Journal of Economics and Public Finance
ISSN 2377-1038 (Print) ISSN 2377-1046 (Online)
Vol. 9, No. 1, 2023
www.scholink.org/ojs/index.php/jepf

Original Paper

Did *Wayfair* Help Brick and Mortar Stores?

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Received: December 7, 2022 Accepted: December 19, 2022 Online Published: January 5, 2023

doi:10.22158/jepf.v9n1p34 URL: http://dx.doi.org/10.22158/jepf.v9n1p34

Abstract

Using a detailed establishment level data, this study finds that the imposition of a sales tax on remote sellers resulted in a 4.8 to 7.2 percent sales increase for brick and mortar retail sellers in 2018 and 2019. Employment gains for such brick and mortar sellers for this period was approximately 4.3 percent. Such sales and employment increases were not even, as bigger establishments, those part of a national chain, and those part of a publicly-traded company generally experienced larger sales and employment gains. However, the onset of the pandemic saw much of this gain erased, as sales declined and shifted back to online retailers.

Keywords

sales tax, remote sellers, retail

1. Introduction

The U.S. Supreme Court's decision in *Wayfair v. South Dakota* (Note 1) in June 2018 brought major changes to sales/use taxes. The decision, reversing a previous Supreme Court decision in *Quill* (Note 2), allowed for states to require that remote, out of state vendors collect sales taxes on sales even there was no physical presence by such vendors in the state. States quickly adopted the South Dakota statute, so that by the end of 2019 almost all states having a sales tax required out of state vendors who met certain annual sales thresholds to collect such taxes. Table 1 shows by-state adoptions by year, and also shows sales/transactions thresholds which trigger such collection responsibilities.

Table 1. Remote Seller Sales Tax Collection Responsibility

State	Enforcement date	Sales/transactions Threshold per Year
Alaska	Varies by district	Although no state tax, a number of districts have local sales tax. Threshold varies by district
Arizona	October 1, 2019	$200,000 (2019) $150,000 (2020) $100,000 (2021 forward)
Arkansas	July 1, 2019	$100,000 or 200 transactions
California	April 1, 2019	$500,000
Colorado	December 1, 2018	Prior to April 1, 2019: $100,000 or 200 transactions As of April 1, 2019: $100,000 (The transactions threshold is eliminated)
Connecticut	December 1, 2018; amended as of July 1, 2019	- Prior to July 1, 2019: Gross receipts of $250,000 and 200 retail transactions - As of July 1, 2019: Gross receipts of $100,000 and 200 retail transactions
Florida	July 1, 2021	$100,000
Georgia	January 1, 2019	- Prior to January 1, 2020: $250,000 or 200 transactions. - As of January 1, 2020: $100,000 or 200 transactions.
Hawaii	July 1, 2018	$100,000 or 200 transactions
Idaho	June 1, 2019	$100,000
Illinois	-October 1, 2018 (for state use tax) -January 1, 2021 (for local use tax)	$100,000 or 200 transactions
Indiana	October 1, 2018	$100,000 or 200 transactions
Iowa	January 1, 2019	-Prior to July 1, 2019: $100,000 or 200 transactions -As of July 1, 2019: The transactions threshold is eliminated
Kansas	July 1, 2021	$100,000
Kentucky	October 1, 2018	$100,000 or 200 transactions
Louisiana	July 1, 2020	$100,000 or 200 transactions
Maine	July 1, 2018	$100,000 or 200 transactions

Maryland	October 1, 2018	$100,000 or 200 transactions
Massachusetts	October 1, 2019	$100,000
Michigan	After September 30, 2018	$100,000 or 200 transactions
Minnesota	October 1, 2018, amended as of October 1, 2019	-Prior to October 1, 2019: 10 or more sales totaling $100,000 or 100 retail sales -As of October 1, 2019: $100,000 or 200 or more retail sales
Mississippi	September 1, 2018	$250,000
Missouri	January 1, 2023	$100,000
Nebraska	April 1, 2019	$100,000 or 200 transactions
Nevada	October 1, 2018	$100,000 or 200 transactions
New Jersey	November 1, 2018	$100,000 or 200 transactions
New Mexico	July 1, 2019	$100,000
New York	Effective “immediately after the Wayfair ruling, June 21, 2018”; however, no clearly stated effective date is currently provided	$500,000 and 100 transactions
North Carolina	November 1, 2018	$100,000 or 200 transactions
North Dakota	October 1, 2018	-Prior to January 1, 2019: $100,000 or 200 transactions -As of January 1, 2019: $100,000 only
Ohio	August 1, 2019	$100,000 or 200 transactions
Oklahoma	November 1, 2019	$100,000
Pennsylvania	July 1, 2019	$100,000
Rhode Island	July 1, 2019	$100,000 or 200 transactions
South Carolina	November 1, 2018	$100,000
South Dakota	November 1, 2018	$100,000 or 200 transactions
Tennessee	July 1, 2019	-Prior to October 1, 2020: $500,000 -As of October 1, 2020: 100,000
Texas	October 1, 2019	$500,000
Utah	January 1, 2019	$100,000 or 200 transactions
Vermont	July 1, 2018	$100,000 or 200 transactions
Virginia	July 1, 2019	$100,000 or 200 transactions
Washington	October 1, 2018	$100,000
Washington, D.C.	January 1, 2019	$100,000 or 200 transactions
West Virginia	January 1, 2019	$100,000 or 200 transactions

Wisconsin	October 1, 2018	- Sales only (200 transactions threshold eliminated effective February 20, 2021) -Sales/transactions threshold: $100,000 (200 transactions threshold eliminated effective February 20, 2021)
Wyoming	February 1, 2019	$100,000 or 200 transactions

The fiscal importance of these law changes are potentially significant in light of increasing web-based commerce. Using 2000 through 2019 data from Census, Figures 1 and 2 show ecommerce sales as a percent of total retail sales, and aggregate retail sales for both ecommerce and in total, respectively; the share of ecommerce sales rose from .93 percent in 2000 to approximately 10.7 percent in 2019. Since much of these ecommerce-based sales were thought to escape sales taxation, the *Wayfair* law changes were hoped to be a boon to state tax collections, as sales previously escaping tax would now be taxable. Indeed, Fox, Hargaden, and Luna (2021) using state-level data, found increased sales tax revenues by 5.4 percent, more so in states with stringent compliance standards, following state adoptions of *Wayfair*-type laws. They also found evidence of a full pass-through of the tax to consumer prices.

