THE EFFECT OF SALES TAXES ON LOCATION DECISIONS AND CAPITAL MARKET OUTCOMES: EVIDENCE FROM WAYFAIR

By

Joanna Shaw

A DISSERTATION

Submitted to
Michigan State University
in partial fulfillment of the requirements
for the degree of

Business Administration—Accounting—Doctor of Philosophy

2022

ABSTRACT

THE EFFECT OF SALES TAXES ON LOCATION DECISIONS AND CAPITAL MARKET OUTCOMES: EVIDENCE FROM WAYFAIR

By

Joanna Shaw

This study examines how investors, analysts, and firms respond to changes in sales tax obligations for e-commerce firms. Previous research finds that e-retailers have a competitive advantage over their brick-and-mortar counterparts due to their ability to avoid collecting sales taxes. Using the Supreme Court of the United States (The Court) decision in *South Dakota v. Wayfair, Inc., 585 U.S.__ (2011) (Wayfair)* as a setting, I examine investor and analyst reactions to, and corporate location decisions following, the broadening of state nexus rules for interstate commerce. I find that investors and analysts responded negatively to the announcement that The Court would hear the *Wayfair* case and to the release of the ruling, as evidenced by negative mean cumulative abnormal returns and downward revenue forecast revisions, respectively. These results are consistent with capital market participants perceiving increased sales tax obligations as being detrimental to e-commerce firms. Next, I investigate how e-commerce firms respond to increased sales tax obligations by examining their location decision-making in the wake of *Wayfair*. I find that e-commerce firms are more likely to establish locations in new states with sales taxes after *Wayfair*. In cross-sectional analysis, I find that my results are primarily driven by retail, wholesale, and service firms. I also find evidence that e-commerce firms are more likely to establish locations in states with high populations after *Wayfair*.

This dissertation would not have been possible without the support of my family, and is dedicated to the memory of my father, Michael U. Thibodeau.
I made it Dad.

ACKNOWLEDGEMENTS

I am thankful for the support and guidance of my Dissertation Chair Michelle Nessa, and my committee members, Kevin Markle, Dan Lynch, Isabel Wang, and Leslie Papke. I am also grateful for the helpful comments and guidance from Sanjay Gupta and Wayne Nesbitt, as well as those from Michigan State faculty and my fellow Ph.D. students. This work was supported through computational resources provided by the Institute for Cyber-Enabled Research at Michigan State University. DataAxle is the provider of the Licensed Database used to create the Youreconomy Time Series (YTS). This work/research was authorized to use YTS through the Business Dynamics Research Consortium (BDRC) by the University of Wisconsin's Institute for Business and Entrepreneurship. Finally, I gratefully acknowledge financial support from the Eli Broad College of Business at Michigan State University and the generous support from the Grant Thornton Doctoral Fellowship. All errors are my own.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

1. INTRODUCTION

This paper investigates how capital market participants and firms respond to a significant U.S. sales tax policy shift. Using the Supreme Court of the United States (The Court) decision in *South Dakota v. Wayfair, Inc., 585 U.S.__ (2011) (Wayfair)* as a setting, I examine investor and analyst reactions to, and corporate location decisions following, the broadening of state nexus rules for interstate commerce. *Wayfair* overturned 50 years of precedent by eliminating the "physical presence" requirement that had been the deciding factor in determining state-level nexus.[1] The *Wayfair* ruling created a consistent sales tax regime for all firms, regardless of their location, by establishing a new economic nexus standard based on the volume of sales transactions in a jurisdiction.[2] The ruling is expected to substantially increase state and local sales tax revenues. Estimates suggest that states lost up to 25% of e-commerce-related sales taxes before *Wayfair* due to states' inability to impose sales tax collection responsibilities on out-of-state sellers (Agarwal et al., 2017; Fox et al., 2014). This monumental shift in policy has broad financial and strategic implications for firms and capital market participants.

Before *Wayfair*, e-commerce companies who made sales to customers in a state where they had no physical presence had no obligation to collect and remit sales taxes.[3] Previous research finds that consumers are sensitive to sales taxes and alter their purchasing behavior to

[1] Prior to *Wayfair*, states and the courts relied on the precedent found in *Quill Corp v. North Dakota*, 504 U.S. 298 (1992), (*Quill*), which held that businesses must have a physical presence within the state to have sales tax nexus. Sales tax nexus refers to a connection between businesses and a state that requires the seller to register for, and then collect and remit, sales taxes (Avalara 2021).

[2] In *Wayfair*, The Court affirmed South Dakota's definition of economic nexus, which is established when a seller has $100,000 or more in sales, or 200 transactions within a state, regardless of the seller's physical location.

[3] I refer to firms operating primarily via the internet within a state as "e-commerce" or "online" firms. The term "brick-and-mortar" refers to businesses with a physical location within a state. Previous research has focused specifically on the retail sector and used the terminology of "remote sellers" and "e-retailers." This study incorporates all e-commerce firms including those in the financial and services sectors.

avoid them (Alm & Melnik, 2012; Anderson et al., 2010; Baker et al., 2021; Baugh et al., 2018; Hoopes et al., 2016). Thus, under the previous sales tax rules, online firms had a competitive pricing advantage over their brick-and-mortar counterparts (Hoopes et al., 2016). Additionally, e-commerce firms also avoided the compliance burdens associated with collecting and remitting sales taxes (Baugh et al., 2018; Hoopes et al., 2016). Complying with sales tax obligations is complex and requires considerable investment in technology and services (Cohn, 2021; McLoughlin, 2020). As a result of *Wayfair*, e-commerce firms face increased sales tax obligations, and need to assess possible exposure to back taxes, interest, and penalties (Jensen et al. 2019). While the *Wayfair* ruling bars states from seeking retroactive sales taxes, there is nothing to prevent them from auditing newly registered firms for past compliance with other state taxes (i.e., income taxes, payroll taxes, unemployment taxes).[4] Therefore, the *Wayfair* decision will impact firm profitability through increased costs and potential liabilities from prior periods.

Previous research has investigated the impact of the physical presence standard on e-commerce firms and their location decisions. Arya and Mittendorf (2018) develop a theoretical model where an online firm weighs the prospect of expanding its market share through new customers at a brick-and-mortar store against the frictions associated with the costs of sales taxes. Empirical research supports the external validity of their model, finding that higher sales tax rates and broader sales tax bases are associated with a reduced likelihood of firms establishing a physical presence in large states (Beem & Bruce, 2021; Bruce et al., 2015). A high-profile example of this friction is evident in the business practices of Amazon. Amazon's founder, Jeff Bezos, chose to headquarter his new company in the state of Washington instead of

[4] For example, Wells Fargo increased its state tax reserve by $481 million due to the *Wayfair* ruling (Rapaport, 2018).

California due to its relatively low population, and to avoid collecting sales taxes in more populous states (i.e., California, New York, Texas) (Stone, 2013).

In a competitive marketplace, the loss of pricing advantage and threat of increased costs will lead both firms and capital market participants to revise expectations of the future profits of e-commerce firms. I expect investors in, and analysts of e-commerce companies to react negatively to events leading up to and including the *Wayfair* ruling. These events would have increased investor perceptions of the likelihood of changes to sales tax nexus rules that would have a negative impact on e-commerce firms. Additionally, because *Wayfair* eliminates the sales tax frictions associated with physical location decisions, I hypothesize that firms will respond to *Wayfair* by establishing physical locations in new states with sales taxes. These new locations will allow e-commerce firms to offset post-*Wayfair* costs with revenue from new customers and protect their market share by providing more timely shipping and other services (e.g., installation).

To test my predictions, I begin by constructing a sample of firms impacted by *Wayfair*. I search all public filings in the Securities and Exchange Commission's (SEC) EDGAR database during the period January 1, 2018, through March 31, 2020, for mention of the *Wayfair* ruling. I examine each document mentioning *Wayfair* to confirm it relates to increased firm risks associated with sales tax nexus changes. In contrast to other studies on the effects of sales taxes on e-commerce firms, I do not focus solely on e-commerce retailers (Bruce et al., 2015; Hoopes et al., 2016; Luchs-Nunez, 2022) because the *Wayfair* ruling has implications for all firms involved in interstate commerce. Thus, I include all firms identified through my SEC filings review. Through this process, I identify 170 treated firms from 29 industries for use in my analyses.

To measure investor reactions to changes in sales tax nexus rules, I examine three separate events related to *Wayfair*. The first event date is January 12, 2018, the date The Court agreed to hear *South Dakota v. Wayfair* and signaled it was open to revisiting the legal precedent established in *Quill* (Rubin, 2018). The fact that The Court agreed to hear the case is remarkable for two main reasons. First, The Court only grants requests to appeal a lower court verdict for less than 1% of the petitions it receives in any given year (www.supremecourt.gov, n.d.) and of those granted, very few are tax cases (Roberson & Spencer, 2022). Second, when making decisions, The Court generally follows prior legal precedent, known as the judicial doctrine of *stare decisis*. *Stare decisis* is at the core of U.S. Common law and ensures stability and uniformity in the judicial system (Lin et al., 2020). The Court has only deviated from this doctrine 233 times since its inception in 1789 (Library of Congress). By granting South Dakota's appeal, The Court signaled it was open to the possibility of establishing a new legal precedent.

The next event date is the hearing of oral arguments on April 17, 2018. Oral arguments allow members of The Court to ask questions of both parties to the litigation, and often these questions provide insight into the various leanings of the Justices[5]. Based on the questions posed by the Justices, The Court appeared to be strongly divided in the case, and it was unclear after oral arguments how a majority of The Court would rule (Bravin, 2018; Horowitz, 2018; Prete, 2018; Reichenberg-Sherr, 2018).

[5] Oral arguments provide an opportunity for both sides of an argument to present their case to The Court and respond to direct questioning from the Justices. The questions asked by the Justices are closely scrutinized by the media, legal scholars, and other interested parties for insights they may provide into how The Court may be leaning. In *Wayfair*, questions posed by some Justices seemed to indicate a preference for leaving the internet sales tax issues to Congress. Other Justices disagreed and argued that it was The Court that created the previous nexus standard, and it was The Court's responsibility to correct and update the standard.

The final event date is June 18, 2018, when The Court affirmed South Dakota's definition of economic nexus in a 5-4 decision.[6] This decision lays the groundwork for a definition of economic nexus that can be adopted by every state, thereby creating a multitude of challenges for firms involved in e-commerce.

To test how investors perceive changes in the sales tax nexus standard, I calculate one and five-day windows of mean cumulative abnormal returns (*CAARs*) beginning on January 12, 2018, using an equally-weighted market index for my sample of treated firms.[7] I find negative and significant *CAARs* for both event windows (p-values <0.01). This is consistent with investors believing that The Court agreeing to hear the case increased the likelihood of changes to existing sales tax nexus rules that would negatively impact e-commerce firms. The results are also economically meaningful, with *CAARs* of -1.46% (one-day window) and -2.39%, (five-day window).

Next, I examine market reactions around April 17, 2018, when The Court heard oral arguments. I find positive and significant *CAARs* (p-values <0.01) for the one and five-day windows beginning on April 17, 2018. This is consistent with investors revising their expectations that The Court would overturn the precedent in *Quill*. These results are also economically significant, with *CAARs* of 0.94% (one-day window) and 1.81% (five-day window).

Finally, I examine the date of the *Wayfair* ruling, June 21, 2018. I find negative and significant *CAAR*s in all event windows (p-values<.01) beginning on June 21, 2018. The *CAAR*s

[6] After oral arguments it was unclear how The Court would rule in *Wayfair*, and therefore the decision was a surprise. Another surprise was how the ideologically conservative and liberal Justices ruled on both sides of the argument. The Justices in favor of adopting economic nexus included Thomas, Alito, and Gorsuch (conservative) and Justices Ginsburg and Kennedy (liberal). Chief Justice Roberts (conservative) wrote the dissenting opinion, and represented the views of Justices Breyer, Sotomayor, and Kagan (liberal).

[7] I use an equally-weighted market index to calculate my abnormal returns to ensure that a single large firm is not responsible for my results. The results are consistent when I use a value-weighted market index.

are again economically significant at -1.09% (one-day window) and -2.24% (five-day window). These results demonstrate that investors expected negative repercussions for e-commerce firms from the broadening of state nexus rules.

I further examine capital market participants' responses to *Wayfair* events by investigating changes in analysts' revenue forecasts. I expect analyst reactions to follow the same pattern as those of investors, consistent with analysts responding negatively to the threats posed by the *Wayfair* decision to e-commerce firms. I use a difference-in-differences research design with firms affected by *Wayfair* as the treatment sample and all other firms matched through COMPUSTAT, IBES and CRSP as the control firms. After entropy balancing, I examine the effect of the interaction between treated firms ($TREATED_i$) and specific *Wayfair* event dates ($WAYFAIREVENT_t$) on the change in analyst revenue forecasts. I find negative and significant coefficients on the interaction terms when The Court agreed to hear the *Wayfair* case (January 12, 2018) and when the ruling was released (June 21, 2018). Unlike my investor-level tests, I fail to find a statistically significant difference between my treatment and control groups when The Court heard oral arguments on April 17, 2018. These results support my prediction that analysts viewed the changes in *Wayfair* as having a negative impact on e-commerce firms.

Because *Wayfair* eliminates the sales tax frictions associated with physical locations in states with sales taxes, I predict that firms will react to *Wayfair* by establishing locations in new states with state-level sales taxes. To test my prediction regarding the impact of *Wayfair* on firms' location decisions, I examine whether treated firms opened new physical locations in new taxing jurisdictions after the *Wayfair* ruling, controlling for firm-level characteristics. I find statistically significant (p-value<.10) evidence that treated firms increased the number of states with sales taxes where they are located after *Wayfair*. I then examine the number of new states

with sales taxes where firms have locations over my panel time series (2016-2019) and determine whether treated firms are more likely to add new state locations in taxable states after *Wayfair*. I find statistically significant evidence (p-value<.10) that treated firms are 85.98% more likely to open a new location in a new state with a state-level sales tax in the fiscal years after the *Wayfair* ruling compared to control firms. These results provide empirical evidence supporting the theoretical framework outlined by Arya and Mittendorf (2018), who hypothesize that e-commerce firms weigh the benefits of adding new customers against the costs of collecting and remitting sales taxes when deciding whether to open a physical location. The increased number of new states for treated firms provides evidence that e-commerce firms had limited their exposure to states with sales taxes before the *Wayfair* ruling.

In cross-sectional analysis, I find that my results for new state locations are driven primarily by firms in the retail, wholesale, and service industries. I also find evidence that e-commerce firms are more likely to establish locations in states with high populations after *Wayfair*.

This study contributes important empirical evidence on how capital market participants and firms respond to changes in sales taxes and answers the call in Dyreng and Maydew (2018) for researchers to provide insights into the effects of taxes other than income taxes. Sales taxes are often overlooked in accounting academic studies of corporate tax because they are not a direct tax on corporations.[8] However, these taxes are ubiquitous and their effects are significant,

[8] The statutory incidence of sales taxes is on the buyer, with sellers responsible for the collection and remittance of the sales taxes to states; therefore, sales taxes are often considered a "pass-through" tax at the seller level. However, the actual tax burden (economic incidence) is shared with parties who are not statutorily assigned to pay the tax unless demand is perfectly inelastic. In the case of sales taxes, the seller's share of the economic incidence varies depending on the price elasticity of demand for their product or service.

and therefore understanding how these indirect taxes impact capital market participants and firm decision making is important.

As such, this is not the only study to examine the impact of the *Wayfair* ruling on investors and firms. In a concurrent study, Kubick, Omer and Yazzie (2021) examine stock price reactions around the *Wayfair* event dates (January 12, 2018, April 17, 2018, June 21, 2018) and find that investor estimation of firm value decreased after each event. The authors then examine what firm and industry characteristics best explain the results from their examination of stock prices. They find that market reactions were more negative for smaller firms, younger firms and more profitable firms.

In an additional concurrent study, Luchs-Nunez (2022) investigates how investors, analysts and firms respond to the *Wayfair* decision. The author limits their analysis to retail and wholesale firms, and finds that investors respond negatively to *Wayfair* events, and that analysts' revenue forecasts decrease for treated firms after the ruling. The author also examines the effect of *Wayfair* on firm location decisions using the firm-state research design from Cen et al. (2017) for their primary analysis. The analysis again focuses on retail and wholesale firms, includes both public and private firms and does not include firm-level financial statement control variables. The author fails to find strong, consistent evidence that firms increase their number of state locations after the *Wayfair* ruling. While my results are consistent with the investor and analyst responses of these concurrent works, as a result of my sample construction and the inclusion of firm level financial control variables I find empirical firm-state level evidence that firms are more likely to expand into new states when sales tax frictions are removed.

This study also provides insights into the effects of sales taxes on a broad sample of firms. Prior research showing that online firms have a pricing advantage over their brick-and-

mortar counterparts focuses only on retailers (Hoopes et al., 2016; Luchs-Nunez, 2022). The *Wayfair* decision has ramifications for other sectors of the economy that operate in jurisdictions without a physical presence, such as the financial and services sectors (BDO 2018). The treated firms in my study include manufacturers, wholesalers, and service firms that are also impacted by *Wayfair*. As such, this study sheds light on the effects of sales taxes on a wider set of firms and therefore provides insights that generalize to a larger portion of the economy.

Finally, this study is informative to federal lawmakers and regulators as they debate legislation that would negate or alter the impact of the *Wayfair* decision. Immediately after the *Wayfair* decision was announced, four separate pieces of legislation were introduced in Congress that, if enacted, would reverse, in whole or in part, the application of nexus to online sales.[9] The effects of *Wayfair* extend beyond sales taxes, as many states have adopted the economic nexus definition approved in *Wayfair* for income taxes. On July 2, 2019, the State of Hawaii was the first to enact an economic income tax nexus standard, with Washington, Oregon, Massachusetts, Texas, Pennsylvania, and California quickly following suit. When evaluating any new policy or law, regulators need to understand the implications of policies currently in existence. This study aids lawmakers in their understanding of how the *Wayfair* ruling impacted corporate decision-making and the resulting economic activity, including unforeseen costs and benefits of the ruling.

[9] The four bills were the Protecting Businesses from Burdensome Compliance Cost Act (H.R. 379), Stop Taxing Our Potential Act (S. 128) No Retroactive Online Taxation Act (H.R. 7184), and the Online Sales Simplicity and Small Business Relief Act (H.R. 1933). The House Small Business Subcommittee on Economic Growth, Tax and Capital Access held hearings on July 24, 2018, March 3, 2020, and on May 20, 2021. The Senate Finance committee also held a hearing June 14, 2022.

2. INSTITUTIONAL BACKGROUND AND RELATED LITERATURE

2.1 Background on Wayfair

Sales taxes are a form of consumption tax. Consumption taxes are generally collected at the point of sale, have various names (e.g., ad valorem, excise, etc.), and are a percentage of the value of the item sold. The statutory incidence of sales taxes is generally on the purchaser (Fullerton & Metcalf, 2002). Governments administer the tax by requiring merchants to collect and remit the taxes on their customers' behalf at the point of sale. When demand is elastic, this requirement can create price distortions and indirect taxes on the sales merchants.

