Journal of Marketing Research
Article Postprint, Volume XLVI

How Sales Taxes Affect Customer and Firm Behavior: The Role of Search on the Internet

January 2009

Eric T. Anderson
Kellogg Graduate School of Management

Nathan M. Fong
MIT Sloan School of Management

Duncan I. Simester
MIT Sloan School of Management

Catherine E. Tucker
MIT Sloan School of Management

We thank the anonymous retailer that generously provided the data used in this study. This paper has benefited from comments by seminar participants at Duke, Kellogg, MIT and the 2006 Marketing Science conference.

Journal of Marketing Research
Article Postprint, Volume XLVI

How Sales Taxes Affect Customer and Firm Behavior: The Role of Search on the Internet

When a multi-channel retailer opens its first retail store in a state, the firm is obligated to collect sales taxes on all Internet and catalog orders shipped to that state. In this paper, we assess how opening a store affects Internet and catalog demand. We analyze purchase behavior among customers who live far from the retail store but who are obligated to pay sales taxes on catalog and Internet purchases. A comparable group of customers in a neighboring state serves as a control. We show that Internet sales decrease significantly, but catalog sales are unaffected. Further investigation indicates that the difference in these outcomes is partly attributable to the ease with which customers can search for lower prices at competing retailers. We extend the analysis to a panel of multi-channel firms and show that retailers who earn a large proportion of their revenue from direct channels avoid opening a first store in high-tax states. We conclude that current U.S. sales taxes laws have significant effects on both customer and firm behavior.

Keywords: sales tax, customer search, internet, retail, channels

> *"It is illogical and now patently unfair that Staples.com is required to collect and remit sales taxes because of our local commitment when many other dot.com competitors do not have a similar requirement."*
>
> Thomas Stemberg, founder of Staples
> (letter to the US Congress, Stemberg 2005)

State sales taxes can add up to nine percent to the price of an item. However, if a retailer does not have a physical presence in a state, such as a warehouse, office or retail store, it is not required to collect these taxes. This distinction is crucial for retailers that sell through direct channels. The obligation to collect taxes from Internet and catalog sales depends on whether the retailer has a physical presence in the customer's state. In this paper, we investigate how the obligation to collect sales taxes affects both customer and firm behavior. We consider two related research questions. First, does collecting sales taxes have a negative impact on sales through Internet and catalog channels? If there is a negative impact on sales, then we might expect it to also affect firm behavior. This motivates our second research question: are direct retailers less likely to establish a physical presence in high-tax states?

We show that the answer to the first question depends on which direct channel we consider. The obligation to collect and remit sales taxes produces a significant decrease in Internet sales but no change in catalog sales. We conclude that the obligation to collect sales taxes has a significant effect on customer behavior, but that there are boundaries to these effects. These boundaries reflect limits on a customer's ability or incentive to search for lower prices at competing retailers. If search is difficult, because it requires access to competitors' catalogs, or if there is little incentive to search because prices are unlikely to be lower elsewhere, then the tax effects are mitigated.

The evidence that taxes lower Internet sales but not catalog sales presents retailers with a

trade-off. Should they forgo the benefits of opening a store in order to avoid damaging Internet demand? We investigate how retailers resolve this trade-off by evaluating whether tax rates affect store-opening decisions. We find that retailers who conduct most of their business through direct channels avoid opening a first store in high-tax states. We conclude that these retailers appear to be forward-looking, anticipating the growth of the Internet channel and avoiding the potential risk to this future revenue stream.

Source of the Findings

Our investigation of customer behavior takes advantage of a natural experiment. We examine changes in customer behavior after the opening of a new retail store by a multi-channel retailer. Importantly, this is the retailer's first store in the state, which introduces a legal obligation to collect sales taxes on all Internet and catalog orders shipped to that state. We use a differences-in-differences approach to measure the effect on Internet and catalog sales. In particular, we compare the difference in sales before and after the store opening between two samples of customers. One sample of customers lives in the state in which the new store was opened. These customers were not charged sales taxes on their Internet and catalog orders before the store opened, but were charged sales taxes after the store opened. The second sample of customers lives in a neighboring state. These customers were not charged sales taxes either before or after the store opening. By comparing the difference in sales over time between two groups of customers (the "differences-in-differences"), we benefit from two inherent controls. Measuring the change in purchases by the same customers over time allows us to control for

individual customer characteristics, while comparing customers on either side of the border controls for time trends and other events that are common to both customer samples.