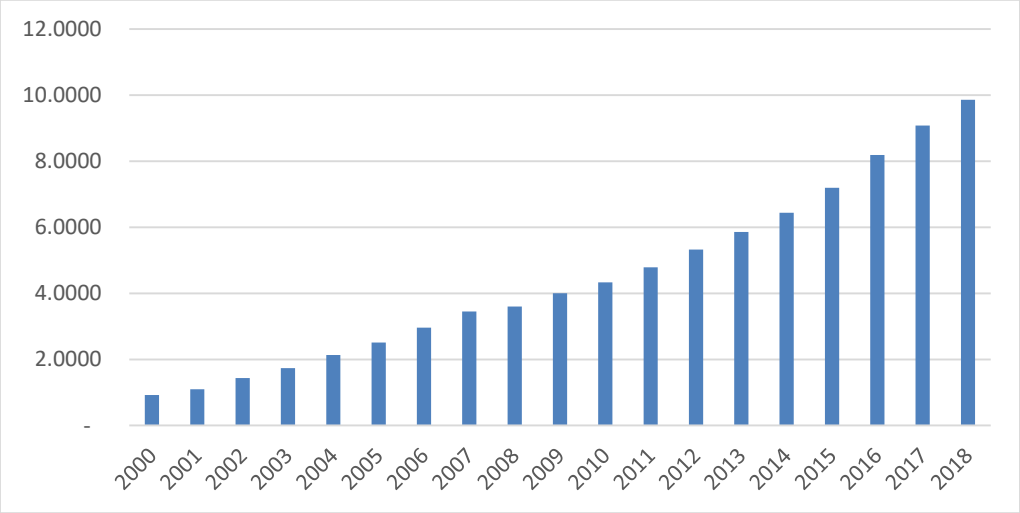

Figure 1. ECommerce Sales as a Percent of Retail Sales

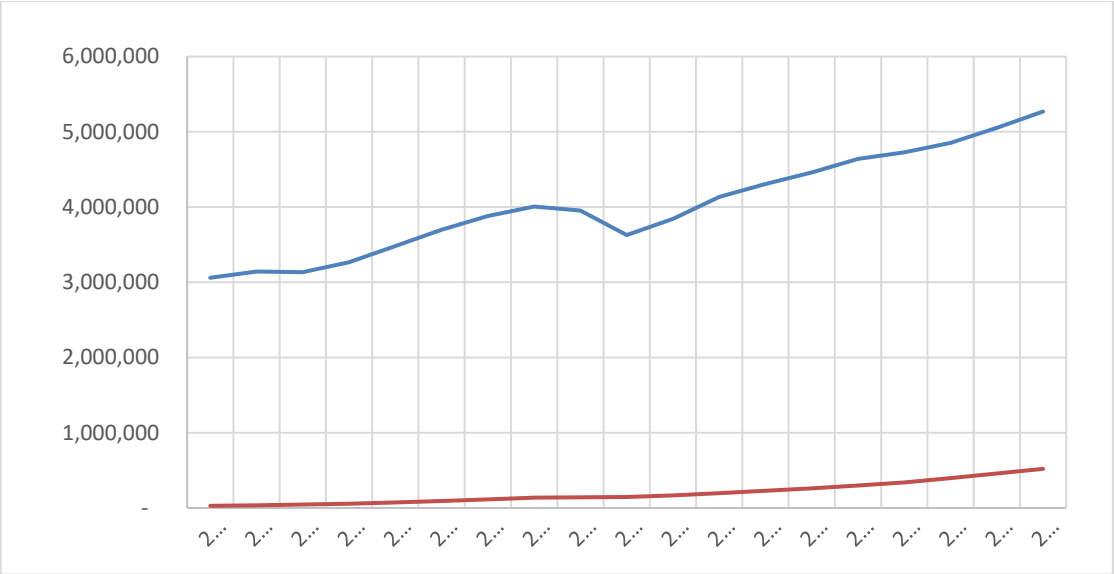

Figure 2. Total Retail Sales (top line) and Total ECommerce Sales (bottom line) in $millions

Given that such ecommerce sales taxes were paid by consumers, this begs the question of whether, given the tax equivalency of ecommerce transactions versus brick and mortar transactions after *Wayfair*, consumers would return to brick and mortar stores for their purchases. Indeed, the increasing competitive threat from online retailers for traditional retailers was the impetus for proposed law such as the *Main Street Fairness Act* (Note 3), whose intent was to return lost business to brick and mortar stores through sales tax equivalence between ecommerce and brick and mortar retailers. Whether a return to brick and mortar shopping post-*Wayfair* is far from obvious for a number of reasons. First, consumers may have viewed the convenience of online shopping as higher than traveling to stores, particularly if a multitude of stores needed to be visited (Note 4). In contrast, marketplace facilitators such as Amazon offered the convenience of door step delivery from a variety of vendors. Second, due to the market power of larger providers, pre-tax prices for ecommerce sellers might actually be lower than those of brick and mortar retailers. Finally, competition from online vendors may have forced closure (or lowered selection) of many local stores, necessitating the use of online vendors.

Several empirical studies have examined the role of sales tax on ecommerce. Goolsbee (2000 a, b) estimated that up to 24 percent of online purchasers would not have purchased online if internet transactions were taxed. Later studies by Alm and Melnik (2005) and Scanlan (2007) performed a similar exercise using questions in the 2001 Current Population Survey. Ellison and Ellison (2009) examine detailed data on the sale of computer memory modules by a retailer located in California. Using price search data, they estimate that consumers searching for certain memory modules are highly price-sensitive, with price elasticities on of around -50 and tax-price elasticities of around -10. They also found that states with a one percentage point higher tax rate have almost 6% more purchases from the retailer. Smith and Brynjolfsson (2001), Anderson et al. (2010) and Goolsbee et al. (2010) also reported relatively high tax sensitivities for specific products (online books, clothing, and cigarettes).