Before *Wayfair*, the last major Supreme Court decision involving state sales tax nexus was *Quill Corp v. North Dakota*, 504 U.S. 298 (1992).[10] The decision in *Quill* held that contact with a state via mail or common carrier was insufficient to establish a "substantial nexus." The *Quill* decision affirmed the "physical presence" standard adopted in *National Bellas Hess, Inc. v. Department of Revenue of Illinois,* 386 U.S. 753 (1967), ruling that a business must have a physical presence in a state before the state can require the company to collect sales tax.

The growth of the internet has allowed businesses to use new e-commerce channels that allow for geographic expansion without deploying new physical capital. By 2007 state and local governments were losing over $7.2 billion annually in uncollected sales and use tax revenues on e-commerce transactions (Bruce et al., 2009). In 2008, many states attempted to overcome restrictions imposed by *Quill* by enacting "click-through nexus" laws that specifically targeted large e-commerce sellers such as Amazon (Rosenthal et al., 2017). The state of New York was

[10] Quill is a company that sells items via catalog and is based in Delaware. In *Quill,* the Supreme Court held that the Dormant Commerce Clause prohibits a state from imposing the duty of sales and use tax collection on companies that do not have substantial nexus within the state, and whose contact is limited to remote selling with mail or common carrier delivery. The Dormant Commerce Clause is a legal doctrine that is meant to prohibit states from enacting any legislation that would discriminate or unduly burden interstate commerce. This doctrine infers its authority from the Commerce Clause of the United States Constitution and is meant to fill in the gap where the explicit Commerce Clause is silent.

the first to adopt a click-through nexus law. The New York statute asserted that a firm had a physical presence in New York if they entered into a contract with a New York resident to direct sales to the firm's website. The statute specifically referenced digital advertising and website links as items that would establish nexus for sales and use tax purposes. Many of these laws faced significant legal challenges and were subsequently deemed unconstitutional by the courts.[11] Legal scholars, however, began to question whether *Quill* remained relevant in the era of e-commerce. Supreme Court Justice Anthony Kennedy expressed support for such an argument in an opinion written in *Direct Marketing Ass'n v Brohl,* 575 U.S. __ (2015).[12]

> *"There is a powerful case to be made that a retailer doing extensive business within a State has a sufficiently 'substantial nexus' to justify imposing some minor tax-collection duty, even if that business is done through the mail or the internet….This argument has grown stronger and the cause more urgent with time."*

In response to Justice Kennedy's opinion, thirteen states passed "Kill Quill" legislation, that created new nexus standards in direct violation of *Quill*, in the hopes of inducing litigation that The Court would ultimately resolve (Bannasch, 2018; Chamseddine, 2018; Liptak, 2015). South Dakota was one of the first states to pass such legislation. Its statute created an "economic nexus" definition that required remote sellers to collect and remit sales tax in South Dakota if they had annual sales transactions that exceeded $100,000, or more than 200 total transactions within the state, regardless of the sellers' physical location. Following the passage of the legislation, the South Dakota Department of Revenue filed suit against online retailers Wayfair, Inc, Newegg Inc., Overstock.com Inc., and Systemax Inc. to compel the companies to collect

[11] For example, *Performance Marketing Ass'n v. Hammer*, No. 2011 CH 26333 (Supreme Court of Illinois, October 18, 2013) was a challenge to the Illinois Public Act 96-1544 click-through nexus law. The Supreme Court of Illinois found in favor of the plaintiff and ruled the Illinois law violated the Commerce Clause of the Constitution of the United States. In *Direct Mktg. Ass'n v. Huber*, Civil Case No. 10-cv-01546-REB-CBS, (U.S. Dist. Colo. March 30, 2012) the U.S. District Court for the District of Colorado found the Colorado click-through nexus law in violation of the Commerce Clause of the United States Constitution.

[12] In *Direct Marketing Ass'n v Brohl,* 575 U.S. __ (2015), The Court addressed use tax reporting by entities that purchased items via the internet from sellers who did not collect and remit sales tax.

sales tax in compliance with the new state law. The state of South Dakota admitted in its court filings that its new legislation was unconstitutional, and its objective was to advance the case to The Court to overturn the physical presence standard of *Quill* (*South Dakota v. Wayfair, Inc., et al.*, D.S.D. No. 3-16-CV-03019-RAL; Egerwels 2016). On October 2, 2017, South Dakota appealed the case to The Court. Thirty-five states signed an amicus brief supporting the appeal from South Dakota.

On January 12, 2018, The Court granted South Dakota's request to hear the case during its 2017-2018 calendar session.[13] The Court heard oral arguments on April 17, 2018, and the questions posed by the justices indicated no clear consensus on how The Court would rule (Bravin, 2018; Cullers, 2018; Horowitz, 2018; Reichenberg-Sherr, 2018; Yetter, 2018). On June 21, 2018, The Court issued a 5-4 decision in favor of South Dakota.[14] The majority held that "the physical presence rule of *Quill* is unsound and incorrect" and that the South Dakota law does not create an undue burden to interstate commerce (Kennedy, 2018). This decision is especially important for e-commerce firms, as it eliminates the pricing advantage that came with exemption from collecting sales taxes.

2.2 Related Literature

2.2.1 Tax Incidence, Compliance Burdens, and Tax Sensitivity

Tax theory emphasizes that tax minimization alone should not motivate business decisions (Erickson et al., 2020). Effective corporate strategy must consider all explicit and implicit taxes when making decisions, and reducing taxes cannot come at the cost of operational

[13] The Supreme Court Calendar begins in October of every year and ends at the end of June. If The Court agrees to hear a case in a calendar session, it is expected to issue a ruling in that case before the term of the session ends.
[14] The makeup of those on the majority is notable, as both conservatives and liberal justices were split on the issue of overturning *Quill*. Joining Kennedy in the majority were Justices Thomas, Alito, Ginsberg, and Gorsuch. In the minority were Chief Justice Roberts, Breyer, Sotomayor, and Kagan, who felt that *stare decisis* required The Court to refrain from overruling *Quill*, and leave it to Congress to enact legislation to address e-commerce concerns.

effectiveness.[15] For example, tax rules can impact the before-tax rate of return due to geographic and tax incentive differences, thereby creating an implicit tax that the company bears. The goal is always to maximize after-tax returns, not merely reduce tax payments.

Economic theory predicts that a change to taxes will alter the market equilibrium (Kotlikoff and Summers 1987). Because the statutory incidence of sales tax is on the consumer, sales taxes are pass-through in nature. However, the economic incidence of a tax is often different from its statutory incidence. The level of competition, marginal cost structure, and supply curves influence the tax burden distribution and the extent to which tax burdens are ultimately shared between producers and consumers (Fullerton & Metcalf, 2002; Hayes, 1921). When consumer demand for a product is more elastic, the producer bears more of the tax burden. Only if consumer demand is perfectly inelastic will the entire tax cost be paid by the consumer. Therefore, in most cases, firms bear at least some of the tax cost (Fullerton & Metcalf, 2002).

Multiple studies examine the tax incidence of corporate income taxes and find that they are at least partially passed through to consumers and labor participants (Dyreng et al., 2020; Jacob et al., 2022; Suárez Serrato & Zidar, 2016). The findings regarding the tax incidence of sales taxes are more nuanced. Poterba (1996) examines sale taxes on clothing during two time periods (1925-1939 and 1947-1977) and finds that sellers partially paid sales taxes (i.e., under shifted) during the depression, however, in the post-war period beginning in 1947 he finds that sellers were able to pass on sales taxes entirely to consumers (i.e., fully shifted). Poterba (1996) concludes that consumer prices rise by the amount of sales tax only in specialized circumstances.

[15] Implicit taxes are a reduction in pre-tax rates of return due to a government policy and involve payments between private parties (Erickson et al., 2020; Weisbach, 1999). While the classic example of an implicit tax is the reduced yield on tax-favored municipal bonds, there are additional studies that investigate business-level implicit tax effects such as those involved with transfer pricing and market competition (Hopland et al., 2018; Jennings et al., 2012; Markle et al., 2020) .

Alm, Sennoga and Skidmore (2009) examine the tax incidence of gasoline excise taxes and find that these taxes fully shift to the consumer only in competitive markets. In contrast, Yilmazkuday (2017) finds that gasoline tax increases and decreases are under shifted to consumers. Overall, these studies find that the incidence of sales taxes depends on the level of competition in the marketplace. Only when markets are perfectly competitive can suppliers fully shift the sales tax burden to their customers.

Even if companies can fully shift sales taxes to consumers, they still face compliance costs from sales tax collection and remittance requirements. The costs of complying with collection, remittance and reporting are significant, and increase with firm size and complexity (Slemrod and Venkatesh 2002; Gupta and Mills 2003). Cline and Neubig (2000) find that sales and use taxes impose a significant compliance burden on retailers, and that multi-state retailers bear higher compliance costs than single state sellers. The authors note that there are thousands of separate sales taxing jurisdictions within the United States, each with their own set of rules and remittance requirements. This creates significant uncertainty related to audit and litigation risk borne by the company, as noted by The Court's dissenting justices.[16]

Prior to *Wayfair*, e-commerce firms were able to use their narrower sales tax compliance requirements to their advantage. Hoopes et al (2016) examine stock market reactions and analyst forecast revisions around the introduction of federal legislative proposals that would have allowed states to collect sales taxes from remote sellers. They find evidence of negative stock market reactions and downward revisions of analyst sales forecasts after the introduction of the

[16] In his dissenting opinion in *Wayfair*, Chief Justice John Roberts notes: "Correctly calculating and remitting sales taxes on all e-commerce sales will likely prove baffling for many retailers. Over 10,000 jurisdictions levy sales taxes, each with 'different tax rates, different rules governing tax-exempt goods and services, different product category definitions, and different standards for determining whether an out-of-state seller has a substantial presence' in the jurisdiction." *South Dakota v. Wayfair, Inc., 585 U.S.__ (2018) (Roberts J. dissenting opinion)*

federal legislation. They conclude that such legislation is perceived negatively by investors and analysts and is a threat to e-commerce firms. Hoopes et al (2016) note that when firms are not required to collect sales taxes, they have a superior pricing advantage because they can offer their products at a lower net cost.

The findings in Hoopes et al. (2016) are consistent with other studies that examine consumer behavior in the presence of sales taxes. Chetty, Looney and Kroft (2009) use a field experiment in a grocery store to examine how consumers respond to sale taxes when the taxes are made more salient. Chetty et al. (2009) establish two separate conditions for their analysis. In the first condition, sales taxes are presented to the consumer at the shelf, with the price of an item displayed along with the sales tax on that item. The second condition conforms to the usual practice of tax being calculated and added at the cash register. The authors find that consumers reduce their spending by 8% when the tax is presented to the customer at the shelf (i.e., made more salient). Miller and Omartian (2021) use mobile device spatial data to examine foot traffic at brick-and-mortar stores in localities that have increased their sales taxes. They find that a 1% increase in sales taxes leads to a 1 – 1.5% decrease in store visits. Baker et al. (2021) use detailed household spending data and tax data to examine how households respond to changes in sales taxes. They find that households alter their spending habits by stocking up on items prior to the tax increase and switching to online purchasing to avoid taxation. Baugh et al. (2018) examine changes in consumer purchasing in states that have implemented a click-through nexus sales tax statute. They find that consumers in these states reduced their Amazon purchases by 9.4% following the implementation of the tax. The results of these studies demonstrate that consumers are sensitive to sales taxes and, in the absence of perfect competition, alter their purchasing habits to avoid them.

2.2.2 Physical Presence Decisions

Establishing a physical presence in a state is a strategic choice of multistate entities where net present value calculations weigh potential new income streams against required capital investments. Laroche, Yang, McDougall, and Bergeron (2005) examine the risks associated with distribution methods that lack a physical presence (i.e., online). They develop a model of consumer decision making that considers the evaluation difficulty and inherent risk associated with online purchases. Using multiple survey instruments, they find that consumers perceive online purchases to be harder to evaluate and to carry significantly more risk than brick-and mortar purchases. Unlike brick-and-mortar purchases, where consumers can examine items closely before buying, the quality and fit of online purchases are less certain, and therefore may be less appealing to customers.

To overcome online selling challenges, firms may choose to complement their online sales with brick-and-mortar storefronts. Pauwels and Neslin (2015) find that when online firms establish physical retail outlets, there is an average increase in total sales of 20%. The authors attribute the increase to the "availability effect," defined as the adding of distribution channels to increase the availability of an item, and thus increase sales. This line of research concludes that firms can maximize their sales by using multiple sales channels to reach their customers.

However, state taxes can dissuade firms from expanding their physical geographic footprints. For example, Giroud and Rauh (2019) find that corporate and pass-through entity level taxes decrease the likelihood of firm establishment. Sales taxes have been shown to be another factor influencing firm location decisions. Avittathur, Shah and Gupta (2005) study the impact of the central sales tax (C.S.T.) in India. The C.S.T. is a federal sales tax assessed when goods produced in one state are sold in another state. The authors find that firms respond to the

tax by establishing new distribution centers that serve as an intermediate stop for their products, an action that is sufficient to recategorize sales of the items as intra-state and not subject to the C.S.T. Bruce, Fox and Luna (2015) examine the propensity of firms to establish nexus in states and find that higher sales tax rates and broader sales tax bases reduce the likelihood of firms creating nexus in large states. Beem and Bruce (2021) find firm-level empirical support for Bruce, Fox and Luna (2015) by examining historical firm and state-level data to estimate the effect of the physical presence nexus standard on firm establishments and employment. They find that the physical presence standard established in *Quill* created a non-neutral sales tax system that resulted in market distortions that limited business activity. These studies demonstrate that firms carefully choose where to establish physical locations to maximize sales tax avoidance.

To help explain the frictions between sales maximization and sales tax avoidance, Arya and Mittendorf (2018) present a model of the choice to create brick-and-mortar stores in a market subject to sales taxes. The model illustrates the tradeoff between the benefits from increased distribution channels and the loss of the competitive pricing advantage due to the collection of sales taxes. The model predicts that when firms can no longer avoid collecting sales taxes, a firm's decision to establish a physical presence rests solely on the firm's desire to increase its customer base by appealing to different segments of the market. For example, a firm that operates exclusively online appeals to customers who prefer to make remote purchases and avoid going to brick-and-mortar stores. However, online firms do not capture sales to customers who prefer the tactile experience of in-person shopping. Additionally, other customers would like to shop online but are not willing or able to wait extended periods for their purchases. By establishing a physical presence in a state, firms can extend their reach and attract additional

types of customers. The frictions associated with firm location decisions and sales taxes are removed by *Wayfair*, as firms can no longer avoid sales taxes by avoiding physical presence. As such, it is possible firms will choose to expand their physical locations to overcome their loss of pricing advantage following the *Wayfair* decision.

3. HYPOTHESIS DEVELOPMENT

The significant change in sales tax nexus standards triggered by *Wayfair* creates many operating hurdles for e-commerce firms. First and foremost, e-commerce firms lose the strategic pricing advantages associated with not collecting sales taxes from their customers (Hoopes et al., 2016). After *Wayfair*, e-commerce firms face increased levels of competition with brick-and-mortar firms. Second, e-commerce firms also face increased costs related to sales tax compliance. Finally, the ruling creates uncertainty and risk associated with firms' previous strategies on state taxation. While the *Wayfair* decision prohibits states from applying economic nexus standards retroactively, firms need to assess if they were fully compliant with previous laws related to other taxes (i.e., income taxes, employment taxes, etc.). To the extent that there are gaps in their filing history, firms may be subject to back taxes, penalties, and interest. Investors and analysts are likely to recognize these threats and adjust their expectations of firm value and future sales prospects accordingly. Therefore, I hypothesize the following:

> ***H1(a):*** *Investors in e-commerce firms react negatively to events that increase the probability of broader sales tax nexus standards.*
>
> ***H1(b):*** *Analysts of e-commerce firms react negatively to events that increase the probability of broader sales tax nexus standards.*

Previous studies of market participants' understanding of complex tax issues find mixed results. Some studies find that investors and analysts are not able to process complex tax information (Plumlee 2003; Chi, Pincus, and Teoh 2014; Francis, Newman, and Newton 2019), while other studies come to the opposite conclusion (Bratten et al. 2016; Hutchens et al. 2020). The complexities of state and local sales tax regulation, as well as the nuances of sales tax

incidence, suggest that a null hypothesis of no reaction on the part of analysts and investors is possible.

How e-commerce firms respond to *Wayfair* is also an open question. Pre-*Wayfair*, e-commerce firms employed strategies to constrain operations and distribution networks to avoid sales taxes (Cockfield, 2002; M. J. Cowan, 2007; Gordon, 2009; Swain, 2001). Such techniques do not allow firms to avoid sales taxes after *Wayfair*. The regulatory shock associated with *Wayfair* will disrupt e-commerce firms' competitive advantage and lead them to either reposition themselves in the marketplace or exit (Argyres et al., 2019). This repositioning will require adjustment costs, which may involve the redeployment of existing capital and labor inputs (Madhok et al., 2015; Sakhartov & Folta, 2014). In a dynamic environment, firms will also try to identify new opportunities and will deploy new capital resources to capitalize on these opportunities (Pisano, 2017). In an e-commerce setting, this is likely to include offering faster delivery times and more ancillary services, such as installation. Such offerings will require firms to be physically closer to their customers; therefore, I hypothesize:

> ***H2:*** *E-commerce firms establish locations in new states with state sales taxes after sales tax nexus standards are broadened***.**

It is possible that I may not find support for *H2* because some e-commerce firms may not have sufficient resources to establish locations in new states or may not be nimble enough to expand in the immediate years after *Wayfair*. It is also possible that e-commerce firms may respond to broader sales tax nexus standards by contracting operations instead of expanding. For example, this was the strategy employed by Amazon after states like North Carolina and Colorado adopted click-through nexus laws targeting them. Amazon responded by cutting all business ties to states that had enacted broader sales tax nexus standards. This allowed Amazon

to continue to avoid collecting sales taxes in states where it had no physical presence (ITEP.org, 2021). Amazon was able to weather the trade-off between revenue and sales tax avoidance; however, such a trade-off may not be tenable for all firms.

4. RESEARCH DESIGN, SAMPLE, AND DESCRIPTIVE STATISTICS

4.1 Identification of Treatment Firms

To identify firms impacted by *Wayfair,* I begin by conducting a set of queries using the full-text search function in the SEC EDGAR database. I focus on 10K, 10Q, 8K and S-4 filings, because any reference within these documents must have been deemed material by management. My first search identifies company SEC filings containing the phrases "sales tax" and "Supreme Court." The result of this query yields 985 filings from 253 unique companies. The second query uses the words "Wayfair" and "Supreme Court" (986 filings, 181 companies), the third "Wayfair" and "sales tax" (964 filings, 58 companies), and finally, "Physical presence" and "sales tax" (432 filings, 136 companies). I combine the results and use company names and ticker symbols to eliminate duplicate records. I then manually review each document (10-K, 10-Q, 8-K, and S-4s) to confirm that it explicitly mentions the *South Dakota v. Wayfair* case. This process yields 170 treated firms. The Appendix provides examples of the language used by treated firms to discuss the ramifications from *South Dakota v. Wayfair* in their SEC filings

I do not limit my analysis to retail firms as previous researchers have done (eg. Bruce et al. 2015; Hoopes et al. 2016; Luchs-Nunez 2022). The ruling in *Wayfair* impacts all firms that engage in interstate commerce, including manufacturing, wholesale, and service firms (BDO 2018). Figure 1 provides a graphical representation of the industry composition by OSHA SIC Division categories of the sample, and Table 1 provides further details on the sub-classification of the industries.[17] My sample includes a significant percentage of manufacturing (22.35%) and service firms (49.41%), which have been largely absent from the previous sales tax research.