Our analysis recognizes several potential confounds in the natural experiment. First, opening a convenient store location may cause customers to shop more often or to substitute between channels. To isolate the tax effect from other effects, we focus on customers who live more than 100 miles from the new retail store. Since this sample of customers cannot easily access the new store, we expect that the only impact of opening the new store is the introduction of sales taxes.

The second key assumption in our customer analysis is that store opening and location decisions are exogenous. Clearly, store locations are not chosen randomly. Discussions with the firm's managers (and analysis of historical transactions) confirmed that stores are located in areas with a high density of existing customers who often buy through the firm's catalog and Internet channels. In this sense, the store-location decision is endogenous as it is based on characteristics of customers in the retail trade area. However, the customers in our sample are not near the retail trade area. It seems reasonable to assume that the store-location decision is not correlated with either observed or unobserved customer characteristics in our sample.

Finally, our interpretation of the natural experiment requires that any factors that could affect the change in sales in the focal state, also affect sales in the neighboring state. We use several robustness checks to investigate this issue. First, we present data on historical purchases that reveal no systematic differences in the characteristics of customers living on each side of the border. Second, we compare the mailing of catalogs and marketing materials to customers on both sides of the border to ensure they are similar. Third, we inspect historical sales trends in the

12 months prior to the new store opening. This inspection reveals no apparent difference in sales trends between the two states. Finally, we repeat our differences-in-differences analysis across different time periods and confirm that the change in orders in the focal state after the store opening do not arise in other periods. Together, these robustness checks provide further support for our conclusions.

Our analysis of firm behavior also addresses a potential confound. We find that, all else being equal, a retailer is *more* likely to open a new store in a high-tax state. This does not mean that high sales taxes attract retail stores. High-tax states presumably have customer characteristics that attract retailers. For example, we might attribute the tendency to open stores in New York despite its high tax rates to the relative wealth of the average New York resident. To overcome this confound and isolate the impact of sales taxes, we measure store opening decisions for a panel of retailers. We compare how these decisions vary both by state tax rate and by the amount of business that a retailer conducts through remote channels. This comparison provides a control for store-opening trends that are common to all retailers, including the general tendency to open stores in high-tax states.

Court Rulings and Related Literature

The Supreme Court ruling in *Complete Auto Transit, Inc. v. Brady*[1] required a taxpayer to have a physical presence, or "nexus", within a state before a state had jurisdiction to require the collection of sales taxes. A later ruling in *Quill Corporation v. North Dakota*[2] clarified that Quill, a Delaware corporation, was not obligated to collect North Dakota sales taxes from customers because Quill did not have a measurable physical presence in North Dakota. Although

sales taxes do apply, the burden of collection resides with consumers rather than with merchants. The individual amounts involved are small and enforcement costs are high, so state governments have not aggressively pursued these tax revenues.

This is not the first paper to recognize that state sales taxes may influence customer sales. Goolsbee (2000) uses survey data to investigate whether customers with Internet access are more likely to purchase a computer online if they live in high-tax areas. Our paper differs from Goolsbee's because the two papers measure different things. Goolsbee describes how the size of the Internet sector grows as state tax rates increase. By contrast, we measure what happens to Internet and catalog sales when a retailer is required to collect and remit sales taxes. This is also not the first paper to investigate how tax rates affect store location decisions. Manuszak and Moul (2008) demonstrate that gasoline tax differences affect both the location of gasoline stations and customers' decisions about where to purchase their gasoline. Given the nature of this product category, they focus solely on physical stores, and do not consider how the physical presence test affects remote retailers.

There are at least two unpublished manuscripts that investigate how variation in local tax rates affects the size of the Internet sector. Ellison and Ellison (2003) use hourly data collected from pricewatch.com. They find that increases in state sales taxes lead to substitution, with more customers purchasing over the Internet. Brynjolfsson, Smith, and Montgomery (2004) study consumer "shopbot" behavior and report that consumers are more sensitive to a dollar of tax than to a dollar change in the unit price of an item.