Hortacsu, Martinez-Jerez, and Douglas (2009) use a sample of eBay transactions collected between February and May 2004, and holding online expenditures fixed, they estimated a one percentage point increase in state sales tax decreases same-state online purchases by 10% or more. Finally, Einav et al. (2014), using detailed eBay data, suggested that a state's sales tax reduces the behavior of online browsing as well as purchasing from the online sellers in the same state, while out-of-state online purchasing increases. They found that we find that a one percentage point increase in a state's sales tax increases online purchases by state residents by just under two percent, but decreases their online purchases from home-state retailers by 3-4 percent.

Two studies directly examined the effects of online sales taxes. Baugh et al (2018), using transaction-level data, documented that households living in taxed states reduced Amazon purchases by 9.4% after sales tax laws were implemented, implying elasticities ranging from –1.2 to –1.4. Baugh et al (2018) found that this effect was more pronounced for large purchases, for which they estimate a reduction of 29.1% in purchases, corresponding to an elasticity of –3.9. Baker et al (2021), using comprehensive high-frequency state and local sales tax data, showed that shopping behavior responds strongly to changes in sales tax rates. They showed that consumers stocked up on storable goods before taxes rose and increased online and cross-border shopping in both the short and long run. Embedding an inventory problem into a continuous-time consumption-savings model, Baker et al (2021) demonstrated that this behavior was optimal in the presence of shopping trip fixed costs. Their models matched estimated short-run and long-run tax elasticities with an implied after-tax reservation wage of $7-10.

In a study more closely related to this paper, Breen and Bruce (2021) use state-level panel data from Census Bureau's Business Dynamics Statistics (BDS) program to examine the extent to which changes in sales tax nexus were associated with changes in firm activity between 1979 and 2014. Their estimates suggested that the number of firms, establishments, and employment levels responded negatively to non-neutral sales tax nexus standards as reflected in the gradual erosion of state sales tax bases. Specifically, their results suggested that increasing sales tax base breadth by 1 percentage point generated 0.14 percent additional firms and establishments and 0.2 percent higher employment levels. They also estimated that increasing the share of online companies with nexus by 1 percentage point translated into 0.1 percent additional (small) firms as the sales-tax-collection obligation is dispersed among a larger share of firms. Their simulations, assuming a 50-percent recovery in base breadth, suggested that the national economy would have seen an additional 90,350 firms, 113,600 establishments, and roughly 2.9 million jobs.

The above studies generally indicate a substantial tax-elasticity with respect to sales taxes on internet commerce. While the above studies *suggest* that purchases may switch to brick and mortar stores, this may not be the case if budget constraints were binding; that is, customers simply did not buy an item(s) anywhere after a sales tax was imposed on ecommerce, or bought less items in total on the ecommerce platform. Accordingly, this paper examines whether post-*Wayfai*r law changes caused sales to shift

back to local retailers. To avoid confounds, I examine only pre-pandemic sales of 2018 and 2019. Using data from a variety of sources, I find that, depending on measurement used, brick and mortar retailers' sales increased 4.8 to 7.2 percent, and that related employment increased by 4.3 percent. Such sales and employment increases were not even, as bigger establishments, those part of a national chain, and those part of a publicly-traded company generally experienced larger sales and employment gains. However, the onset of the pandemic saw much of this gain erased, as sales declined and shifted back to online retailers. Since over 40 percent of Ecommerce sales are attributable to Amazon, the findings here may have bearing on recent proposals to break up Amazon.

2. A Simple Model of Consumer Choice

Consider consumer purchasing behavior in an economy with two competing sellers who sell otherwise identical products, x and y, that are supplied perfectly elastically (Note 5). y is provided by a brick and mortar vendor, while x is sold over the internet. Assume y is subject to a tax rate τ. Normalize the after-tax price of y to one and let p denote the pretax price of x. Assume that x becomes subject to a sales tax τ at the same rate as the brick and mortar vendor. The total price of x is $q = (1 + \tau)\, p$. The price that consumers see when deciding on the internet purchase is p; the tax is not included in the posted price, and is later imposed at "check out" at the applicable state, country, and city tax rates. Here we assume that the consumer knows some sort of tax applies and must estimate it. Thus, since consumers must estimate q themselves but can see only p directly, the tax-inclusive price q is less "salient" than the pretax price p.

Let $x\,(p, \tau)$ denote demand as a function of the posted price and the sales tax. In the neoclassical full-optimization model, demand depends only on the total tax-inclusive price: $x\,(p, \tau) = x\,((1 + \tau)\, p, 0)$. If consumers optimize fully, a 1 percent increase in p and a 1 percent increase in the gross-of-tax price $(1 + \tau)$ reduce demand by the same amount: $\varepsilon_{x,p} \equiv -(\partial \log x)/(\partial \log p) = \varepsilon_{x,1+\tau} \equiv -(\partial \log x)/((\partial \log (1 + \tau))$. In practice consumers underreact to the tax τ because it is less salient: $\varepsilon_{x,p} > \varepsilon_{x,1+\tau}$. Log-linearizing the demand function $x\,(p, \tau)$ we obtain:

$$\log x\,(p, \tau) = \alpha + \beta \log p + \theta_\tau \beta \log (1 + \tau) \quad (1)$$

where θ_τ measures the degree to which the consumer reacts to the tax. Under-reaction variable can be due to a number of factors: cognitive limitations; the costliness of obtaining information on the tax, etc. On the other hand, θ_τ might be non-zero. For example, customers who purchase repeatedly on the internet may become aware of the tax and alter their shopping in favor of brick and mortar stores. Or, businesses consumers may have resources to know tax rates, and switch purchases to brick and mortar stores.