[17] The U.S. Department of Labor's Occupational Safety and Health Administration (OSHA) publishes the Standard Industrial Classification (SIC) Manual. The manual divides different industries into Divisions, that are then subdivided into two-digit major groups (SIC2) (U.S. Department Labor, 2021).

4.2 Investor Response to Wayfair - H1(a)

To investigate investor reactions to Wayfair, I use EVENTUS to test the market reaction for treated firms to specific events leading up to and including the ruling in South Dakota v. Wayfair. My event dates are important milestones in the progression of Wayfair through The Court docket:

January 12, 2018: *Wayfair* case accepted by The Court.

April 17, 2018: *Wayfair* oral arguments heard by The Court.

June 21, 2018: *Wayfair* ruling is released.

The dates before the ruling are significant, as they impacted the probability of The Court making substantial changes to state nexus rules. It is also worth noting that after oral arguments, The Court has discretion as to when it releases its final ruling.

To help validate my choice of event dates, I conduct a Google Trends search of *South Dakota v. Wayfair*. Google Trends tracks web traffic of searches on the search engine and scores the popularity of search terms between 1 and 100, with 100 representing peak popularity for that search and indicating considerable widespread interest in the searched topic. Figure 2 presents a Google Trends chart of web and news search trends for *South Dakota v. Wayfair* between December 1, 2017, and August 1, 2018. There is a distinct spike in search activity for the term *South Dakota v. Wayfair* on the date of oral arguments (April 17, 2018) and the release of the ruling (June 21, 218). The first event date (January 12, 2018) does not show a spike in Google search popularity.[18] Despite the lack of Google Trend scoring, I include January 12, 2018, as my first event date because it is the first procedural move by The Court to revisit the physical

[18] For this date, the lack of attention is likely because the order was issued as part of a Miscellaneous Order that contained multiple orders related to thirteen separate cases. Additionally, the Miscellaneous Order was released on a Friday afternoon before a long holiday weekend and could have escaped the notice of the public and news media.

presence standard of *Quill*. The results of my search allow me to conclude that there was significant popular interest in the progression of the *Wayfair* case, and it is likely that investors and analysts were paying attention.

I use the risk-adjusted mean cumulative abnormal returns for my treated firms to identify investor reactions to the event dates. I begin by computing the abnormal return of firm *i* at time *t* as:

$$AR_{i,t} = R_{i,t} - (\hat{\alpha}_i + \hat{\beta}_i R_{m,t}) \quad (1)$$

Equation (1) represents the difference between the actual firm return, $R_{i,t}$, and the expected return based on a market model using the equal-weighted market index from CRSP, $R_{m,t}$. The market-weighted index includes all Stock Exchanges in CRSP (NYSE, AMEX, and NASDAQ) and the S&P 500. Using an equally-weighted stock market portfolio removes concerns that my findings result from macroeconomic events that may impact the entire stock market and ensure that my results are not skewed by the size of any specific firm in my sample. For completeness, I also conduct my analysis using a value-weighted stock market portfolio. The coefficients $\hat{\alpha}_i$ and $\hat{\beta}_i$ are used to calculate the expected values of $R_{i,t}$ given $R_{m,t}$ and are based on an estimation window that is 255 days long, ending 46 trading days before the event which is the default window recommended by EVENTUS. $\hat{\beta}_i$ represents the expected sensitivity of $R_{i,t}$ to $R_{m,t}$.

The daily abnormal returns are combined to calculate firm *i*'s cumulative abnormal return, *CAR (*Equation 2*),* during the event window. The subscript t_1 represents the first day in the event window and t_2 represent the last day in the event window.

$$CAR_i = \sum_{t=t_1}^{t_2} AR_{i,t} \quad (2)$$

I then calculate the mean cumulative average abnormal returns*, CAAR (*Equation 3*),* which is the average *CAR* for each treatment firm *i* across the event window *t*.

$$CAAR_{t,t_2} = \frac{1}{N}\sum_{i=1}^{N}\sum_{t=t_1}^{t_2} AR_{i,t} \qquad (3)$$

I examine one-day and five-day event windows for each of my event dates. The windows begin with my event date and allow for one or five days for the market to capture and react to each event. The Court adheres to a culture of strict confidentiality and generally does not leak information.[19] Therefore, it is highly unlikely that any information would have leaked prior to the event date (Bump, 2013; Carter, 2017; Rubin, 2018).

I match my list of treated firms to the CRSP database and am left with 137 treated firms. Twenty-nine firms are dropped because they lack sufficient price data during the estimation period, or have insufficient estimation days, for the first event date (January 12, 2018) yielding a sample of 108 firms. The number of firms with sufficient pricing data increases for the second date (April 17, 2018), yielding a sample of 111 firms. Finally, for the third event date (June 21, 2018), 113 firms have sufficient pricing data for estimation. Further detail regarding my sample construction is provided in Table 2, Panel A.

4.3 Analyst Response to Wayfair - H1(b)

To examine analysts' responses to changes to the probability of changes to sales tax nexus standards, I focus on changes in analysts' forecasts of future revenues. I focus on changes in forecasted revenues because *Wayfair* is expected to impact the price competitiveness of online

[19] The significant news coverage following the May 2, 2022 publication of a draft opinion in *Dobbs v. Jackson Women's Health Organization*, No. 19-1392, 597 U.S.__(2022) demonstrates how rare it is for an information breach to occur at The Court (Bravin & Kendall, 2022; Gerstein, 2022; Gresko & Associated Press, n.d.).

firms and may cause them to lose market share. I use the following difference-in-differences OLS regression model:

$$\Delta ANALYSTFORECAST_{i,j,t,v} = \beta_0 + \beta_1\, TREATED_i + \beta_2\, WAYFAIREVENT_t + \beta_3 TREATED_i * WAYFAIREVENT_t + \boldsymbol{X}\gamma + \varepsilon, \quad (4)$$

$ANALYSTFORECAST_{i,j,t,v,}$ is the revenue forecast for firm i, by analyst j, announced at time $t,$ for the fiscal period end date v. $\Delta ANALYSTFORECAST_{i,j,t,v}$ is the difference between $ANALYSTFORECAST_{i,j,t,v}$ and $ANALYSTFORECAST_{i,j,t-1,v}$, scaled by $ANALYSTFORECAST_{i,j,t-1,v}$. [20] The variable $TREATED_i$ is equal to one if firm i mentioned *Wayfair* in its SEC filings, and zero otherwise. $WAYFAIREVENT_t$ is an indicator variable equal to 1 for analyst forecast revisions that occur after one of the three previously identified event dates (see section 4.2).[21] Observations are assigned to a specific event period if the $\Delta ANALYSTFORECAST_{i,j,t,v}$ for that observation straddles an event date. If the $\Delta ANALYSTFORECAST_{i,j,t,v}$ straddles more than one event date, it is assigned to the later event. Figure 3 presents a graphical representation of the assignment of $WAYFAIREVENT_t$ to the sample and examples of treatment assignments. The symbol $\boldsymbol{X}\gamma$ represents a vector of analyst- and firm quarter-level controls based on prior literature (Clement, 1999; Frankel et al., 2006; Hoopes et al., 2016). The analyst level controls include the variable $STALE_{i,j,t,v,}$ which controls for forecast changes simply due to the passage of time. I also control for the analyst's overall experience $(EXP_{j,t})$ and firm-specific experience ($FIRMEXP_{i,j,t}$). Firm-quarter controls include firm size ($QASSETS_{i,t}$), and firm indebtedness ($QLEV_{i,t}$). I control for firm growth prospects and profitability ($QMTB_{i,t,}$ $QROA_{i,t}$) which could also account for

[20] For example, suppose on June 1, 2018 (t), Analyst A(j) revises their previous revenue forecast announced on January 1, 2018*(t-1)* for XYZ Company(i), for the fiscal period ending on December 31, 2018 (v). The $\Delta ANALYSTFORECAST_{i,j,t,v}$ would be the difference between the forecast at time t and the forecast at time *t-1,* scaled by the forecast at time *t-1*.

[21] For example, an observation would be assigned to *Wayfair Event* 1, if the previous analyst forecast precedes January 12, 2018, and the current analyst forecast is after January 12, 2018, and before April 17, 2018. If the time between forecasts spans more than 2 event dates, it is assigned the *Wayfair Event* number of the later event. See Figure 3 for a graphical representation of the assignment.

revenue forecast changes. Finally, I include industry and firm fixed effects, and all standard errors are clustered at the firm level.

To investigate analyst reactions, I use the WRDS Linking Suite to create linking tables between IBES and CRSP, and COMPUSTAT. I then augment the linking tables with data from IBES, and the CRSP/COMPUSTAT merged database. Each record for a company, analyst, and forecast period end date is identified as a group, which allows for the calculation of my outcome variable, $\Delta ANALYSTFORECAST_{i,j,t,v,}$ as well as the independent variable $STALE_{i,j,t,v}$. My final sample comprises 66,605 observations with 26,108 unique analyst-firm-fiscal period end date groups. Given that the *Wayfair* decision was released on June 22, 2018, I limit my sample to forecasts with period end dates between August 31, 2018, and June 30, 2019. Limiting the sample period to these dates helps increase the likelihood that changes in analyst forecasts are related to *Wayfair*. Further detail regarding my sample construction is provided in Table 2, Panel B. See Table 3 for further information on variable names and construction.

Table 4 presents the descriptive statistics for the dependent and independent variables in Equation 4. Panel A presents the statistics for the entire sample, while Panel B presents the statistics partitioned on the variable $TREATED_i$. A test of differences in the means of the variables between the treatment and control firms indicates that they are significantly different and suggests that the two groups may not be comparable. To avoid any confounds due to incomparability, I balance my treatment and control groups on $\boldsymbol{X}\gamma$ using entropy balancing (Hainmueller, 2012). The entropy balancing technique applies a set of weights to the control group observations to balance covariate moments (means, variance, and skewness) between the treated and untreated firms. An advantage of this technique is that all observations are retained in the sample, and regression results are sufficiently robust to allow for causal inferences (Zhao &

Percival, 2017). Table 4, Panels C presents the descriptive statistics after entropy balancing, again, partitioned on $TREATED_i$. The means of the two groups are no longer statistically significant, removing any concern regarding the comparability of my treatment and control groups.

4.4 Firm Location Decisions in Response to Wayfair - H2

My second hypothesis examines firm location decisions. After *Wayfair*, the tension between firm physical presence location decisions and state and local sales taxes are eliminated and leads to my prediction in *H2* that firms will establish locations in new states with sales taxes in the years following the ruling. To identify firm locations, I use the YTS database. The YTS database provides time-series location-specific data, allowing me to identify the number of firm locations for each state and year combination to use in my analysis. To test *H2* I estimate the following model:

$$NUMTAXABLESTATES_{i,t} = \beta_0 + \beta_1 TREATED_i + \beta_2 POST_t + \beta_3 TREATED_i * POST_t + \boldsymbol{X}\gamma + \varepsilon \quad (5)$$

The dependent variable $NUMTAXABLESTATES_{i,t}$ is the total number of states with sales taxes (Walczak & Drenkard, 2018) that firm *i* has locations in during year *t*. The variable $TREATED_i$ is equal to one if firm *i* mentioned *Wayfair* in their SEC filings, and zero otherwise. $POST_t$ is an indicator variable equal to zero for the years 2016 and 2017, and one if firm *i*'s fiscal year-end occurs after the *Wayfair* ruling (2018 and 2019). The independent variable of interest is $TREATED_i * POST_t$ and represents changes in taxable state locations after the *Wayfair* decision for the treatment firms relative to the control firms. $\boldsymbol{X}\gamma$ represents a vector of control variables at the firm level. I control for firm size ($LOGAT_{i,t}$), and firm indebtedness ($LEV_{i,t}$) as well as the firm's growth prospects at the end of the prior fiscal year ($MTB_{i,t-1}$). I also control for firm profitability ($ROA_{i,t}$) and include a firm level control for the firm's capital intensity ($PPE_{i,t}$) as

these would factor into a firm's ability to cover the cost of expanding to new states. Following Ljungqvist et al. (2017), I add additional controls to my model to account for firm liquidity, which could impact whether and how quickly firms can respond to the changes in *Wayfair*.[22] These are cash surplus ($CASHSURP_{i,t}$), net operating loss carryforwards ($LOSSCFWD_{i,t}$), and year-over-year sales growth ($SALESGR_{i,t}$). I also include a lagged dependent variable ($LNUMTAXABLESTATES_{i,t}$) and lagged number of locations in states where there is a sales tax ($LNUMLOCATIONS_{i,t}$), and firm age $(COMPANYAGE_{i,t})$ as these variables are highly predictive of the number of states for firm *i* in time *t* as well as the geographic density of the firm.[23] Finally, I include industry and firm fixed effects, and all standard errors are clustered at the firm level. See Table 3 for further information on variable names and construction.

To further refine my analysis of firm location decision-making after the *Wayfair* ruling, I investigate whether firms locate in new states following *Wayfair* using the following model:

$$NUMNEWTAXABLESTATES_{i,t} = \beta_0 + \beta_1 TREATED_i + \beta_2 POST_t + \beta_3 TREATED_i * POST_t + \boldsymbol{X}\gamma + \varepsilon \quad (6)$$

The dependent variable $NUMNEWTAXABLESTATES_{i,t}$ is a count variable representing the number of new states where firm *i* is located, at time *t*, which have a state sales tax as compared to time *t-1*. By focusing only on new state locations, I can better isolate changes as a result of *Wayfair*. Once again, the variable of interest is $TREATED_i * POST_t$, and I include the same firm-level control variables from the analysis from Equation (5). This analysis is a more

[22] Adding a location in a new state is an investment that increases firm risk; therefore, I incorporate additional firm level control variables from studies that examine firm risk. While the focus of Ljungqvist et al. (2017) investigates firm risk responses to changes in corporate income tax rates, the paper also explores the mechanisms that firms use to reduce their risk.

[23] The use of a lagged dependent variable (dv) raises concern of correlation between the lagged dv and the error term. This is especially concerning given a "small T, and large N." This creates bias in the estimate of the lagged dependent variable and can cause downward bias in other explanatory variables. One suggestion is to double or triple lag the dependent variable to compensate for this bias (Wooldridge 2019). In untabulated regressions, I include double lagged, and triple lagged dependent variables, and the results of my regression remain consistent.

direct test of the Arya and Mittendorf (2018) model that proposes that firms will expand into new territories in the absence of sales tax frictions, as it is focused on new state entries. Firms that have no changes in their number of taxable states are excluded from the Poisson estimation as the dependent variables during the sample period are all zeroes (i.e., no variation). This allows for a comparison of the rate of new state additions between treatment and control firms.

For my analysis of new firm locations, I begin with all U.S. firms in COMPUSTAT from 2015-2019 . These firms are matched to YTS using the company name, stock ticker, and address by data analysts at the Business Dynamics Research Consortium. A field populated in YTS is a parent company identifier for each location. Using the parent identifiers, I link the total number of locations for each parent company and assign them a unique group identification number for each year *t*. Using the group identification number, I then incorporate firm-level consolidated financial information from COMPUSTAT into the sample. I also use the YTS variable that identifies each location's state to identify the total number of states with sales taxes where a firm is located and the total number of locations in taxable states for each year of my sample. I eliminate firms with missing control variables, and firm-years outside of my testing window (before 2016 or after 2019). I then compare the two-digit SIC codes of the treatment and control firms and eliminate all control firm observations for firms whose two-digit SIC codes are not present in the treatment group. After these eliminations, my sample contains 7,652 firm-year observations for 83 treatment firms and 1,830 control firms for 2016-2019. Further detail regarding my sample construction is provided in Table 2, Panel C.

Descriptive statistics are presented in Table 5. Panel A presents the statistics for the entire sample, while Panel B shows the statistics partitioned on the variable $TREATED_i$. A test of differences in means of the variables between the treatment and control firms indicates varying

degrees of differences between treatment and control group means. The treatment group comprises younger firms (lower $COMPANYAGE_{i,t}$). Treatment firms also have less property plant and equipment ($PPE_{i,t}$) and have access to more cash ($CASHSURP_{i,t}$) than control firms. These differences seem reasonable considering that treatment firms are younger and likely have more cash due to recently raised funds via IPOs. As I have already limited the control group to firms that share two-digit SIC codes with treatment firms and winsorized all continuous variables at the 1st and 99% percentiles, no further manipulation of my sample is needed.

To test the appropriateness of the treatment and control assignments for my difference-in-difference analysis, I examine the parallel trends for Equation (5) and Equation (6). Figure 4, Panel A examines the mean number states with sales taxes where firms have locations partitioned by $TREATED_i$. The treatment ($POST_t$) begins after 2017. While the pre-period trend between treatment and control firms are not exactly parallel, they both show a gradual upward trend. Following Roth (2022) I include a counterfactual trendline to illustrate how the increased expansion of state locations after *Wayfair* among treatment firms would have looked had there been no difference in location growth between treatment and control firms. Examination of the figure shows a modest difference in the number of taxable state locations between the treated and control samples. I add further graphical evidence of parallel trends prior to treatment with Figure 4, Panel B, which shows the average change in new taxable states beginning in 2016. Both figures show a divergence in the growth of new sales tax states between treatment and control group after treatment begins in 2017. All figures support my use of difference-in-differences estimation and lend visual support for *H2*.

5. RESULTS

5.1 Investor Reaction Tests - H1(a)

Table 6 reports the mean cumulative abnormal returns (*CAAR*) for treated firms for events that affected the probability of broader sales tax nexus standards (January 12, 2018; April 17, 2018; June 21, 2018). I use two test statistics to test the statistical significance of the *CAAR*s. The first test statistic is the parametric standard cross-sectional test, or B.M.P. test (Boehmer et al., 1991). While the Patell (1976) z-score had been the standard for evaluating whether abnormal returns are greater than zero, a variance increase around the event day could result in a biased test (Brown & Warner, 1980, 1985). The B.M.P. test improves upon the Patell test by correcting for variance increases around the event date. I also present the generalized sign test statistic, a nonparametric test (i.e., does not assume a normal distribution of the abnormal returns), that evaluates the proportion of positive and negative abnormal returns against an assumed 50/50 split between positive and negative returns that would be presumed if there is no reaction to the event (A. R. Cowan, 1992).

The first event date is the date that The Court agreed to hear the case *South Dakota v. Wayfair* (January 12, 2018). By agreeing to hear the case, The Court signaled that it was open to potentially overturning the physical presence requirement established in *Quill*. Table 6, Panel A reports the results. Using the equally-weighted market index I observe negative and significant mean abnormal returns for the two return windows examined. For the one-day period immediately after the announcement (0/+1), the *CAAR* is -1.46% and with significant B.M.P. and Generalized Sign Z-score (G.S.Z.) (p-value<.01) test statistics. The negative *CAAR* increases in magnitude for the 0/+5-day window (*CAAR*=-2.39%, p-value<.01). Panel B reports the results using the value-weighted market index. I continue to observe negative and significant mean

abnormal returns for the one-day period immediately after the announcement (0/+1). The *CAAR* is -1.23% and with significant B.M.P. and Generalized Sign Z-score (G.S.Z.) (p-value<.01) test statistics. The negative *CAAR* again increases in magnitude for the 0/+5-day window (*CAAR*=-2.17%, p-value<.01). The negative and significant reactions documented in Panel A and Panel B for the first event date are consistent with investors perceiving an increased probability of broader sales tax nexus standards that will negatively affect e-commerce firms. These findings support my prediction in *H1(a)*.

