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Original Paper

Empirical Evidence on the Effects of Wayfair on Brick and Mortar Stores: The Case of South Dakota

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Abstract

Employing very specific data for South Dakota, this study shows that the requirement of collecting a sales tax by remote sellers gave rise to a 6.99% sales increase for brick-and-mortar retail sellers from 2018 through 2022. For this period there was also an approximate 4.54% increase in employment for brick-and-mortar employers. Larger operations, and those parts of a chain, experienced even higher post sales-tax growth. There was a significant amount of net moves out of the state post-2017, but employment losses for such moves were lower for brick-and-mortar retailers.

Keywords

sales tax, South Dakota, empirical estimates

1. Introduction

The sales/use tax landscape changed significantly with a recent Supreme Court decision (Note 1). For remote, out-of-state vendors, this case required that such companies collect sales taxes, regardless if they had a physical presence in South Dakota or not. Quickly almost all states followed this decision, requiring out-of-state vendors who were above sales thresholds to collect such taxes.

Because of an increasing trend towards web-based commerce, the impacts of these law changes promised to be significant. For example, retail e-commerce sales rose from 93% in 2000 to approximately 10.7% in 2019 (Swenson, 2023). Since the majority of these e-commerce-based sales were thought to escape sales taxation, these law changes were hoped to significantly enhance state tax collections (Note 2). Swenson (2023) also found that the states which adopted remote seller sales tax rules in 2018 had substantially more brick-and-mortar sales in 2018-19. Whether this latter trend continued from 2020 through 2022, especially since the pandemic occurred during this time, was not certain. With the recent availability of establishment level data for 2022, I can examine this question using South Dakota data (Note 3). I find that the sales tax requirements for remote sellers resulted in a 6.99% increase in sales for brick-and-mortar retail sellers from 2018 through 2022. Employment gains for such brick-and-mortar sellers for this period were approximately 4.54%. Larger operations, and those part of a chain, experienced even higher post sales-tax growth in sales and employment. There was also a significant number of net moves out of the state post-2017, but employment losses from such moves were lower for brick-and-mortar retailers.

2. Prior Research

The issue here is that since e-commerce sales taxes were paid by consumers after 2017, and the same taxes were paid when shopping brick-and-mortar sales, consumers would return to brick-and-mortar stores for their purchases after 2017 (Note 4). It was far from obvious that customers would return to brick-and-mortar stores, for a number of reasons. One reason is economic scale: pre-tax prices for e-commerce sellers could be lower than those of brick-and-mortar retailers due to the market power of larger companies. A second reason might be due to the forced closure (or lowered selection of goods) of many local stores because they could not compete, pushing consumer shopping with online vendors. Finally, the convenience of online shopping versus traveling to stores (Note 5), especially if a number of stores needed to be visited (Note 6), may have contributed.

The most recent and relevant study on this issue was that of Swenson (2023). He found that the imposition of a sales tax on remote sellers resulted in a 4.8% to 7.2% sales increase for brick-and-mortar retail sellers in 2018 and 2019. For this period there was an approximately 4.3% employment gain for such brick-and-mortar sellers. Swenson (2023) also found that bigger establishments, those part of a publicly-traded company and those part of a national chain, generally had even larger bumps in sales and employment.

Several earlier empirical studies have examined the role of sales tax on e-commerce. Ellison and Ellison (2009) estimated tax-price elasticities of around -10 in their examining of detailed data of a retailer on the sale of computer memory modules. Smith and Brynjolfsson (2001), Anderson et al. (2010) and Goolsbee et al. (2010) also showed relatively high tax sensitivities for specific products (clothing, cigarettes, clothing, etc.). Hortacsu, Martinez-Jerez, and Douglas (2009) estimated decreases in same-state online purchases by 10% or more for each percentage point increase in state sales tax. Einav et al. (2014) found that a one percentage point increase in a state's sales tax increases online purchases by state residents by just under 2%, while also decreasing their online purchases from home-state retailers by 3% to 4%. Goolsbee (2000a, b) estimated that if internet transactions were taxed, up to 24% of online purchasers would not have purchased online. Later studies by Alm and Melnik (2005) and Scanlan (2007) performed a similar exercise using questions in the 2001 Current Population Survey.