A key challenge in this literature is establishing causality between tax rates and the consumer response. Studies that infer a causal relationship from variation in tax rates between

states suffer from the limitation that state tax rates may not be independent of other state characteristics. Indeed, we show that variation in tax rates affects whether a retailer establishes a physical presence in a state. Hence, the variation in whether customers purchase over the Internet may reflect variation in the availability of alternative retailers. More generally, any unobserved demand characteristics that are correlated with state tax rates may contribute to variation in sales. As a result, we risk misattributing the impact of these unobserved characteristics to differences in tax rates. We overcome this potential limitation by using a natural experiment comparing changes in behavior for the same households over time. This approach explicitly controls for unobserved state and customer characteristics.

Structure of the Paper

Section 2 measures the change in Internet and catalog sales when a direct retailer starts collecting sales tax. The focus shifts to firm behavior in Section 3. We investigate whether retailers with more direct business are less likely to establish a physical presence in a high-tax state. Section 4 concludes the paper with a review of the findings.

Measuring Sales Responses Across Different Channels

In this section we measure how customers of a multi-channel apparel retailer respond when the retailer starts collecting sales taxes on Internet and catalog orders. The company sells men's and women's clothing through company-owned distribution channels, which include catalogs, an Internet site and retail stores. The products are moderately priced ($48 on average) and customers purchase relatively frequently (1.2 orders containing on average 2.4 items per year).

Our analysis focuses on a sample of 13,021 customers who live on either side of a border of the "focal" state in which the firm opened its new store. This new store was a regularly priced store, not a factory store. Customers in the focal state did not pay sales taxes on catalog or Internet orders prior to the store opening, but were charged sales taxes from the date the store opened. For confidentiality reasons, we cannot reveal where the store is located. However, the focal state is one of 32 states that charges between 4% and 6% sales taxes on retail transactions. The neighboring state does not charge sales tax, so customers in this state paid no sales taxes on purchases from this company either before or after the new store was opened in the focal state.

The 13,021 customers had all bought from the company's Internet or catalog channels in the 39 months before the store opened in the focal state. They were identified by matching household zip codes to counties along either side of the state border with a neighboring state. The counties were small, each with a population of less than 150,000. We also considered two other criteria for selecting customers: customers who live in three-digit zip codes along the state border; and customers in 5-digit zip codes. The three-digit zip codes are larger than the area covered by the counties and yielded 32,996 customers. The five-digit zip codes along the state border yielded just 692 customers. Reassuringly, the key findings remain unchanged for all three selection criteria.

We focus on this state border because all of the customers living on the border were more than 100 miles from the new store and were not expected to be otherwise affected by the opening of the new store. As expected, only 13 of the 13,021 customers made purchases at the new store. We caution that since customers living in this border region may differ from customers living

elsewhere, it is possible that the magnitude of the tax effects will be different for customers who live elsewhere in the state.

We observe customers' historical purchases over a 51-month period, starting 39 months before the store opened and continuing until 12 months after. It is helpful to divide this 51-month period into three different time periods:

Historical Period:	13-39 months before the store opened
Pre-Opening Period:	0-12 months before the store opened
Post-Opening Period	0-12 months after the store opened

We compare sales in the pre- and post-opening periods for customers in the focal and neighboring states. As we discussed, this differences-in-differences approach explicitly controls for customer differences and common changes in demand over time. We also checked the robustness of our results by varying the lengths of the pre- and post-opening measurement periods. Our findings are essentially unchanged under these alternative measures.

We use data from the historical period (13-39 months before the store opened) to ensure that customers in the focal and neighboring states share similar characteristics. We characterize customers using the *Recency*, *Frequency* and *Monetary Value* (RFM) of their historical purchases. These RFM measures were developed by the direct marketing industry to segment customers and so serve as a natural basis for evaluating customer differences. *Recency* measures the number of days between a customer's last order and the date of the store opening. We code this as a negative variable so that higher values represent more recent customers. *Frequency* describes the number of orders placed and *Monetary Value* describes the average dollar value of those orders. In Table 1 we report the average of these variables for the focal state and

neighboring state during the historical period (13-39 months prior to the new store opening). Reassuringly, despite large sample sizes, the averages of these measures do not vary significantly between customers in the two states. We conclude that customers in the two states appear to share similar observable characteristics.

Estimating How Sales Taxes Affected Sales in the Focal State

Our analysis focuses on the change in the number of orders placed by customers in the neighboring and focal state before and after the store opening.[3] We begin by reporting univariate results and then turn to a multivariate approach. The univariate results are reported in Table 2, where we summarize the average number of orders placed by customers in the pre and post-opening periods. The averages are calculated separately for the different channels, and for customers in the different states. Sales for the Internet channel represents orders placed over the firm's website, while sales for the catalog represents orders received via mail or telephone.