The second event date is April 17, 2018, when The Court heard oral arguments in the case. While the date of oral arguments was published well in advance of the actual hearing, little was known about how each individual Justice regarded the issues in the case. Except for Justice Kennedy, no other Justice had openly discussed their judicial leanings relative to *Quill*. After oral arguments it appeared that The Court was sharply divided, which decreased expectations of the probability that The Court would overturn *Quill*. The *CAAR* for the one-day period immediately after oral arguments (0/+1) from Panel A is .94% and with significant B.M.P. (p-value<.01) and slightly less significant Generalized Sign Z-score (p-value<.10) test statistics. Panel A's positive *CAAR* increases in magnitude for the 0/+5 (*CAAR*=1.81%, B.M.P. p-value<.01, G.S.Z. p-value<.05) test statistics. I continue to see positive and significant results in Panel B, where the *CAAR* for the one-day period immediately after oral arguments (0/+1) is .79% and with significant B.M.P. and Generalized Sign Z-score (p-value<.10) test statistics. Panel B's positive *CAAR* increases in magnitude for the 0/+5 (*CAAR*=1.43%, B.M.P. p-value<.05, G.S.Z. p-value<.05). These positive market reactions are consistent with the analysis of experts who felt that The Court was deeply divided about overturning *Quill* (Cullers, 2018; Horowitz, 2018; Reichenberg-Sherr, 2018; Yetter, 2018). I interpret the positive *CAARs* as

investors revising their expectation of The Court overturning *Quill* downwards after oral arguments.

The final event date is June 21, 2018, the day that The Court announced its ruling in *Wayfair*. The decision eliminated the physical presence requirement for sales tax nexus established in *Quill*. As a result of the decision, e-commerce firms can be required to collect sales taxes from customers in states where the firms do not have a physical presence. This change in sales tax nexus standard eliminates the competitive pricing advantage e-commerce firms previously held over brick-and-mortar firms. For the one-day period immediately after the ruling was released (0/+1), Table 6, Panel A reports a *CAAR* of -1.09% and with significant B.M.P.(p-value<.01) and G.S.Z. scores (p-value<.05). Once again, the magnitude of the *CAAR*s increase for the 0/+5 window (*CAAR*=-2.24%, B.M.P. p-value<.01, GSZ p-value<.01). The results from Panel B are consistent with those from Panel A, with 0/+1-day *CAAR*s of -1.23% (B.M.P. p-value<.01, GSZ p-value<.05) and 0/+5-day *CAAR*s of -2.30% (B.M.P. p-value<.01, GSZ p-value<.01). This response is consistent with investors seeing the ruling as being detrimental to e-commerce firms and is consistent with *H1(a)*.

5.2 Analyst Forecast Revisions - H1(b)

Table 7 reports the results of my estimation of Equation (4), which evaluates analyst reactions to events that affected the probability of broader sales tax nexus standards (January 12, 2018; April 17, 2018; June 21, 2018). My variables of interest are the interaction of $TREATED_i$ and $WAYFAIREVENT_t$. The effect of *Wayfair* for treated firms is reflected in the coefficient for each $TREATED=1*WAYFAIREVENT_t$ combination. For treated firms who had a forecast revision after January 12, 2018, but before April 17, 2018, $TREATED_i=1*WAYFAIREVENT_t=1$ is negative and significant (coeff=-.0252, p-value<.01) and consistent with *H1(b)*, that predicted

analysts would perceive the changes in *Wayfair* as having a negative impact on e-commerce firms. The coefficient for treated firms with a forecast revision after April 17, 2018, but before June 21, 2018 ($TREATED_i=1*WAYFAIREVENT_t=2$) is insignificant. This result suggests that, in contrast to investors, analysts did not revise their estimates of the detrimental impact of *Wayfair* after oral arguments were heard at the Supreme Court. Finally, the coefficient for treated firms that had a forecast revision after June 21, 2018, but before December 31, 2018 ($TREATED_i=1*WAYFAIREVENT_t=3$) is negative and significant (coeff=-.0102, p-value<.05). I interpret this result as analysts further downgrading their expectations of firm revenues after the ruling in *Wayfair*. I find these results broadly consistent in supporting my predication in *H1(b)*.[24]

5.2.1 Additional Robustness Tests - H1(b)

To test the robustness of my results from my test of analyst forecast revisions, I re-estimate Equation (4), however, this time I only include observations where the forecast and forecast revision were made within a specific time window surrounding each $WAYFAIREVENT_t$. Limiting the observations for analysis to a tighter window around each event date provides additional assurance that the revisions are related to the specific *Wayfair* event. The results of my re-estimation are reported in Table 8. Column (1) presents the results limiting observations to forecasts that were made less than 30 days before each $WAYFAIREVENT_t$ and revised no more than 30 days after each $WAYFAIREVENT_t$. Consistent with Table 7, the coefficient for $TREATED_i=1*WAYFAIREVENT_t=1$ is negative and significant (coeff=-.0267, p-value<.05) and the coefficient for $TREATED_i=1*WAYFAIREVENT_t=2$ is not significant. Unlike Table 7, the coefficient for $TREATED_i=1*WAYFAIREVENT_t=3$ is not significant. Column (2) revises the

[24] VIF scores evaluate multicollinearity with respect to the independent variables. I calculate VIF scores (untabulated) to ensure the estimates of my independent variables are not biased. No variables have a VIF score over two (average 1.28), and thus I am reasonably assured that my model does not suffer from issues associated with multicollinearity.

window to forecasts made no more than 45 days before each $WAYFAIREVENT_t$ and revised no more than 45 days after each $WAYFAIREVENT_t$. Consistent with Column (1) the coefficient for $TREATED_i=1*WAYFAIREVENT_t=1$ is negative and significant (coeff=-.0276, p-value<.01), while the coefficients for the other event days are insignificant. Overall, these results provide further support of *H1(b)*, however it appears that analysts did not revise their forecasts after the initial announcement that The Court would hear the case.

5.3 Firm Location Decisions - H2

I first perform a simple two-way ANOVA to analyze the effect of the *Wayfair* decision on e-commerce firms' number of locations in taxable states. Table 9, Panel A reveals a statistically significant interaction between e-commerce firms and the *Wayfair* decision ($TREATED_i*POST_t$) on the number of taxable states where firms have locations ($NUMTAXABLESTATES_{i,t}$). Panel B analyzes the effect of e-commerce firms and the *Wayfair* decision on the number of new taxable states where a firm establishes a location ($NUMNEWTAXABLESTATES_{i,t}$), and reveals another statistically significant interaction between e-commerce firms and the *Wayfair* decision ($TREATED_i*POST_t$). The results of this test confirm a statistically significant difference between treatment and control groups pre and post *Wayfair*.

I then proceed to my multivariate analysis of the *Wayfair* decision on firm location decisions. Table 10, Column (1) presents the conditional fixed effects Poisson estimation of Equation (5) using the number of different states with sales taxes where a firm has physical locations ($NUMTAXABLESTATES_{i,t}$) as the dependent variable.[25] My variable of interest is the interaction of treated firms ($TREATED_i$) and the indicator variable ($POST_t$) for fiscal years

[25] I perform a Hausman test to test for model misspecification. The null hypothesis is that there is no correlation between the error terms and the regressors in the model. The results of the test reject the null, and therefore firm fixed effects are recommended for my estimation of Equation (5).

ending after the *Wayfair* ruling. The results of Column (1) show a positive and significant coefficient for $TREATED_i*POST_t$ (one-tailed p-value<.10). This means that for years ending after the *Wayfair* ruling, the number of states with state sales taxes where treatment had physical locations increased by 1.84% as compared to control firms.

Column (2) presents the results for the conditional fixed effects Poisson estimation of Equation (6) with the count of new states with sales taxes where a firm has a location ($NUMNEWTAXABLESTATES_{i,t}$) as the dependent variable. As noted in Section 4.4, the Poisson estimation excludes firms who have no new taxable states during the sample period due to having zero variation in their dependent variable. By excluding these firms, I can conduct a more direct comparison between treatment and control firms that were expanding during the sample period. Once again, the interaction term $TREATED_i*POST_t$ is my independent variable of interest. The coefficient on $TREATED_i*POST_t$ is positive and significant (p-value<.10). The results show that after *Wayfair*, treated firms' new physical locations in states with sales taxes increased by 85.98% as compared to control firms. This result further supports my prediction of *H2*.

5.4 Additional Analysis – H2

5.4.1 Falsification Test

Given the magnitude of my results from Table 9, Column (2) I conduct a falsification test replacing the dependent variable with $NUMNEWNONTAXSTATES_{i,t}$. This variable represents all new physical locations established by treatment and control firms in the five states that do not have sales taxes.[26] Table 11 presents the results of the falsification test. The coefficient on $TREATED_i*POST_t$ is insignificant, indicating there is no statistically significant increase in the

[26] The five states that do not have sales taxes are Alaska, Delaware, Montana, New Hampshire, and Oregon.

number of new locations in states without sales taxes between treatment and control groups during my sample period. This helps to validate my results from Table 10 and increases my support for my prediction of *H2*.

5.4.2 Cross Sectional Test - Industries

The composition of my treatment sample allows me the opportunity to examine whether the effects of broadening sales tax nexus standards differ by industry. I partition my sample into four different samples using OSHA SIC Divisions. Column (1) of Table 12 presents results for manufacturing firms (SIC Divisions 21 through 39). Column (2) presents results for retail and wholesale firms (SIC Divisions 50 through 59). Column (3) presents results for service firms (SIC Divisions 71 through 87). Column (4) of this table presents results for all other firms not identified in Columns (1), (2) and (3). The coefficient on $TREATED_i$*$POST_t$ is insignificant for Columns (1) and (4), and positive and significant for Columns (2) and (3). These results indicate that retail, wholesale and service firms were most impacted by the ruling in *Wayfair*. This is consistent with the practitioner expectation that the impact of *Wayfair* would reach beyond retail and wholesale firms (BDO. 2018, n.d.; Csan et al., 2018; Jennifer Jensen et al., 2019), and is the first empirical evidence of service industry effects.

5.4.3 Decisions to locate in individual states

Next, I explore whether firms locate in certain states due to state level characteristics. To incorporate state level variables into my estimation I follow the sample construction from Cen, Maydew et al. (2017), and Luchs-Nunez (2022), and expand my location sample so that each firm has 51 observations for each year of my analysis.[27] I construct a new dependent variable called

[27] For each year in the sample, every firm has a separate observation for each of the 50 states plus the District of Columbia. This yields 204 for observations for each firm in my sample (51*4=204).

$PHYSICALPRESENCETAX_{i,s,t,}$ that is equal to one if firm i has a physical location in a state with sales taxes $s,$ in year t, and zero otherwise. My revised equation is:

$$PHYSICALPRESENCETAX_{i,s,t} = \beta_0 + \beta_1 TREATED_i + \beta_2 POST_t + \beta_3 TREATED_i * POST_t + \boldsymbol{X}\gamma + \varepsilon \quad (7)$$

I include all control variables from Equation (6); however, I revise the *LNUMTAXSTATES* and *LNUMLOCATIONS* variables, so they are calculated at the firm i, state s, and year t level. I then include a new control variable for the level of economic activity within a state called $LOGSGDP_{s,t,}$ which is the natural log of state-level GDP. Finally, I include a control variable for the combined state and local sales tax rate ($SALESTAXRATE_{s,t}$).

I begin by estimating a Linear Probability Model (LPM) with firm-state fixed effects, as my new dependent variable ($PHYSICALPRESENCETAX_{i,s,t}$) is bounded by zero and one. I am not concerned about a non-linear relationship with my dependent variable and $TREATMENT_i$ and $POST_{t,}$ as these variables are also binary. However, there may be a non-linear relationship between the dependent variable and the remaining continuous control variables, leading to potential bias in my estimation. Due to this concern, I also use correlated random effects (CRE) logit and probit models to estimate Equation (7).[28]

Table 13 reports the results from the regressions of Equation (7). Column (1) presents the results of my revised equation LPM with firm-state fixed effects. My coefficient of interest is once again $TREATED_i * POST_{t.}$ The coefficient is positive and significant (p-value<.01, one-tailed). The results show that the probability that treatment firms have a location in a state with sales taxes after the *Wayfair* ruling increases by 24.43 percentage points. Column (2) reports the predictive

[28] Fixed effects estimation is not possible with logit or probit in my case because it assumes there is no correlation in my error terms. Following Wooldridge (2019) I construct new control variables that reflect the mean of each control variable that is not a constant term for each firm. I then include these mean variables in my regression of Equation (7), and I cluster my standard errors at the firm-state level.

marginal effects from the CRE logit regression. The coefficient on $TREATED_i*POST_t$ is positive and significant (p-value<.01, one-tailed). The results indicate that the probability of treatment firms establishing a location in a new state with sales taxes increases by 23.89 percentage points after *Wayfair*. The results from the CRE probit model in Column (3) are quite similar to those of Column (2), with the coefficient on $TREATED_i*POST_t$ positive and significant (p-value<.01, one-tailed). Column (3) results indicate that the probability of treatment firms establishing a location in a new state with sales taxes increases by 23.99 percentage points after *Wayfair*. The results from Table 13 support the results found in Table 10 and show that e-commerce firms are more likely to establish new locations in states with sales taxes after *Wayfair.*

5.4.2 Cross-Sectional Test – Corporate Tax Rates, Sales Tax Rates, Population

Next, I look at specific states where firms in my sample chose to add new locations and consider the impact of previous determinants associated with establishing physical locations within a state. Previous literature has found that high state corporate taxes, high sales tax rates, and large populations decrease the probability that an e-commerce firm will establish a location within a state (Bruce et al., 2015; Giroud & Rauh, 2019; Reddick & Coggburn, 2007; Stone, 2013). I collect corporate and sales tax rate data from the Tax Foundation, a non-profit think tank, and population totals from the United States Census. Table 14 lists the states with the ten lowest (Column 1) and ten highest (Column 2) state corporate income tax rates (Panel A), sales tax rates (Panel B), and state populations (Panel C) in 2018. [29]

I tabulate the top ten states where firms established new locations after *Wayfair* (*POST=1*) in Table 15. Column (1) reports the states with the highest total number of new firm locations after

[29] Only Wyoming appears in all three panels in Column (1), and only Illinois and California appear in all three panels in Column (2). The lack of consistency between the panels suggests that high/low levels of state corporate taxes, sales taxes, and population may affect firm location decisions differently.

Wayfair for the entire sample, while Column (2) is limited to treatment firms, and Column (3) is limited to control firms. Both treatment and control firms established new physical presences in New York, California, Georgia, and Massachusetts, with New York receiving the highest number of new locations of any state. A visual comparison of Table 14 to Table 15 suggests that state population might be the most salient factor in location decisions after *Wayfair*.

Next, I attempt to empirically disentangle the effects of state corporate taxes, state sales taxes, and state populations on location decisions after *Wayfair*. I modify Equation (7) to add new indicator variables to the equation. The new variables are dummy variables that indicate states with the top quartile of state corporate taxes ($HIGHCORPTAX_{s,t}$), the top quartile of state sales taxes ($HIGHSALESTAX_{s,t}$), and the top quartile of state populations ($HIGHSTATEPOP_{s,t}$) for each specific year in my analysis. I investigate the effect of each determinant (corporate taxes, sales taxes, or population) in separate regressions by examining their interaction with $TREATED_i*POST_t$.

Table 16 reports the results of my analysis with Column (1) reporting results using LPM, Column (2) reporting the predicted margins from the CRE logit model, and Column (3) reporting the predicted margins from the CRE probit model. Panel A reports the results of my analysis of $HIGHCORPTAX_{s,t}$. The coefficient of interest is $TREATED_i*POST_t*HIGHCORPTAX_{s,t}$ and is negative and insignificant in Columns (1)-(3). Panel B reports the results of my analysis of $HIGHSALESTAX_{s,t}$. The coefficient of interest is $TREATED_i*POST_t*HIGHSALESTAX_{s,t}$ and is positive in Column (1) and negative in Columns (2) and (3), and insignificant for all columns. Panel C reports the results of my analysis of $HIGHSTATEPOP_{s,t}$. The coefficient of interest is $TREATED_i*POST_t*HIGHSTATEPOP_{s,t}$ and is positive and significant in Columns (1)-(3). In summary, I fail to find evidence that high state corporate tax and sales tax rates impact firm

location decisions following the *Wayfair* ruling. However, I find positive and significant evidence that e-commerce firms are more likely to establish locations in states with high populations after broadening sales tax nexus standards.

6. CONCLUSION

This paper investigates how capital market participants and firms respond to sales tax nexus standards changes. Prior research has shown that e-commerce firms had a competitive pricing advantage before the *Wayfair* ruling because they were not required to collect state sales taxes from customers unless they had a physical location within a state. *Wayfair* eliminates the physical presence requirement to establish nexus and allows states to require firms to collect sales taxes for sales made remotely (i.e., e-commerce). I find support for my hypothesis that investors and analysts expect the sales tax collection burden to negatively impact e-commerce firms, as evidenced by negative cumulative abnormal returns and downward revenue forecast revisions around events leading up to and including the ruling. I also find evidence that in the wake of the *Wayfair* ruling, e-commerce firms are more likely to expand into new states, suggesting the loss of pricing advantage leads them to alter their location decision making.

The impact of Wayfair goes beyond the collection and remittance of sales taxes, as e-commerce firms are now more likely to generate additional economic activity by establishing new locations in states. The findings of this study contribute to the growing literature investigating non-income taxes (Kubick et al. 2021; Luchs-Nunez 2022). Additionally, these findings should be of interest to legislators and policy makers as they contemplate federal laws that would amend sales tax nexus standards.

APPENDIX

Appendix
Sample Selection Examples

Example 1:

Take-Two Interactive Software, Inc. (TTWO)

Form 10-K for the period ended March 31, 2020
Filed for on May 22, 2020

Item 1A. Risk Factors

Changes in our tax rates or exposure to additional tax liabilities could adversely affect our earnings and financial condition.

On June 21, 2018, the U.S. Supreme Court issued its decision in South Dakota v. Wayfair, which overturned previous case law that precluded states from requiring retailers to collect and remit sales tax on sales made to in-state customers unless the retailer had a physical presence in the state. Although this case is limited to sales tax collection obligations, we continue to monitor the potential impact of this decision on our state income tax footprint.

Example 2:

Funko, Inc. (FNKO)

Form 10-K for the period ended December 31, 2019
Filed for on March 5, 2020

Item 1A. Risk Factors

Our e-commerce business is subject to numerous risks that could have an adverse effect on our business and results of operations.