Baugh et al. (2018) examined individual transactions and found that households living in taxed states reduced Amazon purchases by 9.4% after sales tax laws were implemented. This implied elasticities ranging from –1.2 to –1.4. They also found that this effect was more significant for bigger purchases, for they estimate a reduction of 29.1% in purchases, which implied an elasticity of –3.9. Baker et al. (2021), showed that shopping behavior responds strongly to changes in sales tax rates. Using comprehensive high-frequency state and local sales tax data, they found that consumers stocked up on storable goods before taxes rose. They also found that consumers 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 to $10.

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% additional firms and establishments and 0.2% higher employment levels. They also estimated that increasing the share of online companies with nexus by 1 percentage point translated into 0.1% additional (small) firms as the sales-tax-collection obligation is dispersed among a larger share of firms. Their simulations, assuming a 50% 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.

In general, the aforementioned studies indicate a substantial tax-elasticity with respect to sales taxes on internet commerce. This, in turn, suggests that purchases may switch to brick-and-mortar stores after 2017. However, this may not be the case if customers simply did not buy an item(s) anywhere after a sales tax was imposed on e-commerce, or bought less items in total on the e-commerce platform, both due to budget constraints. Using data from the National Establishment Time Series (NETS) for South Dakota, this study finds that the imposition of a sales tax on remote sellers resulted in a 6.99% increase in sales for brick-and-mortar retail sellers from 2018 through 2022. Employment gains for such brick-and-mortar sellers for this period were approximately 4.54%. Since over 40% of e-commerce sales are attributable to Amazon, the findings here may have bearing on recent proposals to break up Amazon.

3. Retail Sales Activity Post-Wayfair

As noted in Swenson (2023), the addition of a sales tax to a web based-product's price may or may not result in a shift to brick-and-mortar sales. 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 (the sales tax imposed). Appendix 1 shows the model behind this. It follows that sales revenue collected by both online and brick-and-mortar vendors will be affected by such individual purchases, and we can measure these effects at the aggregate levels for both types of vendors. It should be noted that in a competitive situation, if taxes cause lowered demand, internet-based providers can lower pre-tax prices to be more competitive. The aforementioned model assumes a competitive environment, which may not always be the case (Note 7).

This paper's analyses use precise establishment level data from NETS. The 2022 National Establishment Time-Series (NETS) Database has establishment-level data which not only allow 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 (Note 8). Because the 2022 NETS data is expensive (Note 9), I focus on the state most likely to have shown brick-and-mortar sales increase post Wayfair, i.e., the "bellwether" state where the *Wayfair* decision originated (South Dakota adopted remote seller rules in 2016 (effective largely in 2017) (Note 10). 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 the chosen NETS establishments do not include any distribution centers which might have a significant e-commerce component.

The following four figures show South Dakota sales and employment for both retail brick-and-mortar establishments and for all other establishments.

Figure 1 shows increasing non-retail sales until 2020, after which there is a rapid drop-off.