There are several findings of interest in Table 2. First, we see that approximately 17% of the purchases occurred over the Internet channel. This percentage was increasing at the time, and has continued to increase since these measurement periods. Second, in both states we see a drop in catalog sales; this in part reflects substitution to the Internet channel. It also results from our focus on a fixed panel of customers. It is well-established in this industry that as a customer panel ages, customers tend to place fewer orders. This attrition is generally attributed to changes in customers' preferences, but also reflects address changes. Third, Internet purchases in the neighboring state decrease only slightly between the pre- and post-opening periods, which is consistent with substitution from the catalog to the Internet channel. However, in the focal state

there is a large and significant drop in Internet sales between the two measurement periods. This drop occurs despite the trend of customers to switch from the catalog channel to the Internet channel. Notice that the comparison of interest is not merely the pre versus post difference, but whether this difference varies for customers in the focal and neighboring states. We directly estimate this interaction in the multivariate analysis that follows.

Regression Analysis

Because the number of orders placed is a "count" measure we use Poisson regression. In particular, we assume that the number of orders from customer *i* in period *t* (Q_{it}) is drawn from a Poisson distribution where the mean of the distribution is represented by the parameter λ_{it}:

$$\text{Prob}(Q_{it} = q) = \frac{e^{-\lambda_{it}}\lambda_{it}^{q}}{q!}, \quad \text{where } q=0, 1, 2, \ldots, \text{ and } \ln(\lambda_{it}) = \boldsymbol{\beta}\mathbf{X}_{it}. \tag{1}$$

We model the purchase rate as $\ln \lambda_{it} = \boldsymbol{\beta}\boldsymbol{X}_{it}$, where the *i* subscript refers to the 13,021 customers and the *t* subscript refers to the pre- and post-periods (t = 1 or 2). Our initial analysis uses the following specification for the independent variables:

$$\boldsymbol{\beta}\boldsymbol{X}_{it} = \beta_0 + \beta_1 \textit{Post Opening}_{it} + \beta_2 \textit{Focal State}_{it} + \beta_3 \textit{Post Opening}_{it} * \textit{Focal State}_{it} \tag{2}$$

The *Post-Opening* and *Focal State* variables are binary indicators identifying the post-opening period and customers in the focal state (respectively). Under this specification, *Focal State* controls for underlying differences between customers in each state and *Post-Opening* controls for common sales changes over time. The coefficient of interest is the interaction coefficient (β_3), which measures the percentage change in post-opening sales among customers in the focal state compared to customers in the neighboring state. We will interpret β_3 as the

percentage change in orders placed due to the introduction of sales taxes in the focal state. In all specifications, we also include a household level random effect. In Table 3, we report our initial findings, where we distinguish between purchases over the Internet and purchases through the catalog channel (mail or telephone orders).

The results reveal the same pattern as the univariate results, and appear to confirm that the obligation to charge sales taxes has a different impact on sales in the two channels. The store opening in the focal state is associated with an 11.6% decrease in Internet sales (after controlling for the change in sales in the neighboring state). This effect is statistically significant ($p<0.05$). By contrast, sales in the catalog channel are apparently unaffected by the introduction of sales taxes. When we aggregate sales across both channels, the overall impact is negative but not significantly different from zero.

The *Post-Opening* and *Focal State* variables separately control for systematic differences over time and between states (respectively). As expected, the negative coefficient for the *Post-Opening* variable captures the expected natural attrition of customers. The coefficient for *Focal State* is positive and significant in two of three models. This is consistent with the results in Table 2, which indicate that customers in the focal state place more orders through the catalog and Internet in both the pre-period and post-period than customers in the neighboring state. The presence of retail stores in the neighboring state may partially explain this difference. Sales through the retail channel may cannibalize from Internet and catalog purchases in the neighboring state. These stores were opened over a decade earlier, so their availability is constant during the periods we consider. A potential concern is that the rate of substitution to and from the store channel may have changed over time. When we re-estimate our model with only store

purchases, the pre versus post comparisons do not differ between the two states (the interaction between *Focal State* and *Post Opening* is small and insignificant). We conclude that the new store opening in the focal state did not lead to differences between the two states in the rate of store purchases.