Additionally, some jurisdictions have implemented, or may implement, laws that require remote sellers of goods and services to collect and remit taxes on sales to customers located within the jurisdiction. In particular, the Streamlined Sales Tax Project (an ongoing, multi-year effort by U.S. state and local governments to pursue federal legislation that would require collection and remittance of sales tax by out-of-state sellers) could allow states that meet certain simplification and other criteria to require out-of-state sellers to collect and remit sales taxes on goods purchased by in-state residents. Furthermore, in June 2018, the U.S. Supreme Court ruled in South Dakota v. Wayfair that a U.S. state may require an online retailer with no in-state property or personnel to collect and remit sales taxes on sales made to the state's residents, which may permit wider enforcement of sales tax collection requirements. These collection responsibilities and the complexity associated with tax collection, remittance and audit requirements would also increase the costs associated with our e-commerce business.

Figure 1
SIC Divisions of Treated Firms

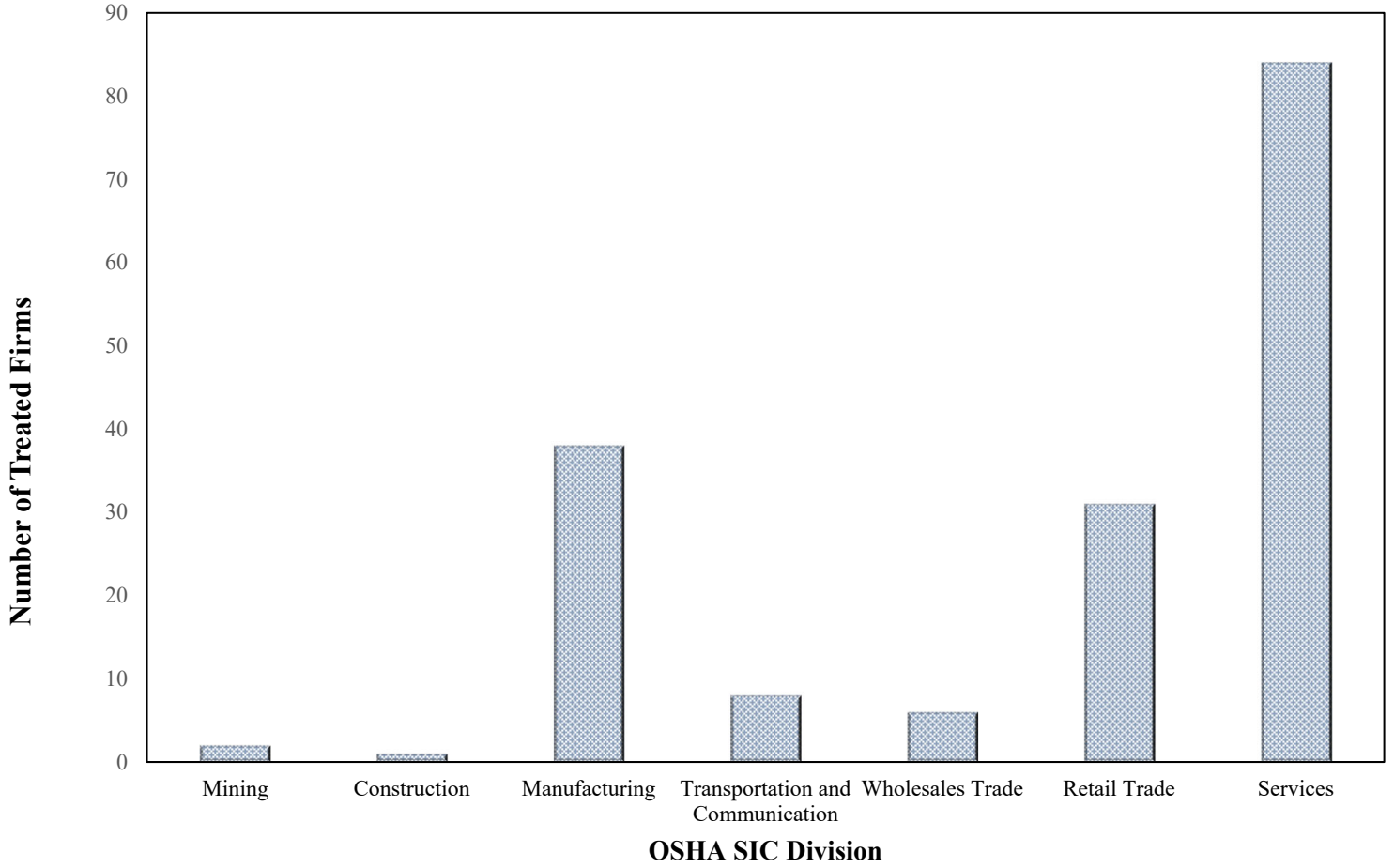


Figure 1 is a graphical representation of the frequency of OSHA SIC Division categories of the firms who make up the treatment group of the sample. These are firms who mentioned the risk of the *Wayfair* ruling in either a 10-K, 10-Q, 8-K, or S-4 filing with the SEC.

Figure 2

Google Trends Popularity Scores: *South Dakota v. Wayfair, Inc*

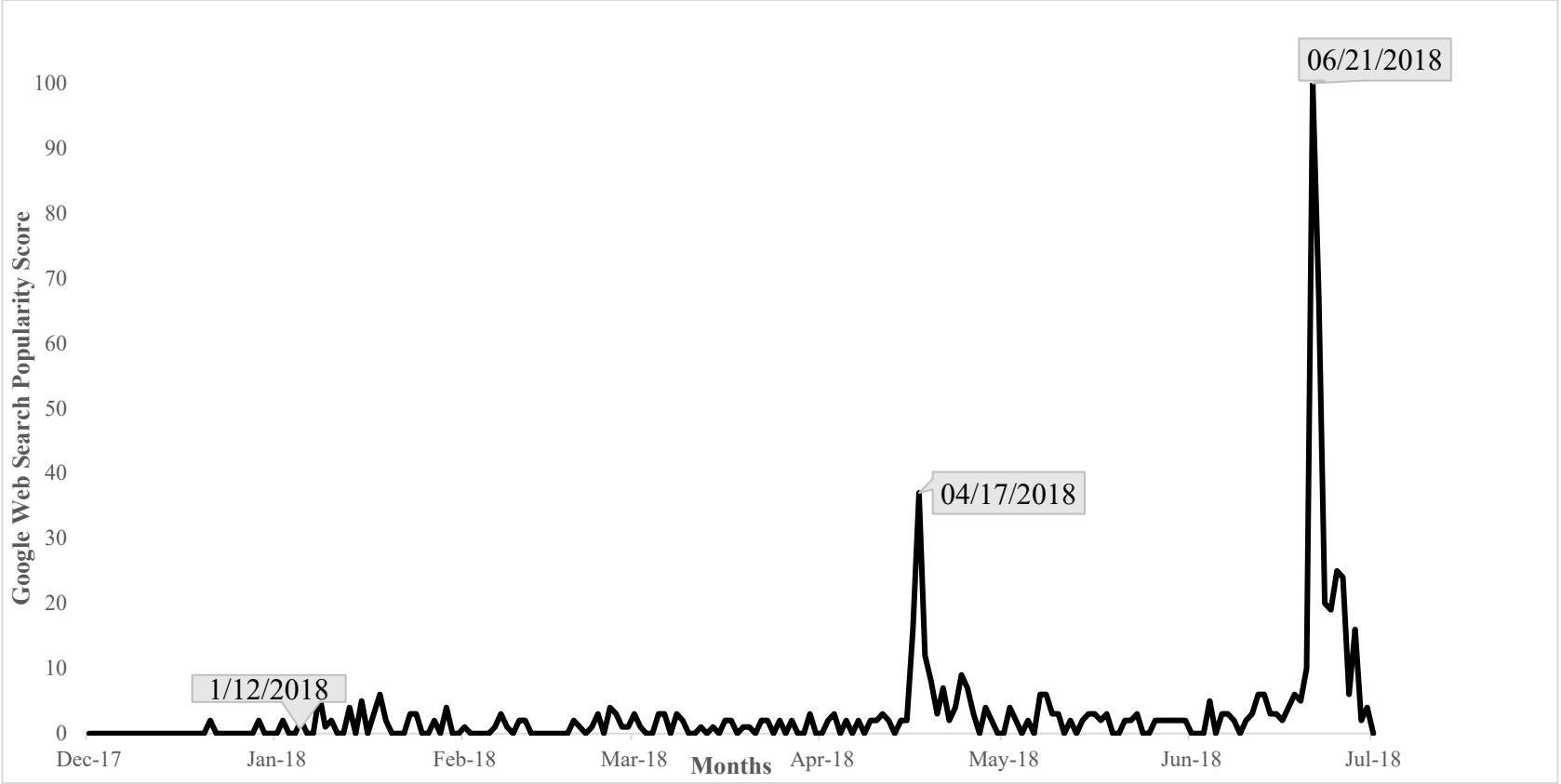


Figure 2 shows a graphical representation of the trend in popularity of Google Web searches for the *South Dakota v. Wayfair* case beginning in December of 2017 through July of 2018. Search terms are given a popularity score between 1 and 100, with 100 representing peak popularity for that search. Dates that are specifically called out in the chart are the *Wayfair* event dates used in the tests of investor and analysts response to the increased probability of changes to the sales tax nexus standards. January 12, 2018 is the date in which the The Court agreed to hear the case. April 17, 2018 is when The Court heard oral arguments, and June 21, 2018 is when the ruling was issued.

Figure 3
Analyst Response Tests - Event Timeline and Windows for Event Indicator Assignments

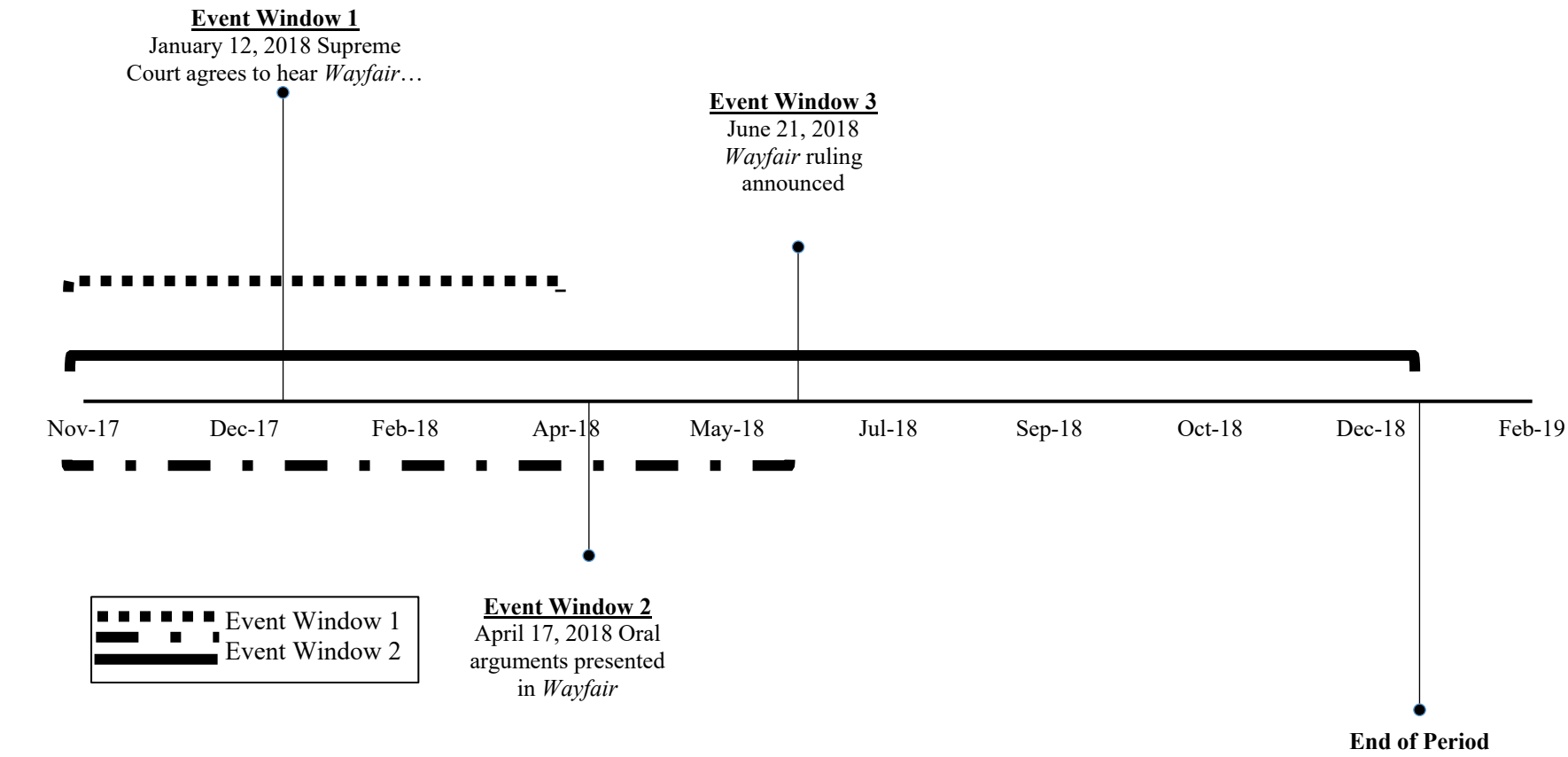


Figure 3 (cont'd)

Example 1:

Analyst A announces a revenue forecast for ABC Inc. on November 1, 2017, for the forecast period end date of December 31, 2018. If Analyst A updates their revenue forecast for ABC Inc. on February 10, 2018, the revised forecast will be assigned to Event Window 1. This is because the previous forecast occured prior to Event Window 1 (January 12, 2018) and the revision occured after Event Window 1, but before Event Window 2 (April 17, 2018).

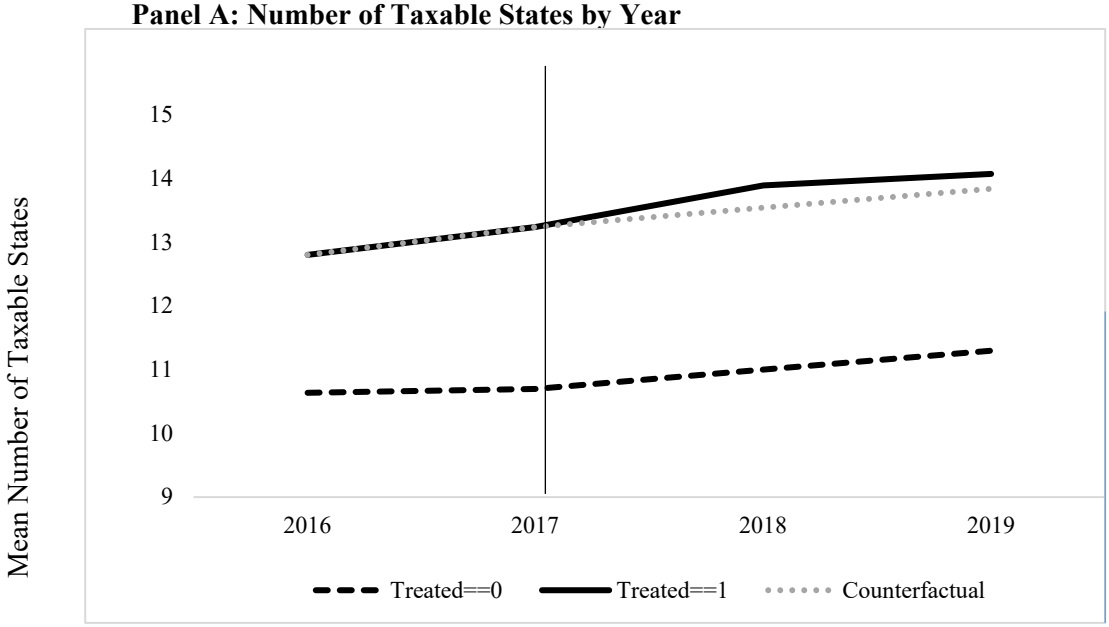
Example 2:

Analyst B announces a revenue forecast for XYZ Company on January 1, 2018, for the forecast period end date of December 31, 2018. If Analyst B updates this revenue forecast for XYZ Company on May 10, 2018, the revised forecast will be assigned to Event Window 2. This is because the original forecast occured prior to Event Window 2 (April 17, 2018) and the revision occured after Event Window 2, but before Event Window 3 (June 21, 2018).

Figure 3 is a graphical representation of how analyst-firm-quarter observations are assigned to each of the three *Wayfair* event dates (January 12, 2018; April 17, 2018; June 21, 2018). Observations are assigned to a specific event period if the $\Delta ANALYST\ FORECAST_{i,j,t,v}$ for that observation straddles an event date. If the $\Delta ANALYST\ FORECAST_{i,j,t,v}$ straddles more than one event date, it is assigned to the later event. See examples above for illustration of Event Window assignment.

Figure 4
Trends in Number of Taxable States

Panel A: Number of Taxable States by Year



Panel B: Change in Number of Taxable States by Year

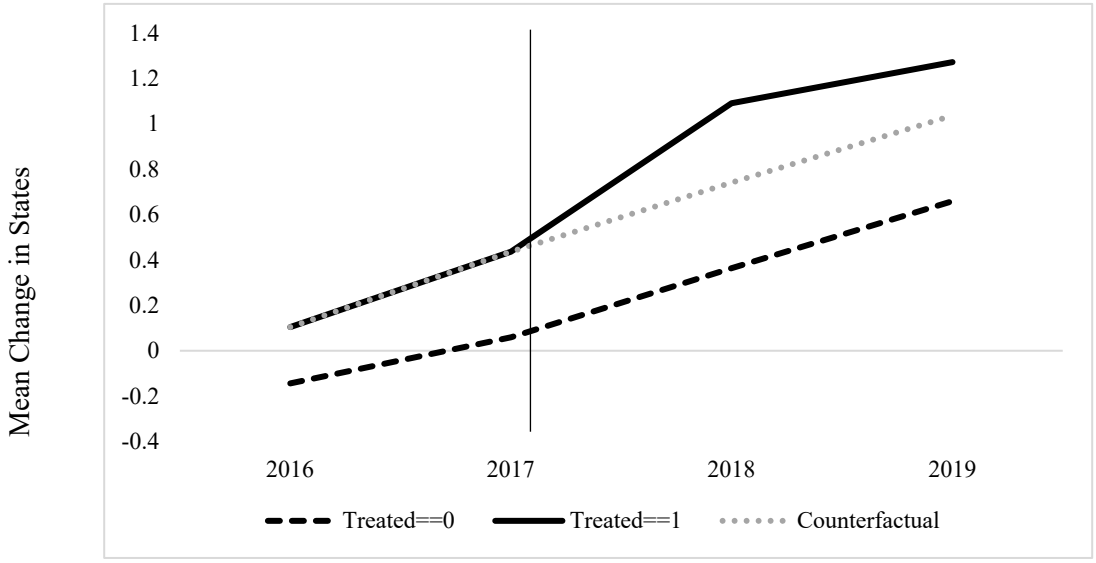


Figure 4 (cont'd)

Figure 4, Panel A examines the mean number states with sales taxes in which firms have locations, partitioned by $TREATED_i$. The y-axis represents the mean number of taxable states for each year and the x-axis represents each year in the sample. The treatment ($POST_t$) begins in 2017. Figure 4, Panel B examines the change in the mean number of states with sales taxes in which firms have locations, partitioned by $TREATED_i$. The y-axis represents the number of locations for each year and the x-axis represents each year in the sample.The treatment ($POST_t$) begins after 2017.The Counterfactual line represents what the growth of the treatment firms would have been in the absence of treatment assuming they continued adding locations at the same rate as the control firms.