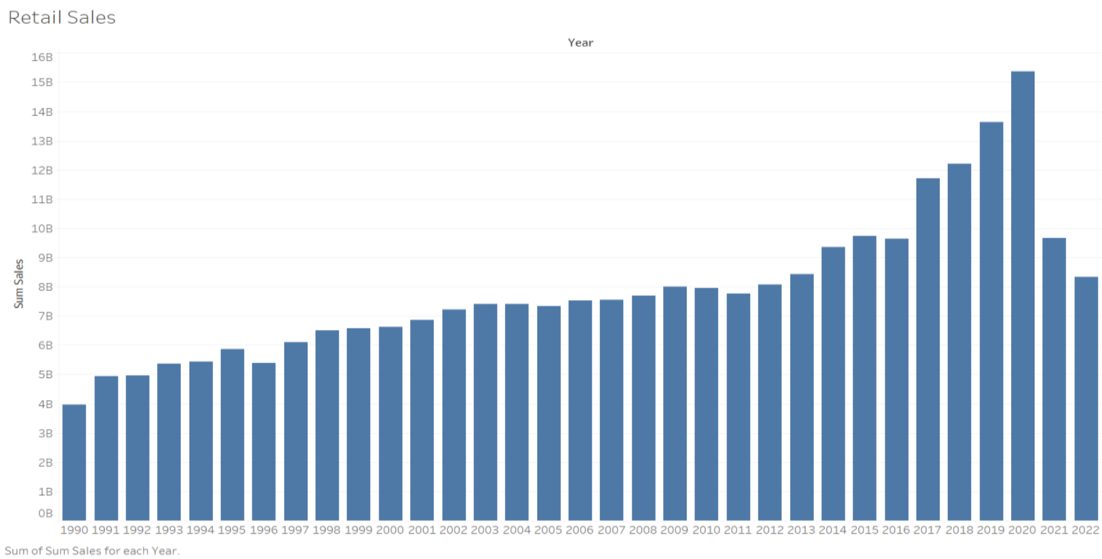

Figure 1. Brick-and-Mortar Retail Sales by Year, South Dakota

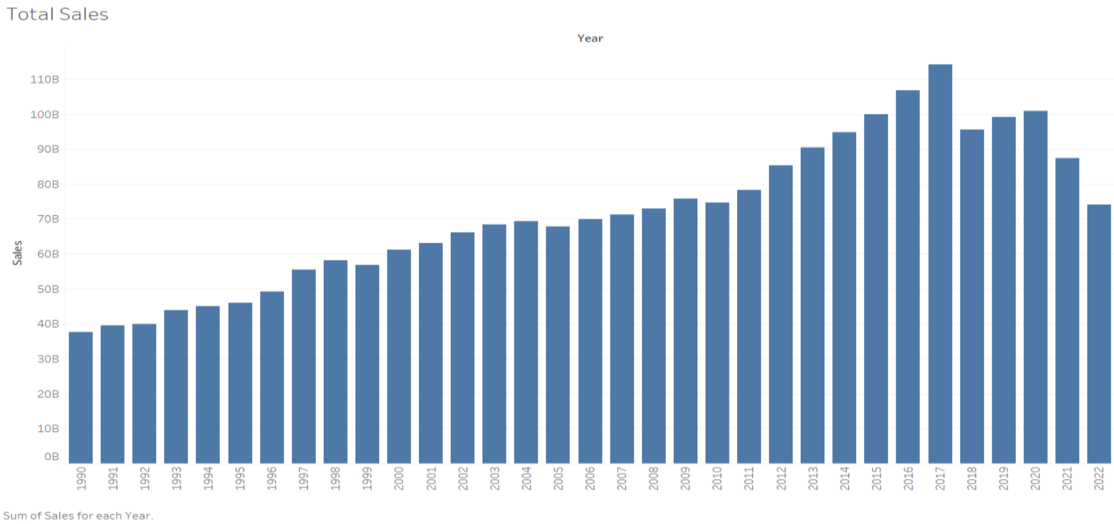

Figure 2. Non-Retail Sales by Year, South Dakota

Figure 2 shows South Dakota sales for brick-and-mortar retailers. As with other South Dakota firms, there is a ramp up until 2020, and a sharp drop thereafter. Figure 3 shows employment for South Dakota non-retail firms. As with sales, there is a ramp up until 2020, followed by a decline. Figure 4 shows South Dakota brick-and-mortar employment; there is an increase until 2020, after which there is a sharp drop-off.