The above model assumes that the only difference between the two goods is after-tax price. However, other differences can be salient. For example, the ecommerce purchase avoids the costs of driving to brick and mortar stores for the same good when the same good can be obtained with doorstep convenience. Alternatively, the consumer may enjoy the physical act of shopping in person, or (s)he

may value the additional customer service of an in-store shopping experience. Converting the above to a utility model, consumer utility for an online purchase and an in-store purchase, respectively, are

$$u_o(x)=v-(p)-s+W, \quad (2)$$

$$u_i(y)=v-(p+\tau)-c+Z, \quad (3)$$

where v is the reservation price (consumer utility of the good itself), s are shipping costs for an online purchase, W is a vector of other idiosyncratic aspects of an online purchase which a consumer may value, c is the cost (in time and money) to commute to a brick and mortar store, Z is a vector of other idiosyncratic aspects of a brick and mortar purchase which a consumer may value, and other terms are defined as above. Here, we start with the assumption that there is no tax t on the internet purchase. If we add such a tax, then for online purchase to be preferred to the in-store purchase, or $u_o(x)' > u_i(y)'$, then $t < (\frac{-s+W}{-c+Z})$ must hold. In other words, the ratio of the consumer's preference for online shopping (less shipping costs) to her preferences for in-store shopping (less commuting costs) must exceed t.

It follows that sales revenue collected by the vendors of x and y will be affected by such individual purchases, and that such effects will be measurable at the industry levels for internet-based providers and brick and mortar vendors. However, in a competitive situation, if taxes cause lowered demand, internet based providers can lower pre-tax prices to be more competitive. The above model assumes a competitive environment, which may not always be the case. For example, the largest internet-based seller (Amazon) provides a significant amount of goods and might be considered a monopsonist in its industry.

3. Retail Sales Activity Post-*Wayfair*

In this section I examine post-*Wayfair* sales using a variety of data, each of which has its advantages and disadvantages. Panels A and B of Table 2 reports aggregate sales from Census (Note 6) pre and post-*Wayfair* sale trends, using a simple difference in differences (DID) method. Panel A computes DID by using the sums of 2014-2015, 2016-2017, and 2018-2019 sales, and compares growth rates for these periods in percents. Post-*Wayfair* Brick and mortar sales grew at 5.71 percent, which was 1.96 percent higher than pre-*Wayfair* growth. Ecommerce sales grew by 31.2 percent, but this was actually only .27 percent higher than the pre-*Wayfair* sales growth. Looking at the differences between them, we see that brick and mortar sales post-*Wayfair* sales growth outpaced that of ecommerce by .169 percent. Panel B takes a closer look at the three largest Ecommerce providers. Using Form 10k data, I examine just ecommerce sales to America for these retailers, and examining differences in differences, I find that Amazon sales declined (again, using a DID analysis) 5.6 percent (Note 7), EBay sales declined 6.8 percent, and Wayfair sales declined 7.5 percent post Wayfair.

Table 2.

Panel A: Growth in Sales for Retail Ecommerce and Brick and Mortar Stores, 2015-2019: National Data ($millions)

	Sales 2014+2015	**Sales 2016+2017**	**Difference %**	**Sales 2018+2019**	**Difference %**	**DID%**
Brick and Mortar	8,733,820	9,061,740	0.0375	9,579,458	0.0571	.0196
Ecommerce	636,226	826,687	0.2993	1,084,616	0.3120	.0027
Difference					**0.0169**	

Panel B: Sales for Top 3 ECommerce-Based Companies ($millions)

	Sales 2015	**Sales 2017**	**Difference %**	**Sales 2019**	**Difference %**	**DID %**
Amazon	63,708	106,110	0.6656	170,773	0.6094	-.0562
EBay	3,624	4,091	0.1289	4,337	0.0601	-.0688
Wayfair	2,135	4,153	0.9452	7,764	0.8694	-0.075

Data from Form 10k; see discussion in text.

Panel C: Regressions, Annual Sales, National by Major Industry, 2010-2019 ($billions)

	ln Total Brick and Mortar Sales
Intercept	5.36222***
	(0.11115)
Sales Tax on Remote Sellers	0.048042***
	(0.09079)
Year and NAICS fixed effects	Yes
Chi squared	28***
N	92

*** significant at .01 or better ** significant at .05 *significant at .10 Robust standard errors in parentheses

Panel C of Table 2 uses aggregate (industry level) data from Census for 2007 through 2019 (Note 8) to run a traditional panel data regression on the log of retail sales for each NAICS industry *i* at for brick and mortar retailers. The model is:

$$Ln(QSales_{i,n}) = \alpha + \alpha_1 SalesTaxRemoteSellers + \phi YEAR_t + \psi NAICS_N + \mathcal{E}_{it}, \quad (4)$$

where *SalesTaxRemoteSellers* is a dummy variable set to 1 for each year in which the remote seller tax is active for that state (2018 or 2019), with *YEAR* and *NAICS* code fixed effects. The results show that brick and mortar retailer sales increased by 4.8 percent post *Wayfair* (significant at .001). The impact of TAX may be understated since for 2018, since only 21 of the 44 states imposing a sales tax had adopted Wayfair-like statutes.

Because of the aggregate nature of the data, standard errors may be inflated which reduce the power of the tests. The 2019 National Establishment Time-Series (NETS) Database has establishment-level data which not only allows for more powerful tests, but also has additional information allowing for more specific tests. NETS is a unique, establishment- specific database derived from the Dun & Bradstreet data, the latter of which is used commercially. This data set became available to academics in 2007, and has been used in a number of economics papers (e.g., Decker et al., 2014; Groizard et al., 2015; Haltiwanger et al., 2015; and Neumark et al., 2011). The database has recently been used for a few peer-reviewed studies in tax; see Kolko and Neumark (2010) and Swenson (2014). The 2019 national NETS Database includes an annual time-series of information on over 36.5 million U.S. establishments from 1990 to 2019. Among other establishment-level items, this database reports sales, employment, industry (at 8 digit NAICS levels), exact location, and affiliation with other establishments (parents, subsidiaries, number of other establishments within the same legal entity). The NETS also reports information on establishment "moves"-- where the establishment moved to/from, year of move, as well as sales and employment moved. Details of the NETS database are reported in an on-line technical Appendix (Note 9). Neumark et al. (2007) conducted a detailed analysis of the quality of the NETS data along various dimensions, and concluded that the NETS by and large provides reliable measurement of employment levels, births and deaths, business relocations, etc. (Note 10).