Table 1
SIC Codes of Treated Firms

OSHA SIC Division	Major Sub Group 2 Digit SIC	Number of Firms	% of Total	Division %
Division B: Mining				
	13: Oil and Gas Extraction	2	1.18%	
				1.18%
Division C: Construction				
	17: Construction: Special Trade Contractors	1	0.59%	
				0.59%
Division D: Manufacturing				
	21: Tobacco Products	2	1.18%	
	22: Textile Mill Products	1	0.59%	
	23: Apparel and Other Finished Products	2	1.18%	
	25: Furniture and Fixtures	2	1.18%	
	27: Printing, Publishing and Allied Industries	3	1.76%	
	28: Chemicals and Allied Products	5	2.94%	
	29: Petroleum Refining and Related	1	0.59%	
	31: Leather and Leather Products	3	1.76%	
	33: Primary Metal Industries	1	0.59%	
	34: Fabricated Metal Products	3	1.76%	
	35: Industrial, Commercial and Computer Equipment	3	1.76%	
	36: Electronics	3	1.76%	
	37: Transportation Equipment	3	1.76%	
	38: Measuring, Photographic, Medical, Watch Equipment	2	1.18%	
	39: Miscellaneous Manufacturing	4	2.35%	
		38		22.35%
Division E: Transportation, Communication, Electric, Gas, and Sanitary Services				
	41: Local and Suburban Transport	1	0.59%	
	48: Communications	6	3.53%	
	49: Electric, Gas, and Sanitary Services	1	0.59%	
		8		4.71%

Table 1 (cont'd)

Division F: Wholesale Trade			
50: Wholesale Trade-durable Goods	3	1.76%	
51: Wholesale Trade-non-durable Goods	3	1.76%	
	6		3.53%
Division G: Retail Trade			
55: Automotive Dealers and Gasoline Service Stations	2	1.18%	
56: Apparel and Accesssory Stores	9	5.29%	
57: Home Furniture, Furnishings, and Equipment Stores	4	2.35%	
59: Miscellaneous Retail	16	9.41%	
	31		18.24%
Division I: Services			
72: Personal Services	4	2.35%	
73: Business Services	71	41.76%	
87: Engineering, Accounting, Research, Mgmt and Related Services	9	5.29%	
	84		49.41%
	170	100.00%	100.00%

Table 1 represents OSHA SIC Division categories of the firms who make up the treatment group of the sample. These are firms who mentioned the risk of the Wayfair ruling in either a 10-K, 10-Q, 8-K, or S-4 filing with the SEC.

Table 2
Sample Selection

Panel A	**Sample Selection - Investor Response H1(a)**	
Treatment Firms identified from SEC Edgar Searches		170
	Less: Firms not matched in CRSP	(33)
	Treatment Firms matched to CRSP	137
	Less: Firms with missing event study estimation data for Event 1	(29)
Sample of Treated firms for Investor Reaction Event Study - Event 1		108
	Plus: Firms with sufficient estimation data for Event 2	3
Sample of Treated firms for Investor Reaction Event Study - Event 2		111
	Plus: Firms with sufficient estimation data for Event 3	2
Sample of Treated firms for Investor Reaction Event Study - Event 3		113
Panel B	**Sample Selection - Analyst Response H1(b)**	
Observations from WRDS CRSP IBES matching file for all forecast periods end dates after of August 31, 2018 or before June 30, 2019		340,487
	Less: Firms with missing data for dependent and independent variables	(273,882)
Total observations for tests of H1b		66,605
	Treated Firm Observations	1,468
	Control Firm Observations	65,137
	Total Analyst -Firm-Forecast Period End date groups	26,108
Panel C	**Sample Selection - Firm Response H2**	
COMPUSTAT Firms with Name and Address Information		7,115
	Firms unable to match to YTS using Ticker, Name and Address	(3,239)
	Matched YTS and COMPUSTAT Firms (Treated=127, Control=3,704)	3,876
Firm-year observations of YTS for the years 2015-2019		18,645
	Less: Firm-Year observations with insufficient data to calculate firm level controls	(4,412)
	Less: Firm-Year observations for years prior to 2016	(2,183)
	Less: Firm-Year control observations without matching SIC2 treated observations	(1,932)

Table 2 (cont'd)

Less: Firm-Year observations with missing observations for all years used in analysis for H2 (years 2016-2019)	(2,466)
Firm-year observations	7,652
Total number of Treated Firms	83
Total number of Control Firms	1,830

This table reports the sample selection used in the tests of stock market, analyst, and firm reactions to changes in sales tax nexus standards as a result of the *Wayfair* decision.

Table 3
Variable Definitions

Dependent Variables	
CAR	Cumulative Abnormal Return calculated using the market model of the CRSP and S&P 500 market-weighted indices.
CAAR	Mean Cumulative Abnormal Return calculated using the market model based of CRSP and S&P 500 market-weighted indices.
$\Delta ANALYST\ FORECAST_{i,j,t,v}$	The revenue forecast for firm *i*, by analyst *j* announced at time *t*, for the fiscal period end date *v,* less the revenue forecast for firm *i* by analyst *j*, at time *t-1* for the fiscal period end date *v,* scaled by the forecast for firm *i* by analyst *j*, at time *t-1*, for the fiscal period end date *v*.
$NUMTAXABLESTATES_{i,t}$	The number of states that have a state sales tax where firm *i* has locations in year *t*.
$NUMNEWTAXABLESTATES_{i,t}$	The number of new states that have have a sales tax where firm *i* has locations in year *t*.
$PHYSICALPRESENCE_{i,s,t}$	Equal to 1 if firm i has a physical location in state s at time t, 0 otherwise.
$NUMNONTAXABLESTATES_{i,t}$	The number of states that do not have a state sales tax where firm *i* has locations in year *t*. States without sales taxes are Alaska, Delaware, Montana, New Hampshire, and Oregon.
Variables of Interest	
$TREATED_i$	Equal to 1 if the firm is Treated, 0 otherwise.
$WAYFAIREVENT=0_t$	Equal to 0 if the analyst does not revise a firms forecast for any firm period ending from August 31, 2018 through June 30, 2019 during the sample period of analyst announcement dates (September 15, 2017-December 31, 2018).
$WAYFAIREVENT=1_t$	Equal to 1 if the analyst-firm-forecast period end date at time t is after January 12, 2018 but before April 17, 2018, and the analyst-firm-forecast period end date at *t-1* is before January 12, 2018.

Table 3 (cont'd)

$WAYFAIREVENT=2_t$	Equal to 2 if the analyst-firm-forecast period end date at time t is April 17, 2018 but before June 21, 2018, and the analyst-firm-forecast period end date at *t-1* is before April 17, 2018.
$WAYFAIREVENT=3_t$	Equal to 3 if the analyst-firm-forecast period end date at time t is after June 21, 2018 but before December 31, 2018, and the analyst-firm-forecast period end date at *t-1* is before June 21, 2018.

Control Variables - Analyst Tests

$STALE_{i,j,v}$	Natural log of number of days between analysts *j*'s revenue forecast for firm *i*, for forecast period *v*.
$EXP_{j,t}$	Continuous variable calculating the days of experience for each analyst *j* at the time of the revenue forecast, *t*.
$FIRMEXP_{i,j,t}$	Continuous variable calculating the number of days of firm *i* specific experience for each analyst *j* at the time of the revenue forecast, *t*.
$QASSETS_{i,t}$	Natural log of assets for firm *i* from COMPUSTAT for the nearest firm quarter that precedes the analyst forecast date, *t*.
$QLEV_{i,t}$	Long-term debt at the end of the quarter (DLTTQ) scaled by total assets at the end of the quarter(ATQ) for firm *i*, for the nearest firm quarter that precedes the analyst forecast date, *t*.
$QMTB_{i,t}$	Market-to-book ratio at the end of quarter, measured as book value of equity (CEQQ) divided by market value of equity at the end of the quarter (PRCC_FQ x CSHOQ) for firm *i*, for the nearest firm quarter that precedes the analyst forecast date, *t*.
$QROA_{i,t}$	Return on assets for at the end of quarter, measured as the ratio of income before extraordinary items (IBQ) to the average of total assets for the quarter (ATQ) for firm *i*, for the nearest firm quarter that precedes the analyst forecast date, *t*.

Table 3 (cont'd)

Control Variables - Location Tests

$LNUMTAXABLESTATES_{i,t-1}$	The number of states with sales taxes where firm *i* operatates in, in year *t-1*
$LNUMLOCATIONS_{i,t-1}$	The number of locations of firm i in year *t-1*
$LOGAT_{i,t}$	Natural log of firm *i*'s assets at year, *t*
$LEV_{i,t}$	Long-term debt at the end of year *t* (DLTT) scaled by total assets at the end of year *t* (AT) for firm *i*.
$BTM_{i,t-1}$	Market-to-book ratio at the end of year *t-1*, measured as book value of equity (CEQ) divided by market value of equity (PRCC_F x CSHO) for firm *i*, for the previous year, *t-1*.
$ROA_{i,t}$	Return on assets for at the end year *t*, measured as the ratio of income before extraordinary items (IB) to the average of total assets for the year (AT) for firm *i*.
$PPE_{i,t}$	Property Plant and Equipment (PPE) for firm *i*, divided by assets (AT) for year *t.*
$CASHSURP_{i,t}$	The cash from asset-in-place (OANCF-DPC+XRD) divided by assets (AT) in year *t*.
$LOSSCFWD_{i,t}$	Indicator variable set to 1 if firm *i* has a loss carryforward, 0 otherwise.
$SALESGROWTH_{i,t}$	The natural log of current year sales ($SALE_{i,t}$) divided by last years sales ($SALE_{i,t-1}$)
$COMPANYAGE_{i,t}$	The number of years the firm has been in business, defined as the year of the IPO (IPOAGE) less the current fiscal year. If IPOAGE is missing, then it is the number of firm years in COMPUSTAT as of year *t*.
$LOGSGDP_{s,t}$	Natural log of GDP for state *s* in year *t*. Source: U.S. Bureau of Economic Analysis (BEA)
$SALESTAXRATE_{s,t}$	Combined state and local sales tax rates for state *s* in year *t*. Source: Tax Foundation
$HIGHCORPTAX_{s,t}$	Indicator variable set to 1 if the state has a corporate tax rate in the top quartile of state corporate tax rates in year *t*, zero otherwise.

Table 3 (cont'd)

$HIGHSALESTAX_{s,t}$	Indicator variable set to 1 if the state has a sales tax rate in the top quartile of state sales tax rates in year *t*, zero otherwise.
$HIGHSTATEPOP_{s,t}$	Indicator variable set to 1 if the state has a population total in the top quartile of state population totals in year *t*, zero otherwise.

Table 4
Descriptive Statistics - Analyst Forecast Revision

Panel A - Entire Sample (Before Entropy Balancing)

Entire Sample

Variable	N	Mean	Standard Deviation	Q1	Median	Q3
ΔANALYST FORECAST$_{i,j,t,v}$	66,605	-0.0027	0.0600	-0.0200	0.0000	0.0100
STALE $_{i,j,v}$	66,605	3.7500	0.9300	3.2200	4.0800	4.5100
EXP $_{j,t}$	66,605	35.6200	9.5100	30.6100	42.0900	42.5900
FIRMEXP $_{i,j,t}$	66,605	19.1800	13.8600	6.3400	16.9900	30.6300
QASSETS$_{i,t}$	66,605	8.7700	1.7900	7.5900	8.7200	9.9800
QLEV$_{i,t}$	66,605	0.6100	0.2300	0.4500	0.6000	0.7900
QMTB$_{i,t}$	66,605	4.1800	10.1300	1.2900	2.5000	5.3100
QROA$_{i,t}$	66,605	0.0100	0.0400	0.0000	0.0100	0.0300

Panel B - Sample Partitioned on Treated (Before Entropy Balancing)

	Treated=1			***Treated=0***			
	N	Mean	Standard Deviation	N	Mean	Standard Deviation	Difference
ΔANALYST FORECAST$_{i,j,t,v}$	1,468	-0.0020	0.0596	65,137	-0.0027	0.0619	
STALE $_{i,j,v}$	1,468	3.9690	0.8778	65,137	3.7481	0.9289	***
EXP $_{j,t}$	1,468	34.3381	9.5965	65,137	35.6523	9.5014	***
FIRMEXP $_{i,j,t}$	1,468	12.9447	9.8514	65,137	19.3236	13.9047	***
QASSETS$_{i,t}$	1,468	7.3959	1.3232	65,137	8.7963	1.7826	***
QLEV$_{i,t}$	1,468	0.5266	0.1880	65,137	0.6099	0.2340	***
QMTB$_{i,t}$	1,468	5.9600	7.0098	65,137	4.1444	10.1816	***
QROA$_{i,t}$	1,468	0.0115	0.0501	65,137	0.0113	0.0370	

Table 4 (cont'd)

Panel C - Sample Partitioned on Treated (After Entropy Balancing)

	Treated=1			***Treated=0***			
	N	Mean	Standard Deviation	N	Mean	Standard Deviation	Difference
$\Delta ANALYST\ FORECAST_{i,j,t,v}$	1,468	-0.0020	0.0596	65,137	-0.0034	0.0709	
$STALE_{i,j,v}$	1,468	3.9690	0.8778	65,137	3.9690	0.8778	
$EXP_{j,t}$	1,468	34.3381	9.5965	65,137	34.3378	9.5965	
$FIRMEXP_{i,j,t}$	1,468	12.9447	9.8514	65,137	12.9445	9.8516	
$QASSETS_{i,t}$	1,468	7.3959	1.3232	65,137	7.3959	1.3232	
$QLEV_{i,t}$	1,468	0.5266	0.1880	65,137	0.5266	0.1880	
$QMTB_{i,t}$	1,468	5.9600	7.0098	65,137	5.9633	7.0142	
$QROA_{i,t}$	1,468	0.0113	0.0511	65,137	0.0113	0.0511	

Table 4 presents the descriptive statistics of 1,468 analyst-firm-quarter observations for treated firms and 65,137 analyst-firm-quarter observations for control firms. All continuous variables are winsorized (reset) at the 1st and 99th percentiles. Panel A reports the descriptive statistics for the Entire Sample. Panel B reports the descriptive statistics for the Treated Sample and Untreated Sample respectively. Differences in means between the Treated and Untreated firms are tested in Panel B (***=p<0.01). Panel C reports the descriptive statistics after Entropy Balancing. Note: There are no significant differences between the Treated and Untreated groups in Panel C (after entropy balancing). All variables are defined in Table 3.

Table 5
Descriptive Statistics - Firm Location Tests

Panel A	*Entire Sample*					
Variable	N	Mean	Standard Deviation	Q1	Median	Q3
$NUMTAXABLESTATES_{i,t}$	7,652	11.9817	12.9089	2.0000	6.0000	18.0000
$NUMNEWTAXABLESTATES_{i,t}$	7,652	0.1875	0.6247	0.0000	0.0000	0.0000
$TREATED_i$	7,652	0.0434	0.2037	0.0000	0.0000	0.0000
$LNUMLOCATIONS_{i,t}$	7,652	100.2090	278.3635	3.0000	13.0000	63.0000
$LOGAT_{i,t}$	7,652	6.2540	2.5672	4.3984	6.4974	8.0927
$LEV_{i,t}$	7,652	0.2323	0.2650	0.0104	0.1854	0.3494
$MTBi_{,t-1}$	7,652	3.2306	11.6709	1.1253	2.2349	4.3952
$ROA_{i,t}$	7,652	-0.2232	1.3469	-0.0989	0.0197	0.0668
$PPE_{i,t}$	7,652	0.2241	0.2349	0.0577	0.1344	0.2972
$CASHSURP_{i,t}$	7,652	-0.0236	0.4860	-0.0151	0.0470	0.1146
$LOSSCFWD_{i,t}$	7,652	0.7588	0.4279	1.0000	1.0000	1.0000
$SALESGROWTH_{i,t}$	7,652	1.1774	0.7955	0.9629	1.0525	1.1732
$COMPANYAGE_{i,t}$	7,652	27.3313	18.2795	12.0000	24.0000	36.0000

Panel B	*Treated=1*			*Treated=0*			
Variable	N	Mean	Standard Deviation	N	Mean	Standard Deviation	Diff
$NUMTAXABLESTATES_{i,t}$	332	14.6024	14.2710	7,320	11.8628	12.8321	***
$NUMNEWTAXABLESTATES_{i,t}$	332	0.3614	1.1002	7,320	0.1796	0.5931	***
$LNUMLOCATIONS_{i,t}$	332	161.0723	394.1809	7,320	97.4485	271.6589	***
$LOGAT_{i,t}$	332	6.6190	1.7649	7,320	6.2374	2.5966	**
$LEV_{i,t}$	332	0.2247	0.2573	7,320	0.2327	0.2653	
$MTB_{i,t-1}$	332	3.1975	6.8538	7,320	3.2321	11.8435	
$ROA_{i,t}$	332	-0.0084	0.1543	7,320	-0.2330	1.3759	**
$PPE_{i,t}$	332	0.1711	0.1650	7,320	0.2265	0.2373	***
$CASHSURP_{i,t}$	332	0.0801	0.1220	7,320	-0.0283	0.4957	***
$LOSSCFWD_{i,t}$	332	0.7741	0.4188	7,320	0.7581	0.4283	
$SALESGROWTH_{i,t}$	332	1.0846	0.4243	7,320	1.1816	0.8081	**
$COMPANYAGE_{i,t}$	332	19.4398	13.9388	7,320	27.6892	18.3727	***

Table 5 presents the descriptive statistics for 83 treatment firms and 1,830 control firms for the years 2016-2019 (7,652 total firm-year observations).All continuous variables are winsorized (reset) at the 1st and 99th percentiles. Panel A reports the descriptive statistics for the Entire Sample. Panel B reports the descriptive statistics for theTreated firms (Treated==1) and Control Firms (Treated==0). Differences in means between the Treated and Untreated firms are tested in Panel B (***=p<0.01, **=p<0.05). All variables are defined in Table 3.

Table 6
Mean Cumulative Abnormal Returns on *Wayfair* event dates

Panel A: Equally-Weighted Indices

Dependent Var. = CAAR	Window	N	CAAR	B.M.P. Z-Score		Generalized Sign	
Wayfair Event : January 12, 2018							
	(0,+1)	108	-1.46%	-4.827	***	-3.919	***
	(0,+5)	108	-2.39%	-3.693	***	-4.112	***
Wayfair Event : April 17, 2018							
	(0,+1)	111	0.94%	2.702	***	1.578	*
	(0,+5)	111	1.81%	3.047	***	2.147	**
Wayfair Event : June 21, 2018							
	(0,+1)	113	-1.09%	-3.189	***	-2.275	**
	(0,+5)	113	-2.24%	-3.641	***	-3.780	***

Panel B: Value-Weighted Indices

Dependent Var. = CAAR	Window	N	CAAR	B.M.P. Z-Score		Generalized Sign	
Wayfair Event : January 12, 2018							
	(0,+1)	108	-1.23%	-4.214	***	-3.352	***
	(0,+5)	108	-2.17%	-4.340	***	-4.122	***
Wayfair Event : April 17, 2018							
	(0,+1)	111	0.79%	2.257	*	1.734	*
	(0,+5)	111	1.43%	2.225	**	0.974	**
Wayfair Event : June 21, 2018							
	(0,+1)	113	-1.23%	-3.861	***	-2.872	**
	(0,+5)	113	-2.30%	-4.054	***	-3.624	***

Table 6 presents windows of abnormal market returns around Wayfair event dates. In Panel A the market model is used to estimate the abnormal returns by calculating the returns of the Treated firms and comparing them with the returns of a reference market and using equally-weighted indices to estimate the mean cumulative abnormal returns (*CAAR*). In Panel B the market model is used to estimate the abnormal returns by calculating the returns of the Treated firms and comparing them with the returns of a reference market and using value-weighted indices to estimate the mean cumulative abnormal returns (*CAAR*). The symbols *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively, using a generic one-tail test.