Table 1 shows sales and employment for brick-and-mortar retailers, and all other establishments, for the 5-year period before *Wayfair*, and the 5 years following this case. The table corroborates the visual impressions given by the figures. For both brick-and-mortar retailers and all other companies, there was a post-*Wayfair* decline in both sales and employment. While the decline in employment was roughly equivalent in both types of companies, the effect was more pronounced for brick-and-mortar retailers.

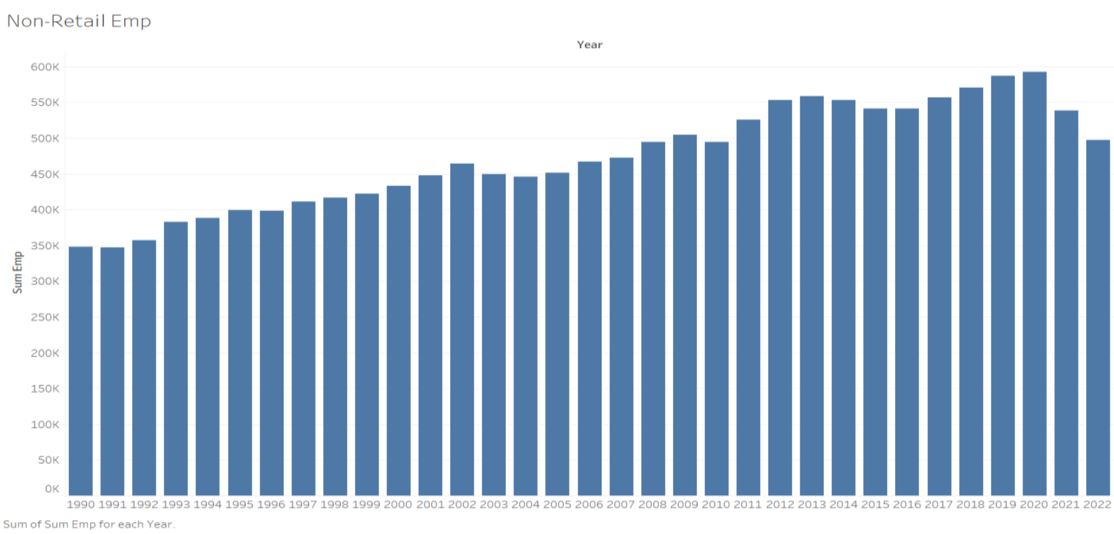

Figure 3. Non-Retail Employment by Year, South Dakota

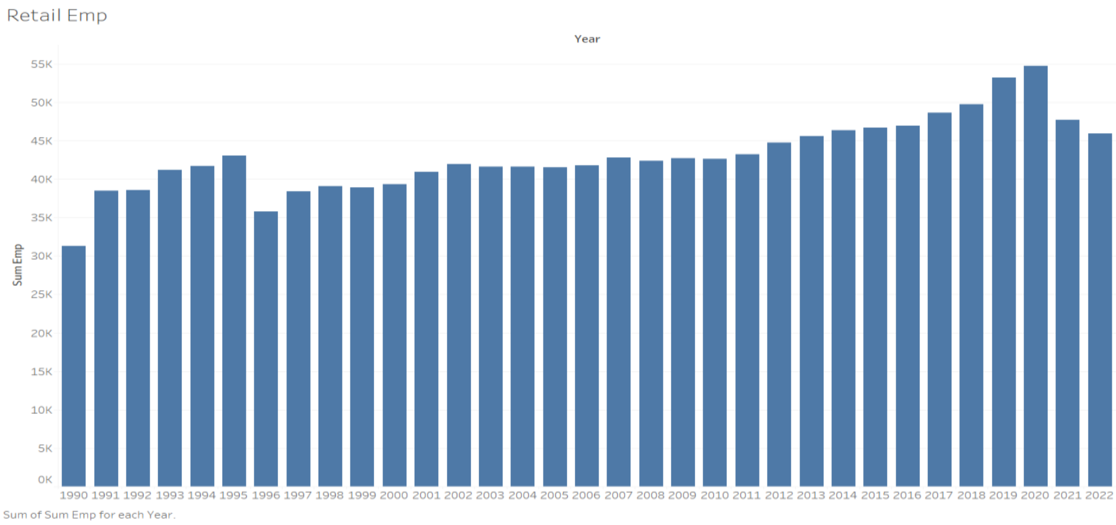

Figure 4. Brick-and-Mortar Retail Employment by Year, South Dakota

Table 1. Changes in Sales and Employment (South Dakota), Brick-and-Mortar Retailers vs. Other Establishments, 1990-2022

		All Others		
	Sales	**Sales Change**	**Employment**	**Employment Change**
2013	81,938,749,971		558,944	
2014	85,455,250,685	0.04	552,803	-0.011
2015	90,195,613,716	0.06	541,475	-0.020
2016	97,090,401,021	0.08	540,854	-0.001
2017	102,437,777,294	0.06	556,253	0.028
2018	83,407,587,982	-0.19	570,021	0.025
2019	85,482,140,549	0.02	587,135	0.030
2020	85,418,027,099	0.00	592,113	0.008
2021	77,646,078,435	-0.09	538,597	-0.090
2022	65,718,784,815	-0.15	497,303	-0.077
	Avg.Change:2013-2017	0.01		0.004
	Avg. Change:2018-2022	-0.08		-0.021
	Difference	-0.09		-0.025
		Brick-And-Mortar Retail		
	Sales	**Sales Change**	**Employment**	**Employment Change**
2013	8,425,716,642		45,566	
2014	9,362,040,487	0.11	46,314	0.016
2015	9,734,189,813	0.04	46,644	0.007

2016	9,630,948,988	-0.01	46,952	0.007
2017	11,703,868,114	0.22	48,619	0.036
2018	12,206,256,149	0.04	49,703	0.022
2019	13,621,616,907	0.12	53,163	0.070
2020	15,358,584,445	0.13	54,687	0.029