Because the NETS data is expensive (Note 11), I focus on states most likely to have shown brick and mortar sales increase post *Wayfair*, i.e., the 21 states which adopted remote seller sales tax rules in 2018, giving us two years of post-*Wayfair* and pre-pandemic data. Because examining all retail establishments in these states is prohibitively expensive, I randomly select 250,000 retail establishments from these states, with the number of establishments sampled from each state proportional to that state's population (see Appendix for details). As a second dataset, I also use the entire NETS data for South Dakota, the "bellwether" state where the *Wayfair* decision originated (South Dakota adopted remote seller rules in 2016 (effective largely in 2017) (Note 12). Because South Dakota started remote seller taxes a year before other states, it may be that we will observe larger sales and employment effects in this state. It is important to note that that the chosen NETS establishments do not include any distribution centers which might have a significant ecommerce component. I run a traditional panel data analysis regression model as follows:

$$Ln(Sales_{e,t,})=\alpha + \alpha_1 SalesTaxRemoteSellers_t + \phi YEAR_t + \sum_{i=1}^{n} \xi \ SIC_i + \sum_{e=1}^{n} \psi \ ESTAB_e + \mathcal{E}_{it}, \quad (5)$$

where the dependent variable is the log of sales for that establishment in year *t*, and we have fixed effects for 8 digit *SIC* code and random effects for establishment (*ESTAB*) and *YEAR*. $SalesTaxRemoteSellers_t$ is set to 1 for the year in which the establishment's state adopted *Wayfair*-type sales tax collection responsibility (Note 13). Results are shown in Table 3.

Table 3. Regressions, Establishment Level Annual Sales (in actual $) for Brick and Mortar Retailers, 2010-2019: States Adopting *Wayfair* Rules in 2018

Panel A: All States

	(1)	(2)	(3)	(4)	(5)
	ln Sales—all establish-ments	*ln* Sales—establish-ments with sales above median	*ln* Sales—establish-ments with sales below median	*ln* Sales — single location	*ln* Sales — multiple locations
Intercept	12.7249***	13.3522***	11.2866***	12.3100***	14.0464***
	(0.0038)	(0.0038)	(0.0019)	(.0006)	(0.0014)
Sales Tax on Remote	0.0753***	0.0746***	0.0579***	0.0529***	0.1413***
Sellers	(0.0008)	(0.0009)	(0.0008)	(0.0006)	(0.0014)
Establishment, Year, and 6 Digit NAICS Fixed Effects	Yes	Yes	Yes	Yes	Yes
Chi squared	7472***	5902***	4340***	6376***	10176***
N	1,268,014	890,260	377,754	925,776	342,234

Panel B: South Dakota

	(1)	(2)	(3)	(4)	(5)
	ln Sales—all establish-ments	*ln* Sales—establish-ments with sales above mean	*ln* Sales—establish-ments with sales below mean	*ln* Sales — single location	*ln* Sales — multiple locations
Intercept	12.8135***	14.41875***	11.87088***	12.3551***	14.4165***
	(0.02166)	(0.02868)	(0.011694)	(0.018635)	(0.05244)
Sales Tax on Remote	0.06549***	0.083979**	0.039611***	0.04222***	0.14178***
Sellers	(0.00779)	(0.006528)	(0.00407)	(0.004747)	(0.013349)
Establishment, Year, and 6 Digit NAICS Fixed Effects	Yes	Yes	Yes	Yes	Yes
Chi squared	176	165	94	79	112
N	41,010	17,020	23,990	30,102	10,908

*** significant at .01 or better. ** significant at .05 *significant at .10 Robust standard errors in parentheses. Total establishments in South Dakota are 212,422.

Panel A shows that national (for the 20 states adopting *Wayfair* in 2018) brick and mortar sales increased by 7.2% (significant at .001). Since larger establishments may have recovered more quickly (from pre-*Wayfair* declines) than smaller ones, I also run separate regressions where one regression uses just establishments with sales above the mean, and the other regression uses just establishments with sales below the mean. For the national dataset, there was a 7.45 percent increase in post *Wayfair* sales for larger establishments, and a 5.79 percent increase for smaller ones (both significant at .001). I also run separate regression for establishments based on whether they were standalones or part of a chain (multiple locations). The regressions estimate a post *Wayfair* sales increase of 5.2 percent for single establishments and a 14.1 percent increase for chain establishments (both significant at .001).

Panel B of Table 4 shows regressions results for sales for South Dakota retailers. The results are similar to the national data set regressions. There was a 6.5 percent increase in post *Wayfair* sales overall, with an 8.3 percent increase for larger establishments, and a 3.9 percent increase for smaller ones (both significant at .001). There is a post *Wayfair* sales increase of 2.2 percent for single establishments and a 14.4 percent increase for chain establishments (both significant at .001). The similarity of these results (which uses all retailers for that state) to the national results provides corroboration that the national dataset, using a random sample of retailers, is a reasonable estimate.

The conclusion is that imposition of the remote seller sales tax did indeed increase brick and mortar sales, in the range of 4.8 to 7.2 percent (averaging across Tables 2 through 4). Importantly, post-*Wayfair* sales indicated a much larger rebound for larger establishments, and those part of a chain.

4. Effects on Payroll and Employment

Since sales for brick and mortar establishments increased post-*Wayfair*, it is worthwhile to see whether this resulted in business expansion for such retailers. Using Census data for brick and mortar retailers for states having a sales tax (Note 14), Table 4 reports an aggregate differences-in-differences (DID) analysis for brick and mortar retailers pre and post *Wayfair*, using state-level data (using only states having a sales tax). The DID compares changes (in percents) of number of establishments, employment, and payroll growth from 2015 to 2017 (before *Wayfair*), to the same growth percents from 2017 to 2019. The Table shows that employment and payroll increased by 3 percent and 4.1 percent, respectively. Traditional panel data regression analyses using Census data are shown in Table 5, using the same data for states with sales tax with fixed effects for year and six digit NAICS. The Table shows a net 3.3 percent growth in employment post-Wayfair (significant at .001), and a .1 percent change in payroll (which is statistically insignificant).

Table 4. DID Analysis, Employment and Payroll, Brick and Mortar Retail: National Data

	2015	2017	Difference %	2019	Difference %	DID %
Employment	14,339,068	14,508,949	0.0118	15,120,677	0.0421	0.0303
Payroll ($th)	381,135,027	402,211,630	0.0552	441,036,026	0.0965	0.0412

Data from *County Business Patterns.* Brick and mortar retail includes NAICS codes 440000 through 454390.