As a robustness check, we also replicated the analysis in Table 3 using a linear model:

$$Q_{it} = \beta_0 + \beta_1 Post\ Opening_{it} + \beta_2 Focal\ State_{it} + \beta_3 Post\ Opening_{it} * Focal\ State_{it} + \varepsilon_{it} \quad (3)$$

This linear model allows us to check whether the findings are specific to the Poisson specification. The findings for the linear model are reported in Table 4. Although the magnitudes of the coefficients are not directly comparable between the two models, the pattern of results is consistent. The introduction of sales taxes is associated with a significant reduction in Internet orders, but no change in catalog orders.

Reasons Sales May Have Changed

Recall that the critical identifying assumption is that any extraneous factors (other than the introduction of sales taxes) that affected the change in demand in the focal state also affected demand in the neighboring state. Therefore, when checking robustness we need to focus on factors that may have contributed to a change in the number of pre- and post-opening purchases. One possibility is that direct-mail promotional activities may have varied in the focal state following the store opening. If so, we might be misattributing our results to the collection of sales taxes rather than these activities. To check for this, we investigated whether there was a change in the number of catalogs or other promotional materials sent to customers in each state before and after the store opening. Reassuringly, we found no systematic differences. As a

further check, we re-estimated Equation 2 with a control variable for retail promotions, and our findings remained unchanged.

We also investigated whether in the months before the store opening there was a similar time trend in monthly sales. In Figure 1 we report the average monthly purchases by households over the course of the calendar year before the new store opened for each of the two states. Inspection of this figure suggests that there was no systematic difference in the historical sales trends for the two states.

To evaluate more formally whether there was any difference in these trends, we estimated the following household-level Poisson regression model:

$$\beta X_{it} = \sum_{t=1}^{12} \beta_t Month\, t_{it} + \sum_{t=1}^{12} \eta_t Month\, t_{it} * Focal\ State_{it} \quad (4)$$

The dependent variable was the number of items purchased by each household in each of the 12 months preceding the store opening, and *Month t* denotes dummy variables identifying each of these months (these incorporate the intercept). The coefficients of interest are $\eta_1, \ldots, \eta_{12}$, which measure whether the monthly time trends varied between the two states. Reassuringly, none of these coefficients were significantly different from zero.

As a final check for robustness, we repeated our analysis for other historical time periods to assess whether the difference in sales trends between the two customer samples was unique to the store opening period. If there are no differences in the historical trends for the two states, we should not find an interaction between the *Post-Period* and *Focal State* variables. To investigate this issue, we re-estimated Equation 2 using the same pre and post-period calendar dates one year

before and one year after the store opened. The results are reported in Table 5.

The *Post-Period * Focal State* coefficients confirm that there was no significant difference between the two states in the rate at which sales changed over time. This suggests that the difference in the sales in these two states after the store opening was unusual, and gives us greater confidence that we can attribute this difference to the store-opening.

Differences Across Product Categories

We also consider how the tax effect varies by the *type of item* purchased. The firm sells women's clothing, men's clothing and "other" items (other items include home furnishings, luggage and clothing for children). There are small differences in the sales for each category across the two channels. For example, women's clothing represents 29.0% of catalog sales and 33.5% of Internet sales, while men's clothing represents 28.2% of catalog sales and 25.8% of Internet sales. It is possible that these preferences for purchasing different items in the two channels may have contributed to the contrasting findings. To investigate this possibility we re-estimated the models in Table 3 using separate measures of sales for each product category. These findings (which are available from the authors) again replicate the pattern of findings in Table 3. There is a decrease in Internet sales across all three product categories, and this decrease is significant ($p<0.05$) in all but the men's category. In contrast, there is effectively no change in catalog sales in either channel (the *Post-Opening * Focal State* coefficients do not approach significance). These findings further confirm the robustness of the results in Table 3. They also suggest that the contrast in the Internet and catalog findings cannot be explained by differences in which product categories are purchased in the two channels.

We conclude that there is robust evidence that collecting sales taxes had a substantial negative effect on Internet sales. Surprisingly, there was no measurable decrease in catalog sales. We now examine why.