2021	9,667,779,826	-0.37	47,729	-0.127
2022	8,326,140,884	-0.14	45,945	-0.037
	Avg.Change:2013-2017	0.09		.01641
	Avg.Change:2018-2022	-0.04		-.00881

While the above data is instructive, to perform more rigorous tests, I also take an econometric (regression) approach. I run a traditional panel data analysis regression model as follows (1990 through 2022):

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

where the dependent variable is the log of sales for that establishment in year t, and we have fixed effects for establishment (ESTAB) and YEAR. In theory, the YEAR dummy variables can control for the effects of the pandemic. SalesTaxRemoteSellerst is set to 1 for brick-and-mortar retailers post 2017, and 0 otherwise. Results are shown in Table 2 (Note 11).

Table 2. Regressions, Establishment Level Annual Sales for South Dakota, 1990-2022

	(1) ln Sales—all establishments	(2) ln Sales with Sales below mean	(3) ln Sales—with Sales above mean	(4) ln Sales—single location	(5) ln Sales—multiple locations
Intercept	11.8567***(.0022)	11.7043*** (.0016)	14.7484*** (.0064)		
Sales Tax on Remote Sellers	.0699*** (.0039)	.0226*** (.0041)	.0883*** (.0062)		
Establishment, Year Fixed Effects	Yes	Yes	Yes		
Chi-squared	320.92***	29.86***	203.01***		
N	2,306,282	2,079,720	226,563		

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

The first column, which uses all establishments, shows that post-*Wayfair* sales for brick-and-mortar establishments increased by 6.99% (significant at.01). The second two columns break out establishments into larger and smaller operations; here, larger establishments increased post-*Wayfair* sales by 8.83% (significant at.01), whereas smaller establishments increased sales by 2.26% (significant at.01).