Table 5. Regressions, Payroll and Employment, 2010-2019: National Data by State and NAICS

	(1)	(2)
	***ln* Payroll**	***ln* Employment**
Intercept	7.53236***	4.48649***
	(.04779)	(.04677)
Sales Tax on Remote Sellers	0.00106	0.03301***
	(0.01477)	(0.01387)
Year, State, and 6 digit NAICS Variables	Yes	Yes
Adj. R^2	.4375	.4483
N	28,846	28,846

*** significant at .01 or better ** significant at .05 *significant at .10 Robust standard errors in parentheses. Data from *County Business Patterns.* Brick and mortar retail includes NAICS codes 440000 through 454390.

Table 6 shows panel data regression results for using the NETS data for both South Dakota, and the other 20 states imposing *Wayfair*-type rules in 2018. The models are the same shown in (5), except that the dependent variable is the log of employment (Table 15). Panel A shows national (20 state) results. Here, we see that overall post-*Wayfair* employment increased 4.3 percent. This result is roughly consistent with Table 5 results. For establishments above the sales mean, employment increased by 3.6 percent, and for establishments with sales below the mean, employment increased 3.5 percent. For single locations and chain locations, employment increased by 4 percent and 4.9 percent, respectively. All of the foregoing results are significant at .001.

Table 6. Regressions, Establishment Level Annual Employment for Retail Firms, 2010-2019

Panel A: All States

	(1) *ln* Employ-ment— all establish-ments	(2) *ln* Employment— establish-ments with Employment above median	(3) *ln* Employment— establish-ments with Employment below median	(4) *ln* Employ-men t— single location	(5) *ln* Employ-ment— multiple locations
Intercept	1.66543***(	3.3361**	0.99153***	1.41488***	2.41322***
	0.002270)	(0.003278)	(0.0010767)	(0.001957)	(0.00555)
Sales Tax on Remote	0.04264***	0.03696***	0.03554***	0.04070***	0.04971***
Sellers	(0.000377)	(0.00044)	(0.000409)	(0.003765)	(0.00096)
Establishment, Year, and 6 Digit NAICS Fixed Effects	Yes	Yes	Yes	Yes	Yes
Chi-squared	12764***	8627***	7540***	11691***	2652***
N	1,268,014	890,260	377,754	925,776	342,238

Panel B: South Dakota

	(1) *ln* Employ-ment— all establish-ments	(2) *ln* Employment— establish-ments with Employment above median	(3) *ln* Employment— establish-ments with Employment below median	(4) *ln Employ-me nt — single location*	(5) *ln Employ-ment — multiple locations*
Intercept	1.76993***	2.57609***	1.26907***	1.49396***	2.7294***
	(0.01401)	(0.025319)	(0.05994)	(0.00930)	(0.04391)
Sales Tax on Remote	0.04321***	0.03294***	0.03736***	0.03951**	0.05870***
Sellers	(0.00309)	(0.004471)	(0.002418)	(0.002568)	(0.009456)
Establishment, Year, 6 digit NAICS Fixed Effects	Yes	Yes	Yes	Yes	Yes
Chi-squared	195	54	238	236	38
N	41,010	17,020	23,990	30,119	10,908

*** significant at .01 or better ** significant at .05 *significant at .10 Robust standard errors in parentheses.

Panel B shows that South Dakota employment increased by 4.3 percent. For establishments above the sales median, employment increased by 3.2 percent, and for establishments with sales below the median, employment increased 3.7 percent. For single locations and chain locations, employment increased by 3.9 percent and 5.8 percent, respectively. All of the foregoing results are significant at .001.

Taken together, the national (both aggregate and establishment level) and South Dakota analyses consistently indicate a 4.3 percent post-*Wayfair* growth in employment. As with sales, establishments which were part of a chain fared better than single-level establishments, although there was little difference in employment changes based on size (sales level).

5. Additional Analyses

As noted in previous analyses, brick and mortar sales prior to *Wayfair* were declining, which may have put additional financial distress on these retailers. To see if the post-*Wayfair* period resulted in less stress, I examine Paydex scores (reported in the NETS data). These scores, by Dun and Bradstreet, are monthly credit risk scores, where higher scores (up to a maximum of 100) indicated higher credit default risk. The scores are PaydexMin (minimum score for any one year) and PaydexMax (maximum score for any one year). Regressions in Table 7 results for PaydexMin and PaydexMax show that post *Wayfair*, PaydexMax and PaydexMin scorers declined by .86 and .69, respectively, and both were significant at .001. While minor in magnitude, they are significant changes compared to average score changes over the 2010-2019 period. The results indicted reduced financial stress post-*Wayfair* for brick and mortar retailers.

I also examine the general effects of local Amazon competition (recall that Amazon accounts for approximately 40% of ecommerce sales). For the 20 states examined with NETS data, there was a wide variation in the number of distribution/fulfillment centers in each state (at one extreme, Alabama, Maine, Maryland, Minnesota, and Vermont had no distribution centers, and at the other extreme, Kentucky had 14 distribution centers) (Note 16). A conjecture here is that fewer distribution centers would potentially slow down deliveries to consumers, thereby making Amazon sales potentially less convenient to customers. Table 7 regression results for sales and employment, where there is a variable for the number of Amazon distribution centers in the state, and another for the distribution centers in the state after 2018. The results show that the number of centers in the state after 2018 increased brick and mortar sales by .02 percent, and increased brick and mortar employment by .05 percent.