Explanations for the Difference in Catalog and Internet Findings

We consider three explanations for why we observe a decrease in Internet sales, yet no appreciable change in catalog sales:

1. **Awareness:** Chetty, Looney and Koft (2007) report findings from a series of field experiments showing that consumers may react differently if taxes are made salient at the point of purchase. This effect could have contributed to our results as the salience of the tax calculation varies when ordering by telephone and the Internet. When completing an order over the telephone, customers in our study are often told only the total amount of the order. Because most customers order multiple items (the average order size is 2.3 items) and the total amount may include other components such as shipping charges, it is possible that these customers do not realize that the total amount includes sales tax. In contrast, Internet customers can see taxes explicitly calculated on the checkout page.
2. **Personal Interaction:** Telephone orders involve a personal interaction with a company representative, but ordering over the Internet is relatively impersonal. As a result, when customers learn at the end of the transaction that they are being charged sales tax, they may feel more comfortable cancelling an Internet order than abandoning a telephone order.
3. **Customer Search:** When a firm collects sales tax, one possible customer response is to search for similar items at competing retailers that do not charge sales tax. Customers who use the Internet channel may be more willing to search competing retailers than those who

use the catalog channel. This may reflect the ease of searching alternative retailers on the Internet. Internet customers can simply visit the website of one of the firm's many competitors. In contrast, customers without computers or who are reluctant to purchase over the Internet must either visit a competitor's store or wait until they receive competitors' catalogs. These options generally require more effort and more time than visiting a competing Internet site.

It is likely that each of these explanations contributes to the findings, so we do not seek to isolate a single explanation. Instead, our results elaborate on the earlier findings, and help to reveal the relative contribution of these three explanations.

We proceed in two steps. First, we distinguish between two types of catalog orders: orders by telephone and orders by mail. Telephone orders are made by calling the firm's toll-free telephone number and speaking to a customer service representative. Mail orders are made using the order form bound into the middle of each catalog. When ordering by mail, a customer lists the desired items and their prices, calculates a total, and then computes the sales taxes (if any). The customer can then pay via a personal check, money order or credit card. Under the *Awareness* and *Personal Interaction* explanations, orders by mail share characteristics similar to Internet orders: they include an explicit calculation of sales taxes and do not involve a personal interaction with a customer service representative. On the other hand, customers ordering by mail can turn to the same range of competing retailers as customers ordering by telephone. Therefore, if the *Awareness* and *Personal Interaction* explanations play an important role in determining

customers' reactions, we would expect to see an outcome for mail orders similar to the one that we observe for Internet orders.

Second, we investigate the role of customer search by identifying a class of items for which we do not expect customers to search competing retailers in either channel. If the search explanation is important, we would expect to see the same (null) effect for these items in the Internet channel as we see in the catalog channel.

Comparing Mail and Telephone Orders for the Catalog Channel

In our data, 16.1% of the firm's direct orders are placed via the Internet, 80.7% are placed via telephone, and only 3.2% are placed via mail. In our earlier analyses we subsumed both mail and telephone orders into the catalog channel. In Table 6, we distinguish between the mail and telephone channels and repeat our earlier analysis (we also include the Internet results from Table 3 as a benchmark).

Recall from our previous discussion that if the *Awareness* and *Personal Interactions* explanations were important, we would expect the result for mail orders to mimic the outcome for Internet orders. This is not the case. The results for the mail channel actually mimic the telephone channel: the obligation to collect sales taxes did not significantly affect either mail or telephone orders. These findings suggest that the contrasting findings in Table 3 cannot be fully explained by the *Awareness* and *Personal Interaction* interpretations.

Customer Search Behavior

When a firm begins to collect sales taxes, a natural reaction by customers is to look for similar

products at competing retailers that do not charge sales taxes. It is this anticipated customer reaction that prompted the founder of Staples to address his concerns to the US Congress (see the quote at the start of the paper). If the relative ease of searching on the Internet means that competitive comparisons are more likely on the Internet channel than the catalog channel, then differences in customer search behavior may explain why sales taxes reduce Internet sales.

To investigate this explanation, we identified a sample of items for which customers are unlikely to search competing retailers in either channel. If the contrasting findings in Table 3 are due to differences in customer search behavior, we would not expect any difference in the findings across the two channels when restricting attention to these items. Our first approach was to look for items that are only sold by this retailer. Unfortunately this approach was not successful. We identified a sample of items that we believed were most likely to be unique to this firm, but an Internet search quickly identified relatively close substitutes at competing stores for each of the products in this sample.