Table 3 shows panel data regression results for employment using the NETS data for South Dakota. The models are the same shown in (1), except that the dependent variable is the log of employment (Note 12). The first column, which uses all establishments, shows that post-*Wayfair* employment for brick-and-mortar establishments increased by 4.54% (significant at.01). The second two columns break out establishments into larger and smaller operations; here, only larger establishments increased post-*Wayfair* employment (significant at.01).

Table 3. Regressions, Establishment Level Annual Employment for South Dakota, 1990-2022

	(1) ln Employment— all establishments	(2) ln Employment— establishments with Sales below mean	(3) ln Employment— establishments with Sales above mean
Intercept	.8502*** (.0016)	.7531*** (.0014)	2.5820*** (.0074)
Sales Tax on Remote Sellers	.0454*** (.0030)	.0293*** (.0031)	.0065 (.0053)
Establishment, Year Fixed Effects	Yes	Yes	Yes
Chi-squared	225.47***	84.94***	1.48
N	2,369,447	2,105,620	263,828

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

The results show that post-Wayfair employment increased for all establishments. When I run separate regressions for larger versus smaller establishments, only smaller establishments show a statistically significant post-Wayfair employment gain.

4. Additional Analyses—Moves In and Out of the State

If there were significantly positive impacts (or significantly negative) impacts of the tax on brick-and-mortar retailers, we might expect that this might cause firms to move in or out of South Dakota post *Wayfair*. Using the NETS data on moves, Figure 5 shows moves in and out of South Dakota by brick-and-mortar retailers (Note 13). There was an upward trend for outbound moves after 2017 (with the exception of 2019). On the other hand, there was also an upward trend for moves into the state after 2017.

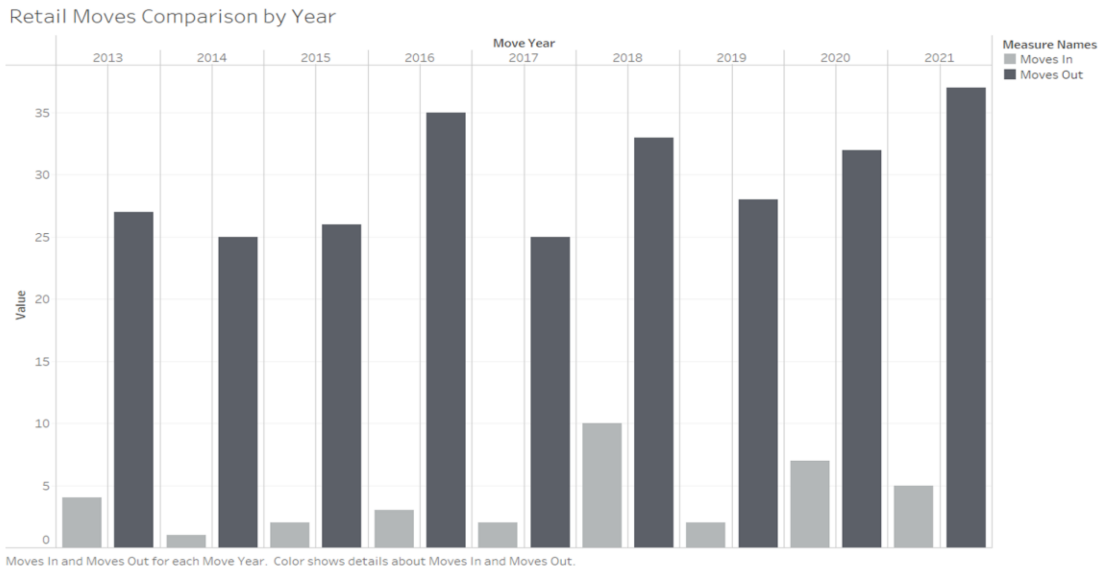

Figure 5. Moves In and Out of South Dakota for Brick and Mortar Retail Establishments

For comparison, Figure 6 reports in and out moving data for all other establishments. As with retailers, there was a general upward trend in both in and out of state moves, post 2017. The above two graphs generally indicate similar patterns in migration for both retailers and other establishments, post 2017.