Table 7. Financial Distress Score and Amazon Regressions, Establishment Level Annual Employment for Retail Firms, 2010-2019 All States

	(1) PaxDex Max Score	(2) Paydex Min Score	(3) *ln* (Sales)	(4) *ln*(Employment)
Intercept	73.8632***	69.0078***	12.65224***	1.62931***
	(0.026061)	(0.040755)	(.005110)	(0.00297)
Sales Tax on Remote Sellers	-0.86036***	-0.69237***	0.067496***	0.039498***
	(0.000377)	(0.031775)	(0.00087)	(0.00054)
Number of Amazon Distribution Centers in State			-0.00834	0.00005
			(0.00062)	(0.00036)
Number of Amazon Distribution Centers in State*Tax			0.00022**	0.00055***
			(0.00010)	(0.00001)
Establishment, Year, and 6 Digit NAICS Fixed Effects	Yes	Yes	Yes	Yes
Chi-squared	474***	1109***	18109***	18240***
N	860,152	860,152	1,268,014	1,268,014

Table 8 reports regressions for sales and employment, for the 20 NETS states I examine, where I split the sample into establishments which were privately held (which comprised approximately ninety percent of the establishments), versus those which were part of a publicly-held corporation (ten percent of the establishments). The idea here is that the latter might have the resources to respond more quickly to increased post-*Wayfair* demand. It is important to note that that the NETS establishments do not include any distribution centers which might have a significant ecommerce component. The regression result show that post-*Wayfair* sales were ten percent higher for publicly-held establishments, but only 6.6 percent higher for privately held establishments. On the other hand, publicly-held establishments only increased employment by three percent post Wayfair, versus approximately 4.5 percent for privately held firms. These results are similar to Table 3 results, where establishments with sales above the mean reported less employment growth post-Wayfair, than establishments with sales below the mean.

Table 8. Establishment Level Annual Sales and Employment for Retail Firms, 2010-2019, All States: Public versus Privately Held Companies

	(1)	(2)	(3)	(4)
	ln (Sales)	*ln*(Sales)	*ln* (Employment)	*ln*(Employment)
	Privately Held	Publicly Traded	Privately Held	Publicly Traded
Intercept	12.44297***	14.7523***	1.52129***	2.7451***
	(0.00277)	(0.00987)	(0.00158)	(0.00726)
Sales Tax on Remote Sellers	0.06509***	0.10089***	0.044889***	0.030487***
	(0.00053)	(0.001632)	(0.00033)	(0.001039)
Establishment, Year, and 6 Digit NAICS Fixed Effects	Yes	Yes	Yes	Yes
Chi-squared	14828***	3822***	18049***	859***
N	1,797,803	199,623	1,797,803	199,623

Finally, Table 9 reports regression results where instead of a dummy variable for 2018 and 2019 (post-*Wayfair*), I use the state sales tax rate times this dummy variable. The idea here is that the increase in post-*Wayfair* sales and employment for brick and mortar sellers may be larger, the larger is the sales tax rate. Regression results in Table 9 confirm this; post-*Wayfair* brick and mortar sales increased by 1.15% for each one percent higher sales tax rate. Similarly, employment increased by .72 percent, for each one percent increase in the state tax rate.

Table 9. Establishment Level Annual Sales and Employment for Retail Firms, 2010-2019 All States: Effects of Sales Tax Rates

	(1)	(2)
	ln (Sales)	*ln*(Employment)
Intercept	12.64557***	1.62907***
	(0.00297)	(0.001732)
Sales Tax on Remote Sellers*Sales Tax Rate	1.15665***	0.721034***
	(0.00874)	(0.00547)
Establishment, Year, and 6 Digit NAICS Fixed Effects	Yes	Yes
Chi-squared	17481***	17321***
N	1,268,014	1,268,014

6. Epilogue

Although the focus of this study is pre-pandemic years, it is instructive to examine ecommerce sales after 2019. Although 2020-2021 NETS data, and detailed Census data, are unavailable (at this writing), aggregate-level Census data is available. Table 10 reports quarterly ecommerce sales and total retail sales for 2019, 2020, and the first quarter of 2021. For 2019, brick and mortar sales were 89.42% of total retail sales. Averaging the next four quarters, this percent dropped to 85.9%. Clearly, the pandemic shifted more sales to ecommerce, essentially erasing most of the brick and mortar gains in 2018-2019 as a result of the sales tax on remote sellers. Whether this trend continues in 2021 and later years is worthy of investigation when such data becomes available.

Table 10. Retail Sales 2019 and Later

	Total Retail	Ecommerce	Brick and Mortar	Brick and Mortar as % of Total
2021 Q1	1,472,314	196,808	1,275,506	0.8663
2020 Q4	1,548,016	235,957	1,312,059	0.8476
2020 Q3	1,460,101	191,573	1,268,528	0.8688
2020 Q2	1,320,701	193,624	1,127,077	0.8534
Totals	5,801,132	817962	4,983,170	**0.8590**
2019	5,411,037	578,501	4,838,536	**0.8942**

7. Limitations

This study has two major limitations. Firstly, there may be a number of state-specific factors (many of which are unobservable) which have an effect on the results. Second, although a number of states adopted *Wayfair* in 2018, the effect may not have been felt until 2019. Indeed, analysis where 2019 is used as the effective date of *Wayfair* shows that there was little or no impact on brick and mortar sales. On the other hand, we cannot ignore that South Dakota showed a positive impact on brick and mortar sales starting in 2018, when the *Wayfair* decision was adopted. Future studies controlling for the foregoing limitations are called for.

8. Conclusion

Using a variety of data sources, I find that the imposition of a sales tax on remote sellers resulted sales increases of 4.8 to 7.2 percent for brick and mortar retail sellers in 2018 and 2019. Employment gains for such brick and mortar sellers was 4.3 percent. In light of an ever-increasing trend for consumers to use ecommerce, arguments that the remote seller sales tax brought back commerce to brick and mortar sellers do appear to have some merit. However, this gain appears to have reversed during pandemic years, and it remains to be seen whether consumers' shift to back to ecommerce becomes permanent or even increases.

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Appendix 1—Estimated Sales and Employment in NETS Data

In estimating our regressions, we need to account for potential measurement error in the NETS data. Since sales and employment data is often not reported by D&B survey respondents for some years, either D&B or the NETS vendor (Walls and Associates) provides estimates, so such estimates tend to smooth the data (i.e, under-report volatility). As described below, in a regression setting, such estimation tends to not bias coefficient estimates, but does inflate standard errors, potentially masking statistical significance.