As an alternative, we recognized that if the firm discounts the price of an item, customers are less likely to find lower prices at competing stores. Indeed, there is evidence elsewhere that one of the roles of retail "sales" is to signal to customers that they need not continue searching for lower prices at competing stores (see for example Anderson and Simester 1998, 2001). This suggests that if an item is discounted, customers may be less likely to compare prices at competing retailers – even when the firm begins to collect sales tax. This offers an opportunity to test whether differences in customer search behavior across the channels explains the different findings. If we restrict attention to items that are discounted, and this limits the extent of customer search, we should observe the same (null) results in both channels.

The retailer typically charges the same price in its Internet channel as it charges in the catalog channel. As a result, there is almost no difference in the distribution of items purchased at a discount through the two channels. In the 39 months before the store opened, 11.5% of the items purchased on the Internet channel and 11.7% of the items purchased in the catalog channel were sold at a discounted price. The average price reduction was also almost identical across the two channels; the mean was $7.42 for the catalog channel and $7.22 for the Internet. We also compared the selection of items purchased at discounted prices through the two channels. The largest difference was in women's clothing, where 15.9% of the catalog purchases were made at a discount, compared to 17.5% over the Internet.

In Table 7, we repeat our earlier analysis when distinguishing between purchases made at a discount and purchases made at full price (since a single order may combine regular price and sale items, the purchase amount is measured as number of items rather than number of orders). The findings follow precisely the predicted pattern. For discounted purchases, there is no difference between the Internet and catalog channels: the change in sales tax policy does not affect sales in either condition. This is consistent with customers recognizing that they are unlikely to find lower prices for these items at competing stores, even after the firm starts charging sales taxes. For items purchased at full price, we see the now familiar pattern that Internet sales decrease in the focal state when the retailer collects sales tax, but there is no change in catalog sales.

These findings support the interpretation that the contrasting impact of sales taxes on sales in the two channels is due to differences in customer search behavior. This interpretation relies on our claim that when items are discounted, customers are unlikely to search for lower

prices at competing retailers. Notice that the strength of this claim depends in part on the depth of the discounts: in general, the larger the discounts, the less likely it is that customers will find lower prices elsewhere. The findings again follow the pattern that we would predict. If we restrict attention to purchases at "deep" discounts of more than 25%, the *Post-Opening * Focal State* coefficients are not significantly different from zero (or from each other) in either channel. However, for "shallow" discounts of less than 25%, the results return to the earlier pattern: the introduction of sales taxes is associated with a significant (8.8%) decrease in Internet sales, but no change in catalog sales.

We also investigated whether the finding reported in Table 7 could be explained by differences in customer preferences for purchasing discounted rather than regularly priced items. We used transaction data from the historical period to calculate the percentage of items that customers had historically purchased at discounted prices, and then interacted this measure with *Post-Opening * Focal State*. Our estimate for this additional interaction was both small and insignificant. We conclude that the findings survive when we explicitly control for differences in customers' propensity to purchase at a discount.

Summary

We have measured how establishing a physical presence in a state affects purchases through Internet and catalog channels. The results suggest that the obligation to collect and remit sales taxes leads to a significant decrease in Internet sales, but no appreciable change in catalog sales. This is a robust result that extends to different customers and different products.

We evaluate three explanations for why we do not observe a reaction to sales taxes in the catalog channel. Evidence that *mail* orders mimic *telephone* orders (rather than Internet orders)

suggests that the contrasting results cannot be explained by either the role of personal interactions with customer service representatives, or by a lack of awareness among catalog customers that the firm is collecting sales taxes. Neither of these explanations can be applied to orders placed via mail. Customers ordering by mail explicitly calculate sales taxes themselves, and generally do not have a personal interaction with a customer service representative.

The third explanation focuses on the role of customer search. We show that the impact of sales taxes is moderated by whether an item is discounted. The negative sales tax effect is isolated to non-discounted items purchased over the Internet. For these items, customers have the greatest incentive to search (high prices) and the greatest ease of search (the Internet). When a price is already deeply discounted, customers may be unlikely to consider alternative retailers even if the firm charges sales tax. Moreover, customers who order through the catalog channel face a much more difficult search task: they must wait to receive competitors' catalogs in order to compare prices. We conclude that the contrasting impact of taxes on Internet and catalog sales appears to be at least partly attributable to differences in customer search behavior.

In the next section, our focus shifts to firm behavior and we consider whether having to collect sales taxes affects retail store opening decisions.