Table 7
Analyst Revenue Forecast Revisions following *Wayfair* Events

	Coefficient (Std. Error)	
Variables	$\Delta ANALYST\ FORECAST_{i,j,t,v}$	
$TREATED=1_i$	0.0060	**
	(0.0024)	
$WAYFAIREVENT=1_t$	0.0110	***
	(0.0030)	
$WAYFAIREVENT=2_t$	0.0093	***
	(0.0021)	
$WAYFAIREVENT=3_t$	0.0039	**
	(0.0018)	
$\mathbf{TREATED=1_i*WAYFAIREVENT=1_t}$	**-0.0252**	***
	(0.0069)	
$\mathbf{TREATED=1_i*WAYFAIREVENT=2_t}$	**-0.0067**	
	(0.0045)	
$\mathbf{TREATED=1_i*WAYFAIREVENT=3_t}$	**-0.0102**	**
	(0.0045)	
$STALE_{i,j,v}$	-0.0006	
	(0.0011)	
$FIRMEXP_{i,j,t}$	-0.0003	***
	0.0000	
$EXP_{i,t}$	-0.0003	***
	0.0000	
$QASSETS_{i,t}$	-0.0005	
	(0.0009)	
$QLEV_{i,t}$	0.0005	
	(0.0039)	
$QMTB_{i,t}$	0.0007	***
	(0.0001)	
$QROA_{i,t}$	0.2496	***
	(0.0255)	
Constant	0.0067	
	(0.0087)	
Observations	66,605	
R^2	0.0491	
Analyst FE	Yes	

Table 7 (cont'd)

Table 7 presents results for the OLS regression examining changes in analyst revenue forecasts *(*Δ*ANALYST FORECAST*$_{i,j,t}$*)* over three *Wayfair* event dates. All continuous variables are winsorized (reset) at the 1st and 99th percentiles. Standard errors are robust to heteroscedasticity and clustered at the analyst level. Variables are defined in Table 3. The symbols *, **, and *** denote statistical significance at 0.10, 0.05, and 0.01 levels using a two-tailed test.

Table 8
Analyst Revenue Forecast Revisions following *Wayfair* Events - Shorter Event Assignment Windows

	Coefficient (Std. Error)		Coefficient (Std. Error)	
	(1)		(2)	
Variables	***ΔANALYST FORECAST$_{i,j,t,v}$***		***ΔANALYST FORECAST$_{i,j,t,v}$***	
TREATED=1$_i$	0.0105		0.0060	
	(0.0077)		(0.0055)	
WAYFAIREVENT=1$_t$	-0.0097	**	-0.0087	**
	(0.0045)		(0.0043)	
WAYFAIREVENT=2$_t$	-0.0131	***	-0.0114	***
	(0.0043)		(0.0038)	
WAYFAIREVENT=3$_t$	-0.0208	***	-0.0154	***
	(0.0048)		(0.0042)	
TREATED=1$_i$*WAYFAIREVENT=1$_t$	**-0.0267**	**	**-0.0276**	***
	(0.0121)		**(0.0091)**	
TREATED=1$_i$*WAYFAIREVENT=2$_t$	**-0.0113**		**-0.0068**	
	(0.0085)		**(0.0064)**	
TREATED=1$_i$*WAYFAIREVENT=3$_t$	**-0.0079**		**-0.0004**	
	(0.0092)		**(0.0066)**	
Observations	20,690		32,270	
R^2	0.0461		0.0305	
Control Variables	Yes		Yes	
Analyst FE	Yes		Yes	

Table 8 presents results for the OLS regression examining changes in analyst revenue forecasts ($\Delta ANALYSTFORECAST_{i,j,t,v}$) over three *Wayfair* event dates limited to time windows around each $WAYFAIREVENT_t$. Column (1) reports the regression results when observations are limited to forecasts that are made no more than 30 days before and revised no more than 30 days after each $WAYFAIREVENT_t$. Column (2) reports the regression results when observations are limited to forecasts that are made no more than 45 days before and revised no more than 45 days after $WAYFAIREVENT_t$. All continuous variables are winsorized (reset) at the 1st and 99th percentiles. Standard errors are robust to heteroscedasticity and clustered at the analyst level. Variables are defined in Table 3. The symbols *, **, and *** denote statistical significance at 0.10, 0.05, and 0.01 levels using a two-tail test.

Table 9
Firm Location Decisions - Two-way ANOVA of Dependent Variables

Variables	Partial SS	Degrees of Freedom	Mean Squared Error	F-Stat	
Panel A					
NUMTAXABLESTATES$_{i,t}$					
Model	603,401.850	3	201133.9500	869.1500	***
TREATED$_i$	29,580.556	1	29580.5560	127.8300	***
POST$_t$	28,320.306	1	10215.5730	122.3800	***
TREATED$_i$*POST$_t$	**10,215.573**	**1**		**44.1400**	***
Residual	1,770,081.200	7,649	231.4134		
Observations	7,652				
Root MSE	15.2120				
R^2	0.2542				
Adj R2	0.2539				
Panel B					
NUMNEWTAXSTATES$_{i,t}$					
Model	168.6120	3	56.2040	139.2900	***
TREATED$_i$	25.7978	1	25.7978	63.9300	***
POST$_t$	6.6189	1	6.6189	16.4000	***
TREATED$_i$*POST$_t$	**2.4269**	**1**	**2.4269**	**6.0100**	**
Residual	3086.3880	7,649	0.4035		
Observations	7,652				
Root MSE	0.635218				
R^2	0.0518				
Adj R2	0.0514				

Panel A presents results of an ANOVA results using *NUMTAXABLESTATES$_{i,t}$* as the dependent variable and *TREATED$_i$*, *POST$_t$* and *TREATED$_i$*POST$_t$* as factors. Panel B presents two-way ANOVA results using *NUMNEWTAXABLESTATES$_{i,t}$* as the dependent variable and *TREATED$_i$*, *POST$_t$* and *TREATEDi*POST$_t$* as factors. All continuous variables are winsorized (reset) at the 1st and 99th percentiles. Variables are defined in Table 3. The symbols *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively, using a two-tail test.

Table 10
Locations in States with Sales Taxes - Conditional Fixed Effects Poisson Regression

Variables		Coefficient (Std. Error)		Coefficient (Std. Error)	
		(1)		(2)	
	Predicted Sign	***NUMTAXABLE STATES$_{i,t}$***		***NUMNEWTAX STATES$_{i,t}$***	
POST$_t$	(+)	0.0190	***	1.2004	***
		(0.0017)		(0.1192)	
TREATED$_i$*POST$_t$	**(+)**	**0.0184**	*****	**0.8598**	*****
		(0.0116)		**(0.5550)**	
LNUMTAXABLESTATES$_{i,t-1}$	(?)	0.0293	***	-0.6036	***
		(0.0022)		(0.1004)	
LNUMLOCATIONS$_{i,t-1}$	(?)	-0.0001	**	-0.0026	
		0.0000		(0.0033)	
LOGAT$_{i,t}$	(?)	0.0095	**	0.3843	**
		(0.0037)		(0.1795)	
LEV$_{i,t}$	(?)	-0.0090		0.1948	
		(0.0086)		(0.4725)	
MTB$_{i,t-1}$	(?)	0.0001		0.0070	*
		(0.0001)		(0.0047)	
ROA$_{i,t}$	(?)	0.0033		0.0792	
		(0.0030)		(0.0915)	
PPE$_{i,t}$	(?)	0.0146		2.7949	**
		(0.0172)		(1.4025)	
CASHSURP$_{i,t}$	(?)	-0.0108	*	-0.9244	**
		(0.0067)		(0.4213)	
LOSSCFWD$_{i,t}$	(?)	-0.0022		-0.2033	
		(0.0030)		(0.1771)	
SALESGROWTH$_{i,t}$	(?)	-0.0022	*	0.1295	*
		(0.0016)		(0.0895)	
COMPANYAGE$_{i,t}$	(?)	-0.0047	**	-1.0359	***
		(0.0021)		(0.1453)	
Observations		7,638		2,878	
Observations dropped because all zeroes		14		4,774	

Table 10 (cont'd)

Wald χ^2 (13 df, N=1,498)	1075.2200	328.2100
Prob>χ^2	0.0000	0.0000
Firm FE	Yes	Yes

Column (1) of this table presents results for the fixed effects GLM Conditional Poisson regression of the dependent variable $NUMTAXABLESTATES_{i,t}$. Column (2) presents results for the fixed effects GLM Conditional Poisson regression of the dependent variable $NUMNEWTAXABLESTATES_{i,t}$. All continuous variables are winsorized (reset) at the 1st and 99th percentiles. Standard errors are robust to heteroscedasticity and clustered at the firm level. Variables are defined in Table 3. The symbols *, **, and *** denote one-tailed statistical significance at the 0.10, 0.05, and 0.01 levels.

Table 11
Number of New Locations in States Without Sales Taxes - Falsification Test

Variables	Coefficient (Std. Error)	
	NUMNONTAXABLESTATES$_{i,t}$	
POST$_t$	3.7174	***
	(0.8413)	
TREATED$_i$*POST$_t$	**0.5750**	
	(0.7242)	
LNUMTAXABLESTATES$_{i,t-1}$	-7.3574	***
	(1.2818)	
LNUMLOCATIONS$_{i,t-1}$	0.0093	
	(0.0058)	
LOGAT$_{i,t}$	-0.1033	
	(0.8558)	
LEV$_{i,t}$	1.4135	
	(4.7149)	
MTB$_{i,t-1}$	-0.1727	
	(1.0267)	
ROA$_{i,t}$	-2.0302	
	(4.1689)	
PPE$_{i,t}$	0.0067	
	(5.1383)	
CASHSURP$_{i,t}$	8.6374	**
	(4.2513)	
LOSSCFWD$_{i,t}$	-0.6384	
	(0.8173)	
SALESGROWTH$_{i,t}$	1.1184	
	(1.6111)	
COMPANYAGE$_{i,t}$	1.5916	***
	(0.6052)	

Table 11 (cont'd)

Observations	352
Observations dropped because all zeroes	7292
Wald χ^2 (13 df, N=1,498)	94.2000
Prob>χ^2	0.0000
Firm FE	Yes

Table 11 presents results for the fixed effects GLM Conditional Poisson regression of the dependent variable $NUMNONTAXABLESTATES_{i,t}$. All continuous variables are winsorized (reset) at the 1st and 99th percentiles. Standard errors are robust to heteroscedasticity and clustered at the firm level. Variables are defined in Table 3. The symbols *, **, and *** denote two-tailed statistical significance at the 0.10, 0.05, and 0.01 levels.

Table 12
Number of Taxable States, by Industry – Conditional Fixed Effects Poisson Regression

Variables	Coefficient (Std. Error)		Coefficient (Std. Error)		Coefficient (Std. Error)		Coefficient (Std. Error)	
	(1)		(2)		(3)		(4)	
	NUMTAXABLE STATES$_{i,t}$ **Manufacturing**		***NUMTAXABLE STATES$_{i,t}$*** **Retail/Wholesale**		***NUMTAXABLE STATES$_{i,t}$*** **Services**		***NUMTAXABLE STATES$_{i,t}$*** **Other**	
POST$_t$	0.0247	***	0.0057	***	0.0357	***	0.0126	***
	(0.0023)		(0.0023)		(0.0068)		(0.0021)	
TREATED$_i$*POST$_t$	**0.0027**		**0.0328**	*	**0.0257**	*	**0.0197**	
	(0.0135)		**(0.0235)**		**(0.0199)**		**(0.0216)**	
Observations	4,108		728		1496		1312	
Observations dropped because all zeroes	0		0		4		4	
Wald χ^2 (13 df, N=1,498)	560.5400		248.0200		405.9300		212.6600	
Prob>χ^2	0.0000		0.0000		0.0000		0.0000	
Firm FE	Yes		Yes		Yes		Yes	
Control Variables	Yes		Yes		Yes		Yes	

Table 12 presents results for the fixed effects GLM Conditional Poisson regression of the dependent variable *NUMNEWTAXABLESTATES$_{i,t}$* partitioned by two-digit OSHA SIC codes. Column (1) of this table presents results for manufacturing firms as identified by their OSHA two-digit SIC numbers (SIC 21 through 39). Column (2) of this table presents results for retail and wholesale firms as identified by their OSHA two-digit SIC numbers (SIC 50 through 59). Column (3) of this table presents results for service firms as identified by their OSHA two-digit SIC numbers (SIC 71 through 87). Column (4) of this table presents results for all other firms not identified in Columns (1), (2) and (3). All continuous variables are winsorized (reset) at the 1st and 99th percentiles. Standard errors are robust to heteroscedasticity and clustered at the firm level. Variables are defined in Table 3. The symbols *, **, and *** denote one-tailed statistical significance at the 0.10, 0.05, and 0.01 levels.

Table 13
Physical Presence: Firm-State-Year Analysis

Variables	Coefficient (Std. Error)		Coefficient (Std. Error)		Coefficient (Std. Error)	
	(1)		(2)		(3)	
	PHYSICAL PRESENCE $TAX_{i,s,t}$		***PHYSICAL PRESENCE $TAX_{i,s,t}$***		***PHYSICAL PRESENCE $TAX_{i,s,t}$***	
	LPM		**CRE Logit**		**CRE Probit**	
$TREATED_i$			0.1042	***	0.1172	***
			(0.0014)		(0.0055)	
$POST_t$	-0.2411	***	0.1030	***	0.1162	***
	(0.0001)		(0.0014)		(0.0016)	
$TREATED_i$*$POST_t$	**0.2445**	***	**0.2389**	***	**0.2399**	***
	(0.0015)		**(0.0044)**		**(0.0044)**	
Observations	390,252		390,252		390,252	
R^2	0.0080					
	0.0847					
	0.0837					
Pseudo R^2			0.4768		0.4755	
Wald χ^2 (51 df)			24310.5900		27835.4900	
Prob>χ^2			0.0000		0.0000	
Firm FE	Yes		No		No	
Control Variables	Yes		Yes		Yes	
CRE Control Variables	No		Yes		Yes	

Table 13 for the sample of the treatment and control firms for location testing. Each firm *i* has an observation for each state *s* for each year *t* in the sample (2016-2019). This results in each firm having 204 observations in the sample (51*4)=204). Column (1) of this table presents the fixed effects linear probability model (LPM) OLS regression of the dependent variable $PHYSICALPRESENCE_{i,s,t}$. Column (2) presents the marginal effects from the correlated random effects logit regression of the dependent variable $PHYSICALPRESENCE_{i,s,t}$. Column (3) of this table presents the marginal effects from the correlated random effects probit of the dependent variable $PHYSICALPRESENCE_{i,s,t}$. All continuous variables are winsorized (reset) at the 1st and 99th percentiles. Standard errors are robust to heteroscedasticity and clustered at the firm-state level. Variables are defined in Table 3. The symbols *, **, and *** denote 2-tailed statistical significance at the 0.10, 0.05, and 0.01 levels.

Table 14
State Level Demographic Information For States with Sales Taxes

Panel A	(1)		(2)
State Corporate Income Tax Rate	**Lowest**		**Highest**
Wyoming	0.00%	Iowa	12.00%
South Dakota	0.00%	Pennsylvania	9.99%
Nevada	0.00%	Minnesota	9.80%
Washington	0.00%	Illinois	9.50%
Ohio	0.00%	New Jersey	9.00%
Texas	0.00%	Maine	8.93%
North Carolina	3.00%	California	8.84%
North Dakota	4.31%	Vermont	8.50%
Colorado	4.63%	D.C.	8.25%
Arizona	4.90%	Connecticut	8.25%
Panel B			
Combined State and Local Sales Tax Rate			
Hawaii	4.35%	Louisiana	10.02%
Wisconsin	5.42%	Tennessee	9.46%
Wyoming	5.46%	Arizona	9.41%
Maine	5.50%	Washington	9.18%
Virginia	5.63%	Alabama	9.10%
D.C.	5.75%	Oklahoma	8.91%
Maryland	6.00%	Illinois	8.70%
Kentucky	6.00%	Kansas	8.68%
Michigan	6.00%	California	8.54%
Idaho	6.03%	New York	8.49%
Panel C			
State Population			
Wyoming	577,737	California	39,557,045
Vermont	626,299	Texas	28,701,845
D.C.	702,455	Florida	21,299,325
North Dakota	760,077	New York	19,542,209
South Dakota	882,235	Pennsylvania	12,807,060
Rhode Island	1,057,315	Illinois	12,741,080
Maine	1,338,404	Ohio	11,689,442
Hawaii	1,420,491	Georgia	10,519,475
Idaho	1,754,208	North Carolina	10,383,620
West Virginia	1,805,832	Michigan	9,995,915

Table 14 (cont'd)

Panel A of this Table presents the states with the ten lowest and the ten highest state corporate tax rates in 2018. Panel B presents the states with the ten lowest and the ten highest combined state and local sales state tax rates in 2018. Panel C presents the states with the ten lowest and the ten highest state population totals as of 2018. Sources: Source: Tax Foundation (https://taxfoundation.org) and United States Census Bureau (https://www.census.gov/newsroom/press-kits/2018/pop-estimates-national-state.html).

Table 15
Top 10 Number of New State Locations

(1)		(2)		(3)	
Total		**Treated**		**Control**	
New York	29	New York	3	New York	26
Pennsylvania	27	Texas	3	Pennsylvania	26
Illinois	22	Maryland	3	Illinois	21
North Carolina	22	Missouri	3	North Carolina	21
Massachusetts	22	Massachusetts	2	Massachusetts	20
California	21	California	3	California	18
Florida	20	Florida	3	Arizona	18
Georgia	20	Georgia	2	Georgia	18
Colorado	19	Alabama	2	Colorado	18
Connecticut	19	New Jersey	3	Connecticut	18

Table 15 presents the top 10 number of new firm-state locations established in 2018 and 2019 (*POST=1*). Column (1) of this table presents the top 10 new firms-state locations for the entire sample. Column (2) of this table presents the top 10 new firms-states locations established after *Wayfair (POST=1)* for treated firms ($Treated_i=1$). Column (3) of this table presents the top ten new firms-state locations established for control firms ($Treated_i=0$) after *Wayfair* (*POST=1*).

