	Household	Household
N	51,267,480	14,696,461	14,696,461

Table shows the effects of economic nexus and remote seller tax enforcement on the log of unit prices, an any tax indicator variable, and the tax as a percent of overall price. Coefficients are multiplied by 100 for interpretation. All regressions include household fixed effects, year-month fixed effects, and state-specific (pre-treatment) linear time trends. Standard errors are two-way clustered at the state and year-month level.

Table H2: Heterogenous effects on listed price by consumer type

	(1) ln(Unit Price)	(2) ln(Unit Price)	(3) ln(Unit Price)
Online	15.71***	16.56***	15.26***
	(0.95)	(0.74)	(0.70)
Highest Income HH			0.15
			(0.18)
Nexus Treatment	0.18	0.40	0.26
	(0.22)	(0.29)	(0.27)
Marketplace Treatment	0.71*	0.33	0.32
	(0.37)	(0.36)	(0.35)
Nexus × Online	-0.86	2.06**	1.61*
	(1.13)	(0.93)	(0.84)
Marketplace × Online	0.80	-1.89	-1.84
	(1.54)	(1.22)	(1.19)
Online × High			0.88
			(0.84)
Nexus × High			0.17
			(0.16)
Marketplace × High			0.38*
			(0.20)
Nexus × Online × High			-2.69***
			(0.98)
Marketplace × Online × High			3.02
			(2.04)
Joint Effect	-0.06	0.17	0.32
S.E.	1.44	1.07	2.14
Unit of observation	Unit-transaction	Unit-transaction	Unit-transaction
Level of FE	UPC	UPC	UPC
Group	High Income	Low Income	Interaction
N	21,837,233	29,265,594	51,267,480

Table shows the effects of economic nexus and remote seller tax enforcement on pre-tax prices, by consumer income band, for goods that are typically taxable. The highest income group is households with incomes in excess of $70,000. Coefficients are multiplied by 100 for interpretation. All regressions include household fixed effects, year-month fixed effects, and state-specific (pre-treatment) linear time trends. Standard errors are two-way clustered at the state and year-month level.

Table H3: Heterogenous effects on tax indicator by consumer type

	(1) Tax Indicator	(2) Tax Indicator	(3) Tax Indicator
Online	-9.04***	-10.63***	-10.64***
	(0.95)	(0.80)	(0.80)
Highest Income HH			0.17
			(0.18)
Nexus Treatment	0.13	-0.23	0.06
	(0.26)	(0.29)	(0.31)
Marketplace Treatment	-0.19	-0.10	0.05
	(0.24)	(0.28)	(0.28)
Nexus × Online	4.86***	1.95**	1.89**
	(1.01)	(0.80)	(0.79)
Marketplace × Online	5.50***	6.44***	6.54***
	(1.16)	(1.35)	(1.34)
Online × High			1.59**
			(0.76)
Nexus × High			-0.33
			(0.31)
Marketplace × High			-0.39
			(0.34)
Nexus × Online × High			3.09**
			(1.25)
Marketplace × Online × High			-1.12
			(1.88)
Joint Effect	10.36***	8.39***	1.97
S.E.	0.93	1.38	1.51
Unit of observation	Transaction	Transaction	Transaction
Level of FE	Household	Household	Household
Group	High Income	Low Income	Interaction
N	6,152,909	8,543,548	14,696,461

Table shows the effects of economic nexus and remote seller tax enforcement on an any-tax indicator, by consumer income band. The highest income group is households with incomes in excess of $70,000. Coefficients are multiplied by 100 for interpretation. All regressions include household fixed effects, year-month fixed effects, and state-specific (pre-treatment) linear time trends. Standard errors are two-way clustered at the state and year-month level.

Table H4: Heterogenous effects on percent tax by consumer type

	(1) Percent Taxes	(2) Percent Taxes	(3) Percent Taxes
Online	-10.73***	-10.18***	-10.20***
	(0.95)	(0.80)	(0.80)
Highest Income HH			0.40*
			(0.22)
Nexus Treatment	0.08	0.07	0.29
	(0.22)	(0.29)	(0.33)
Marketplace Treatment	-0.12	0.09	0.23
	(0.26)	(0.32)	(0.29)
Nexus × Online	4.07***	0.48	0.49
	(1.15)	(0.73)	(0.72)
Marketplace × Online	3.12**	4.16***	4.23***
	(1.17)	(1.46)	(1.45)
Online × High			-0.53
			(0.91)
Nexus × High			-0.48
			(0.36)
Marketplace × High			-0.42
			(0.25)
Nexus × Online × High			3.69***
			(1.36)
Marketplace × Online × High			-1.15**
			(1.83)
Joint Effect	7.19***	4.64***	2.54
S.E.	0.96	1.16	1.36
Unit of observation	Transaction	Transaction	Transaction
Level of FE	Household	Household	Household
Group	High Income	Low Income	Interaction
N	6,152,909	8,543,548	14,696,461

Table shows the effects of economic nexus and remote seller tax enforcement on tax as a percent of overall price, by consumer income band. The highest income group is households with incomes in excess of $70,000. Coefficients are multiplied by 100 for interpretation. All regressions include household fixed effects, year-month fixed effects, and state-specific (pre-treatment) linear time trends. Standard errors are two-way clustered at the state and year-month level.