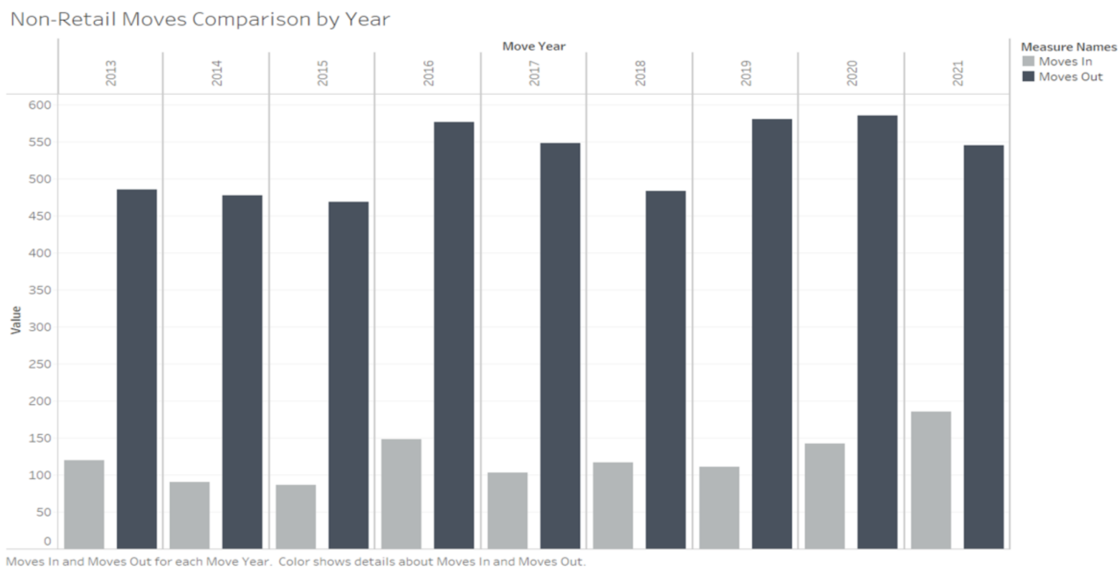

Figure 6. Moves In and Out of South Dakota for All Other Establishments

Table 4 reports net moves in and out of the state, in terms of number of moves, and employees moved, in the 5-year intervals before and after *Wayfair*. As a comparison group, the table also shows similar data for all other South Dakota establishments. Note that all numbers are negative, indicating net moves (and related employment) out of state, for both types of establishments, both pre- and post-*Wayfair*. For number of establishments, brick-and-mortar retailers had a net 20 less establishments move out of state after *Wayfair* (a 15.87% improvement), whereas all other establishments showed an 18.39% improvement. For net loss of employment post *Wayfair*, brick-and-mortar retailers had a 17.77% slowdown in employment loss, whereas all other establishments had a 548.47% increase in employment loss. This latter statistic is consistent with regression results, shown above.

Table 4. Net Number of Establishments Moving, and Net Employment Change From Moves

Net Moves In (Out)			
	2012-2017	2018-2022	Change
All Others	-2007	-1638	+18.39%
Brick-and-Mortar Retail	-126	-106	+15.87%
Net Employment Increase (Decrease)			
	2012-2017	2018-2022	Change
All Others	-2,121	-13,754	-548.47%
Brick-and-Mortar Retail	-1,238	-1,018	+17.77%

5. Limitations

This study has three major limitations. First, there may be a number of state-specific factors (many of which are unobservable) which have an effect on the results, although in theory, the fixed effects models used here can control for these factors. Second, this study only examined one state, so it is possible that the results cannot be generalized to other states.