Table 16
Cross-sectional Analysis of Number of New Tax States after *Wayfair*

	(1)	(2)	(3)
	PHYSICAL PRESENCE$_{i,s,t}$	***PHYSICAL PRESENCE$_{i,s,t}$***	***PHYSICAL PRESENCE$_{i,s,t}$***
	LPM	**CRE Logit**	**CRE Probit**
Panel A - Corporate State Taxes			
TREATED$_i$*POST$_t$ *HIGHCORPTAX$_{s,t}$	**-0.0031**	**-0.0058**	**-0.0086**
	(0.0038)	**(0.0081)**	**(0.0097)**
Observations	390,252	390,252	390,252
R^2 Within	0.0266		
Between	0.3946		
Overall	0.3898		
Pseudo R^2		0.4769	0.4756
Wald χ^2 (51 df)		24337.1400	27898.8400
Prob>χ^2		0.0000	0.0000
Control Variables	Yes	Yes	Yes
CRE Control Variables	No	Yes	Yes
Panel B - State Sales Tax			
TREATED$_i$*POST$_t$ *HIGHSALESTAX$_{s,t}$	**0.0360**	**-0.0832**	**-0.0417**
	(0.0043)	**(0.0489)**	**(0.0274)**
Observations	390,252	390,252	390,252
R^2 Within	0.0265		
Between	0.3888		
Overall	0.3841		
Pseudo R^2		0.4535	0.4528
Wald χ^2 (51 df)		24640.1200	27923.8400
Prob>χ^2		0.0000	0.0000
Control Variables	Yes	Yes	Yes
CRE Control Variables	No	Yes	Yes

Table 16 (cont'd)

		(1)		(2)		(3)	
		PHYSICAL PRESENCE$_{i,s,t}$		***PHYSICAL PRESENCE$_{i,s,t}$***		***PHYSICAL PRESENCE$_{i,s,t}$***	
		LPM		**CRE Logit**		**CRE Probit**	
Panel C - State Population							
TREATED$_i$*POST$_t$							
****HIGHSTATEPOP$_{s,t}$***		**0.0162**	***	**0.0781**	**	**0.0482**	**
		(0.0049)		**(0.0380)**		**(0.0211)**	
Observations		390,252		390,252		390,252	
R^2	Within	0.0087					
	Between	0.1025					
	Overall	0.1010					
Pseudo R^2				0.3498		0.3475	
Wald χ^2 (51 df)				20496.9200		22017.4800	
Prob>χ^2				0.0000		0.0000	
Control Variables		Yes		Yes		Yes	
CRE Control Variables		No		Yes		Yes	

Table 16 presents the triple-interaction of *TREATED$_i$* and *POST$_t$* and the various determinants of state location decisions between treated and control groups. Panel A of this table reports the marginal effects of high state corporate taxes on treated firm state location decisions (*TREATED$_i$*POST$_t$*HIGHCORPTAX$_{s,t}$*) . Panel B examines the effect of combined state and local tax rate on treated firm state location decisions (*TREATED$_i$*POST$_t$*HIGHSALESTAX$_{s,t}$*). Panel C presents the effect of state population on treated firm state location decisions (*(TREATED$_i$*POST$_t$*HIGHSALESTAX$_{s,t}$)*.Standard errors are robust to heteroscedasticity and clustered at the firm-state level. Variables are defined in Table 3. The symbols *, **, and *** denote 2-tailed statistical significance at the 0.10, 0.05, and 0.01 levels.

REFERENCES

REFERENCES

Agarwal, S., Marwell, N., & McGranahan, L. (2017). Consumption Responses to Temporary Tax Incentives: Evidence from State Sales Tax Holidays. *American Economic Journal. Economic Policy*, *9*(4), 1–27. http://dx.doi.org.proxy1.cl.msu.edu/10.1257/pol.20130429

Alm, J., & Melnik, M. I. (2012). Cross-Border Shopping and State Use Tax Liabilities: Evidence from eBay Transactions. *Public Budgeting & Finance*, *32*(1), 5–35. https://doi.org/10.1111/j.1540-5850.2011.00999.x

Anderson, E. T., Fong, N. M., Simester, D. I., & Tucker, C. E. (2010). How Sales Taxes Affect Customer and Firm Behavior: The Role of Search on the Internet. *Journal of Marketing Research*, *47*(2), 229–239. https://doi.org/10.1509/jmkr.47.2.229

Argyres, N., Mahoney, J. T., & Nickerson, J. (2019). Strategic responses to shocks: Comparative adjustment costs, transaction costs, and opportunity costs. *Strategic Management Journal*, *40*(3), 357–376. https://doi.org/10.1002/smj.2984

Avalara. 2021. (2021, June 28). *Sales & Use Tax Nexus—Avalara*. Www.Avalara.Com. https://www.avalara.com/us/en/learn/nexus.html

Baker, S. R., Johnson, S., & Kueng, L. (2021). Shopping for Lower Sales Tax Rates. *American Economic Journal: Macroeconomics*, *13*(3), 209–250. https://doi.org/10.1257/mac.20190026

Bannasch, M. (2018, January 22). *The Attempt to Kill Quill is Alive and Well*. Rhemann. https://www.rehmann.com/resources-insights/business-wisdom-2/item/2210-the-attempt-to-kill-quill-is-alive-and-well

Baugh, B., Ben-David, I., & Park, H. (2018). Can Taxes Shape an Industry? Evidence from the Implementation of the "Amazon Tax." *The Journal of Finance*, *73*(4), 1819–1855. https://doi.org/10.1111/jofi.12687

BDO. 2018. (n.d.). *The State Income Tax Consequences of Wayfair*. BDO. Retrieved July 8, 2021, from https://www.www.bdo.com/insights/tax/state-and-local-tax/the-state-income-tax-consequences-of-wayfair

Beem, R., & Bruce, D. (2021). Failure to launch: Measuring the impact of sales tax nexus standards on business activity. *Journal of Public Economics*, *201*, 104476. https://doi.org/10.1016/j.jpubeco.2021.104476

Boehmer, E., Masumeci, J., & Poulsen, A. B. (1991). Event-study methodology under conditions of event-induced variance. *Journal of Financial Economics*, *30*(2), 253–272. https://doi.org/10.1016/0304-405X(91)90032-F

Bravin, J. (2018, April 17). Supreme Court Weighs Internet Sales-Tax Case. *Wall Street Journal*. https://www.wsj.com/articles/supreme-court-weighs-internet-sales-tax-case-1523990816

Bravin, J., & Kendall, B. (2022, May 3). Roe v. Wade May Be Overturned, a Leaked Draft of Supreme Court Opinion Indicates. *Wall Street Journal*.

https://www.wsj.com/articles/leaked-draft-of-supreme-court-opinion-indicates-roe-v-wade-may-be-overturned-11651554510

Brown, S. J., & Warner, J. B. (1980). Measuring security price performance. *Journal of Financial Economics*, *8*(3), 205–258. https://doi.org/10.1016/0304-405X(80)90002-1

Brown, S. J., & Warner, J. B. (1985). Using daily stock returns: The case of event studies. *Journal of Financial Economics*, *14*(1), 3–31. https://doi.org/10.1016/0304-405X(85)90042-X

Bruce, D., Fox, W. F., & Luna, L. (2009). State and local government sales tax revenue losses from electronic commerce. *State Tax Notes*, *52*(7), 537–558.

Bruce, D., Fox, W. F., & Luna, L. (2015). E-tailer sales tax nexus and state tax policies. *National Tax Journal*, *68*(3S), 735–766.

Bump, P. (2013, June 6). *Why Hasn't SCOTUS Sprung a Leak Yet?* The Atlantic. https://www.theatlantic.com/politics/archive/2013/06/supreme-court-leaks/313943/

Carter, B. S. L. (2017, July 4). *Supreme Court is last leak-proof institution*. Chicagotribune.Com. https://www.chicagotribune.com/opinion/commentary/ct-supreme-court-leaks-20170704-story.html

Cen, L., Maydew, E. L., Zhang, L., & Zuo, L. (2017). Customer–supplier relationships and corporate tax avoidance. *Journal of Financial Economics*, *123*(2), 377–394. https://doi.org/10.1016/j.jfineco.2016.09.009

Chamseddine, J. (2018, January 23). *Court Likely to Kill Quill, Method Unclear*. Tax Notes. https://www.taxnotes.com/editors-pick/court-likely-kill-quill-method-unclear

Clement, M. B. (1999). Analyst forecast accuracy: Do ability, resources, and portfolio complexity matter? *Journal of Accounting and Economics*, *27*(3), 285–303. https://doi.org/10.1016/S0165-4101(99)00013-0

Cockfield, A. J. (2002). *Walmart.Com: A Case Study in Entity Isolation* (SSRN Scholarly Paper ID 325383). Social Science Research Network. https://papers.ssrn.com/abstract=325383

Cohn, M. (2021, October 21). *Businesses spend heavily on sales tax compliance*. Accounting Today. https://www.accountingtoday.com/news/businesses-spend-heavily-on-sales-tax-compliance

Constitution Annotated | Congress.gov | Library of Congress. (n.d.). Retrieved April 14, 2022, from https://constitution.congress.gov/

Cowan, A. R. (1992). Nonparametric event study tests. *Review of Quantitative Finance and Accounting*, *2*(4), 343–358. https://doi.org/10.1007/BF00939016

Cowan, M. J. (2007). Tax Planning Versus Business Strategy: The Rise and Fall of Entity Isolation in Sales and Use Taxes. *Idaho Law Review, Fall*.

Csan, S., Philipovitch, G., & Heller, R. (2018, June 26). *Supreme Court overturns Quill's physical presence standard*. Deloitte | Tax@hand.

https://www.taxathand.com/article/9887/United-States/2018/Supreme-Court-overturns-Quills-physical-presence-standard

Cullers, M. (2018, April 17). *Oyez! The Supreme Court Hears Oral Arguments in Wayfair, and Now We Play the Waiting Game*. The National Law Review.

https://www.natlawreview.com/article/oyez-supreme-court-hears-oral-arguments-wayfair-and-now-we-play-waiting-game

Dyreng, S., Jacob, M., Jiang, X., & Miller, M. (2020). Tax Avoidance and Tax Incidence. *SSRN Electronic Journal*. https://doi.org/10.2139/ssrn.3070239

Egerwels. (2016, December 28). *The Possible Upshot of South Dakota's Master Plan to "Kill Quill."* Baker McKenzie SALT Savvy. https://www.saltsavvy.com/2016/12/28/the-possible-upshot-of-south-dakotas-master-plan-to-kill-quill/

Erickson, M. M., Hanlon, M., Maydew, E. L., & Shevlin, T. J. (2020). *Taxes & Business Strategy* (6 edition). Prentice Hall.

Fox, W. F., Luna, L., & Schaur, G. (2014). Destination taxation and evasion: Evidence from US inter-state commodity flows. *Journal of Accounting and Economics*, *57*(1), 43–57.

Frankel, R., Kothari, S. P., & Weber, J. (2006). Determinants of the informativeness of analyst research. *Journal of Accounting and Economics*, *41*(1–2), 29–54. https://doi.org/10.1016/j.jacceco.2005.10.004

Fullerton, D., & Metcalf, G. E. (2002). Chapter 26 Tax incidence. In *Handbook of Public Economics* (Vol. 4, pp. 1787–1872). Elsevier. https://doi.org/10.1016/S1573-4420(02)80005-2

Gerstein, J. (2022, May 2). *How rare is a Supreme Court breach? Very rare*. POLITICO. https://www.politico.com/news/2022/05/02/supreme-court-draft-opinion-00029475

Giroud, X., & Rauh, J. (2019). State Taxation and the Reallocation of Business Activity: Evidence from Establishment-Level Data. *Journal of Political Economy*, *127*(3), 1262–1316. https://doi.org/10.1086/701357

Gordon, M. R. (2009). Up the Amazon without a Paddle: Examining Sales Taxes, Entity Isolation, and the Affiliate Tax. *North Carolina Journal of Law & Technology*, *11*, 299.

Gresko, J., & Associated Press. (n.d.). *The leak of the Roe v. Wade draft opinion was incredibly, shockingly rare*. Fortune. Retrieved July 30, 2022, from https://fortune.com/2022/05/03/supreme-court-decision-leaks-roe-v-wade-abortion/

Hainmueller, J. (2012). Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies. *Political Analysis*, *20*(1), 25–46. https://doi.org/10.1093/pan/mpr025

Hayes, G. (1921). The Incidence of a Sales Tax. *The Annals of the American Academy of Political and Social Science*, *95*, 207–212.

Hoopes, J. L., Thornock, J. R., & Williams, B. M. (2016). Does use tax evasion provide a competitive advantage to e-tailers? *National Tax Journal*, *69*(1), 133–168. https://doi.org/10.17310/ntj.2016.1.05

Hopland, A. O., Lisowsky, P., Mardan, M., & Schindler, D. (2018). Flexibility in Income Shifting under Losses. *Accounting Review*, *93*(3), 163–183. https://doi.org/10.2308/accr-51907

Horowitz, A. (2018, April 18). *Oral Argument in Wayfair Raises the Possibility That a Sharply Divided Court Will Preserve Quill: Miller & Chevalier's Tax Appellate Blog*. https://appellatetax.com/2018/04/18/oral-argument-in-wayfair-raises-the-possibility-that-a-sharply-divided-court-will-preserve-quill/

ITEP.org. (2021). *Amazon Sales Tax by State (1995-2020)*. ITEP.Org. https://itep.sfo2.digitaloceanspaces.com/AmazonSalesTaxbyState_1995-2020-2.gif

Jacob, M., Müller, M. A., & Wulff, T. (2022). *Do Consumers Pay the Corporate Tax?* (SSRN Scholarly Paper ID 3468142). Social Science Research Network. https://doi.org/10.2139/ssrn.3468142

Jennifer Jensen, Gorton, T., Bell-Jacobs, M., Mulligan, J., & Reichenberg Sherr, E. (2019, December 1). Practical implications of the Wayfair decision. *The Tax Adviser*. https://www.thetaxadviser.com/issues/2019/dec/implications-of-wayfair-decision.html

Jennings, R., Weaver, C., & Mayew, W. J. (2012). The Extent of Implicit Taxes at the Corporate Level and the Effect of TRA86*. *Contemporary Accounting Research*, *29*(4), 1021–1059. https://doi.org/10.1111/j.1911-3846.2012.01157.x

Kennedy, A. (2018). *17-494 South Dakota v. Wayfair, Inc. (06/21/2018)*. 40.

Lin, E., Mueller, M., McGuire, M., & Paschal, E. (2020). Supreme Court Justices Spar Over Precedent. *The National Law Review*, *X*(118). https://www.natlawreview.com/article/supreme-court-justices-spar-over-precedent

Liptak, A. (2015, March 3). Upholding Internet Sales Tax Law, a Justice Invites a New Case. *The New York Times*. https://www.nytimes.com/2015/03/04/business/supreme-court-backs-trade-groups-challenge-to-internet-sales-tax-law.html

Luchs-Nunez, J. (2022). Economic Effects of Sales Tax Collection Requirements: Evidence from Wayfair. *SSRN Electronic Journal*. https://doi.org/10.2139/ssrn.3924416

Madhok, A., Keyhani, M., & Bossink, B. (2015). Understanding alliance evolution and termination: Adjustment costs and the economics of resource value. *Strategic Organization*, *13*(2), 91–116. https://doi.org/10.1177/1476127015580309

Markle, K. S., Mills, L. F., & Williams, B. (2020). Implicit Corporate Taxes and Income Shifting. *Accounting Review*, *95*(3), 315–342. https://doi.org/10.2308/accr-52526

McLoughlin, J. (2020, June 22). *The Wayfair Tax Case Two Years Later: Compliance And Cost*. Forbes. https://www.forbes.com/sites/taxnotes/2020/06/22/the-wayfair-tax-case-two-years-later-compliance-and-cost/

Pisano, G. P. (2017). Toward a prescriptive theory of dynamic capabilities: Connecting strategic choice, learning, and competition. *Industrial and Corporate Change*, *26*(5), 747–762. https://doi.org/10.1093/icc/dtx026

Prete, R. (2018, April 13). 'Tax case of the millennium' hits high court. *Accounting Today*. https://www.accountingtoday.com/opinion/tax-case-of-the-millennium-hits-the-supreme-court

Rapaport, M. (2018, July 13). Wells Fargo's $481 Million Tax Surprise. *Wall Street Journal*. https://www.wsj.com/articles/wells-fargos-481-million-tax-surprise-1531499680

Reddick, C. G., & Coggburn, J. D. (2007). E-commerce and the Future of the State Sales Tax System: Critical Issues and Policy Recommendations. *International Journal of Public Administration*, *30*(10), 1021–1043. https://doi.org/10.1080/01900690701221373

Reichenberg-Sherr, E. (2018, April 18). *Supreme Court hears oral arguments in sales tax nexus case*. Journal of Accountancy. https://www.journalofaccountancy.com/news/2018/apr/supreme-court-sales-tax-nexus-case-201818800.html

Roberson, A., & Spencer, K. (2022). Supreme Court Grants Certiorari in One Tax Case, Denies it in Several Others. *The National Law Review*, *XI*(279). https://www.natlawreview.com/article/supreme-court-grants-certiorari-one-tax-case-denies-it-several-others

Rosenthal, C. L., Paton, K., & Smith, S. (2017, January 22). *SLT | Understanding Click-Through Nexus*. The CPA Journal. https://www.cpajournal.com/2017/01/22/slt-understanding-click-through-nexus/

Rubin, B. K. and R. (2018, January 12). Supreme Court to Consider Internet Sales Tax Collection. *Wall Street Journal*. https://www.wsj.com/articles/supreme-court-to-consider-internet-sales-tax-collection-1515788446

Sakhartov, A. V., & Folta, T. B. (2014). Resource relatedness, redeployability, and firm value. *Strategic Management Journal*, *35*(12), 1781–1797. https://doi.org/10.1002/smj.2182

Stone, B. (2013). *The Everything Store: Jeff Bezos and the Age of Amazon*. Little, Brown.

Suárez Serrato, J. C., & Zidar, O. (2016). Who Benefits from State Corporate Tax Cuts? A Local Labor Markets Approach with Heterogeneous Firms. *American Economic Review*, *106*(9), 2582–2624. https://doi.org/10.1257/aer.20141702

Swain, J. A. (2001). Cybertaxation and the Commerce Clause: Entity Isolation or Affiliate Nexus. *Southern California Law Review*, *75*(2), 419–474.

U.S. Department Labor. (2021). *SIC Manual | Occupational Safety and Health Administration*. https://www.osha.gov/data/sic-manual

Walczak, J., & Drenkard, S. (2018, February 13). State and Local Sales Tax Rates, 2018. *Tax Foundation*. https://taxfoundation.org/state-and-local-sales-tax-rates-2018/

Weisbach, D. A. (1999). Implications of Implicit Taxes. *SMU Law Review*, *52*, 11.

Wooldridge. (2019). *Introductory Econometrics* (7th ed.).

www.supremecourt.gov. (n.d.). *Frequently Asked Questions: General Information—Supreme Court of the United States*. Retrieved August 8, 2022, from https://www.supremecourt.gov/about/faq_general.aspx

Yetter, D. (2018, May 18). *South Dakota v. Wayfair Analysis & Predictions: Sales Tax Experts Weigh-In*. Sales Tax Institute. https://www.salestaxinstitute.com/resources/south-dakota-v-wayfair-analysis-predictions-sales-tax-experts-weigh-in

Zhao, Q., & Percival, D. (2017). Entropy Balancing is Doubly Robust. *Journal of Causal Inference*, *5*(1). https://doi.org/10.1515/jci-2016-0010