To see this, consider a model below where y' is regressed on X using OLS estimation

$$y' = \alpha + \beta * X + \mu. \quad \text{(A1)}$$

This example uses a univariate model for simplicity, but X could also be a vector of regressors, as in this study. The dependent variable y' (in our case, sales or employment) is unobservable for at least some of the sample, so, in empirical estimation, y is employed as an observable proxy for y'. The variable y contains measurement error v:

$$v = y - y' = y - v. \quad \text{(A2)}$$

If v has a non-zero mean, then in regression analysis this mean will be captured by α and leaves other parameter estimates unaffected. If v has a mean zero and is uncorrelated with both X and μ, then substituting y' for $(y - v)$, and rearranging v to the right-hand side yields the following:

$$y - v = \alpha + \beta * X + \mu \text{, and} \quad \text{(A3)}$$

$$y = \alpha + \beta * X + (\mu + v), \quad \text{(A4)}$$

where (A4) is the estimable model. The general view taken among researchers is that measurement error in dependent variables does not affect coefficient estimates but simply biases against finding statistical relations. This statement is true in the simple case above; when v is additive and uncorrelated with X and μ, OLS yields unbiased coefficient estimates, and standard errors are larger given the increase in the error variance (i.e., $(\sigma_v+\sigma_\mu)>\sigma_\mu$). As noted above, the intercept α is biased if v has a non-zero mean, but the intercept is not of interest in our study.

As mentioned above, the issue with the D&B data is that a number of sales and employment data are estimated by D&B and by the vendor, Walls and Associates. Whether such estimations have errors which are constant, or if they have a non-constant mean, β parameter estimates will be unbiased, but standard errors will be inflated in the case of errors not having a constant mean.

One method to adjust for such inflated standard errors is to assume that they are a function of two treatment "clusters"; one cluster for the dependent variable being exactly as reported by D&B respondents, the other being missing values estimated by D&B (or Walls) (Note 17). Alternatively, we can cluster on the dependent variable itself (whether sales or employment) on the theory that even

respondent-reported values are estimates, in which case all levels of sales or employment are clusters. Accordingly, in our empirical estimation we cluster on both actual/estimated values, and on sales (or employment) in Tables 3 and 6 through 9. Results (available on request) have slightly higher standard errors compared to unclustered results, but all variables continue to be statistically significant at .001 or better.

Appendix 2

Selecting a Random Sample from NETS Data

20 states which had enacted remote seller taxes in 2018 were elected. From this, randomly-drawn establishments from each of the states were selected, for roughly 250,000 establishments. The samples were proportional to populations, as shown in Table A1 below. Establishments which went out of business before 2018, or without (or missing) sales and/or employment were eliminated. Only retailers with NAICS codes which were not primarily online were selected; a list of these 828 8 digit NAICS codes are available from the author. Retailers which were likely not to have any remote customers (e.g., food service) and retailers with delivery/ecommerce based business (NAICS codes greater than 454390) were eliminated.

Table A1: Sample Selection for NETS Data

state	2019 population	population%	Number of establishments in sample
Alabama	4,903,185	0.045429371	11,357
Colorado	5,758,736	0.053356289	13,339
Connecticut	3,565,287	0.033033375	8,258
Hawaii	1,415,872	0.013118447	3,279
Illinois	12,671,821	0.117407942	29,351
Indiana	6,732,219	0.062375879	15,593
Kentucky	4,467,673	0.041394232	10,348
Maine	1,344,212	0.012454498	3,113
Maryland	6,045,680	0.056014905	14,003
Michigan	9,986,857	0.092531005	23,132
Minnesota	5,639,632	0.052252757	13,063
Mississippi	2,976,149	0.027574847	6,893
Nevada	3,080,156	0.028538501	7,134
New jersey	8,882,190	0.082295958	20,573
North Carolina	10,488,084	0.097175012	24,293
North Dakota	762,062	0.007060716	1,765
South Carolina	5,148,714	0.047704266	11,926

Vermont	623,989	0.005781431	1,445
Washington	7,614,893	0.070554099	17,638
Wisconsin	5,822,434	0.053946469	13,486
Totals	107,929,845		249,989

Notes

Note 1. *South Dakota v. Wayfair, Inc*., 585 U.S. ___. Decision on June 21, 2018.

Note 2. *Quill Corp. v. North Dakota*, 504 U. S. 298.

Note 3. See text in https://www.congress.gov/bill/112th-congress/house-bill/2701. The Act was introduced in 2011 but was allowed to expire.

Note 4. The convenience factor also depends on the ability to wait for an online delivery. Visits to local stores could provide merchandise the same day, whereas online delivery could sometimes take days or longer, depending on vendor and product.

Note 5. We assume that the supply curve is effectively flat; the hotel/motel industry is highly competitive within any particular class of rooms/facilities (e.g., budget hotels versus luxury hotels). See below discussion of a non-competitive environment.

Note 6. Census of Retailers https://www.census.gov/retail/index.html

Note 7. The Amazon result is significant in light the company's collecting of sales taxes prior to Wayfair in some states. It collected in six states as of July 2012 (Jopson, 2012). Later, Amazon adjusted its business model from that of a remote seller without any physical facilities in most states to a company with many distribution warehouses in order to get close to customers and reduce consumers' waiting costs. Accordingly, by 2014, Amazon was forecasted to collect sales tax in approximately half of all 50 states by 2014 (Jopson, 2012).

Note 8. Census, County Business Patterns. https://www.census.gov/programs-surveys/cbp.html

Note 9. This online Appendix will be made available upon publication decision.

Note 10. Because D&B sales and employment data are sometimes missing, the NETS vendor imputes such missing values where feasible. These estimates tend to smooth out variability in the data and bias *against finding statistically significant results*. See Appendix 1 where I argue that this is not an issue in this study.

Note 11. Purchased from Walls and Associates, the South Dakota data plus data for the other 20 states cost $10,500.

Note 12. S. 106, 2016 Leg. Assembly, 91st Sess. (S. D. 2016) (S. B. 106).

Note 13. For all regressions using NETS data, the panel data specification used a random effects model (hence the Chi square model fit statistic). Regressions using fixed effects specification indicted essentially identical results, although a Hausman test indicated that the random effects models were slightly better. See Ham, Swenson, Imrohoroglu, and Song (20111) for an example of application of the Hausman test.

Note 14. States without a sales tax are Alaska, Delaware, Montana, New Hampshire, and Oregon.

Note 15. Payroll data is not reported in NETS.

Note 16. https://fba.help/list-of-amazon-fulfillment-centers

Note 17. See Petersen (2009) for a discussion of clustering with panel data of financial information.