6. Conclusion

Using a variety of data sources, I find that the imposition of a sales tax on remote sellers resulted in sales increases of 6.99% for brick-and-mortar retail sellers in the 2018- 2022 period. Employment gains for such brick-and-mortar sellers was 4.54%. Post 2017, net out-of-state employment losses were lower for brick-and-mortar retailers than for other establishments. Since there is a trend for consumers to use e-commerce, it seems there is some merit to arguments that the remote seller sales tax brought commerce back to brick-and-mortar sellers. However, it remains to be seen whether consumers' shift back to e-commerce becomes permanent or even increases.

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Appendix 1—Model of consumer choice with sales tax

The following is from my previous paper (Swenson 2023). Consider consumer purchasing behavior in an economy with two competing sellers that sell otherwise identical products, x and y, that are supplied perfectly elastically (Note 14). 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 1 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% increase in p and a 1% 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:

$$\log x(p, \tau) = \alpha + \beta \log p + \theta_\tau \beta \log (1 + \tau) \tag{A1}$$

where θ_τ measures the degree to which the consumer reacts to the tax. Underreaction 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 nonzero. 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 e-commerce 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

$$u_o(x)=v-(p)- s+ W, \tag{A2}$$

$$u_i(y)=v-(p + \tau)- c + Z, \tag{A3}$$

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.

Appendix 2—Estimated Sales and Employment in NETS Data

In estimating our regressions, we need to account for potential measurement errors in the NETS data. Since sales and employment data are 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

$$y' = \alpha + \beta^*X + \mathcal{E}. \quad \text{(A4)}$$

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:

$$v = y - y' = y - v. \quad \text{(A5)}$$

If v has a nonzero 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 α, then substituting y' for (y – v), and rearranging v to the right-hand side yields the following:

$$y - v = \alpha + \beta^*X + \mathcal{E}, \text{ and} \quad \text{(A6)}$$

$$y = \alpha + \beta^*X + (\mathcal{E} + v), \quad \text{(A7)}$$

where (A7) 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 α, OLS yields unbiased coefficient estimates, and standard errors are larger given the increase in the error variance. As noted above, the intercept α is biased if v has a nonzero 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 nonconstant mean, β parameter estimates will be unbiased, but standard errors will be inflated in the case of errors not having a constant mean. Accordingly, the results shown in the tables may actually be conservative in terms of statistical significance.

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 15). 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. While the empirical estimation reported did not cluster, regression results using clusters are available, and show very similar results.

Notes

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

Note 2. Indeed, Fox, Hargaden, and Luna (2021) using state-level data, found increased sales tax revenues by 5.4%, 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.

Note 3. The NETS data, which I use here, is quite expensive, therefore I focus on the state most likely to have a quick reaction to *Wayfair*—South Dakota. (I assume that you are referring to the case *Wayfair*…)

Note 4. Indeed, the increasing competitive threat from online retailers for traditional retailers was the impetus for proposed law such as the *Main Street Fairness Act* ; 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 5. In contrast, marketplace facilitators such as Amazon offered the convenience of doorstep delivery from a variety of vendors.

Note 6. 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 7. For example, the largest internet-based seller (Amazon) provides a significant amount of goods and might be considered a monopsonist in its industry.

Note 8. 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 2022 national NETS Database includes an annual time-series of information on over 37 million U.S. establishments from 1990 to 2022. 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.

Note 9. Purchased from Walls and Associates, the South Dakota data plus data for the other 20 states cost $10,500.

Note 10. S. 106, 2016 Leg. Assembly, 91st Sess. (S. D. 2016) (S. B. 106).

Note 11. Because the NETS data is survey-based, some measurement error is possible in both sales and employment. Appendix 2 explains why this is not likely a problem for our hypotheses tests.

Note 12. Payroll data is not reported in NETS.

Note 13. While the NETS data has data through 2022, moving data is only available through 2021.

Note 14. 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 noncompetitive environment.

Note 15. See Petersen (2009) for a discussion of clustering with panel data of financial information.