The Impact on Firms' Store Location Decisions

Our analysis of customer behavior in Section 2 shows that an obligation to collect sales taxes reduces purchases through the Internet channel. This introduces an interesting empirical question. The evidence that sales taxes lower demand suggests that retailers will take this into account when deciding where to open physical stores, by avoiding states with high tax rates. On

the other hand, the reduction in demand is limited to the Internet, which was small at the time of the study, though growing quickly. This presents the retailers with a trade-off: should they forgo the benefits of opening a store in order to avoid damaging demand in a new channel that, while currently small, promises to be an important source of future revenue? In this section we investigate how retailers responded to this trade-off by evaluating whether sales taxes affected their store-opening decisions.

To investigate how state tax rates affect store opening decisions, we constructed a dataset from several sources. First, we identified a sample of apparel, jewelry, sporting goods and home furnishing retailers that sell through both retail stores and direct (Internet and catalog) channels. The sample was identified from the *2005 Multichannel Merchant 150*, which includes a description of each retailer's business, together with the percentage of sales conducted through retail stores and direct channels. We restricted the sample to retailers with fewer than 200 stores, and excluded any retailers that charged sales taxes on Internet and catalog purchases in states in which they did not have a physical presence.[4] Second, we used the *Retail Tenants Directory* to identify states in which retailers had physical retail locations. This annual publication is used by landlords, retailers and realtors to support the retail leasing process. It contains detailed profiles of retailers across the United States - listing individual retail locations. The intersection of retailers listed in both the *Multichannel Merchant 150* and the *Retail Tenants Directory* resulted in a panel of 14 retailers.

It is important to note that the *Multichannel Merchant 150* distinguishes only sales through direct channels and physical stores; it does not distinguish between the Internet and catalog channels. As a result, we cannot compare the store-opening decisions of retailers that

conduct most of their business over the Internet with the decisions of retailers who use the catalog channel. However, we can compare the decisions of retailers that use direct channels with those that sell primarily through physical stores. The potential loss of Internet sales affects only the first group, so we look for evidence that these retailers avoid opening stores in high tax states. For retailers with little direct business, the potential loss of Internet sales is less important. In order to compete in a state, these retailers generally have to open a physical store.

The analysis that follows is limited to the 48 contiguous states. Including observations for Hawaii, Alaska and Washington DC had little impact on the findings. We also omit 19 observations to remove states with either company headquarters or fulfillment centers. These qualify as a physical presence, so there is no incremental tax effect from opening a store in these states.

Analysis

As a preliminary analysis, we grouped the 14 retailers according to the percentage of their sales that are direct, and report the average tax rate in states with and without a physical store. The analysis reveals two findings of interest:

1. Retailers with a high percentage of direct business have a strong tendency to open stores in states with lower tax rates. For retailers that earn more than 70% of their revenue from direct channels, the average tax rate in states with stores is 2.65%. This is significantly ($p < 0.01$) lower than the 4.90% average rate in states without stores.
2. Retailers that have less direct business actually prefer high-tax states, presumably because these states have other characteristics that make them attractive. Among these retailers, the

average tax rate in states with stores is 5.06%, which is significantly ($p < 0.01$) higher than the 4.51% average rate in states without stores. The difference between 5.06% and 2.65% is also significant (comparing states with stores for the two groups of retailers).

These univariate findings are consistent with our prediction that store location decisions are influenced by an interaction between state tax rates and the amount of business that the retailer conducts through direct channels. We can directly estimate this interaction using a multivariate approach, which also allows us to explicitly control for state and retailer differences. We will focus on a retailer's decision to open a *first* store in a state. After this first store, there is no incremental change in the tax policy if a retailer opens additional stores. We will later also consider the decision to open subsequent stores in a state, to confirm that the deterrence of high tax rates (if any) is limited to the first store.

We begin by identifying states in which the retailers did not have stores in 2001. We then use data from 2004 to identify whether retailers had opened a first store in those states by 2004. In particular, we used the *Retail Tenants Directory* to identify states in which the panel of retailers did not have a retail store, headquarters, or fulfillment center in 2001. On average the retailers had approximately 35 states without stores, so across all 14 retailers this resulted in a sample of 450 state-retailer observations in which there was an opportunity to open a first store. For each of these 450 observations, we then used the *Retail Tenants Directory* to determine whether a store had been opened by that retailer in that state by 2004. The dependent measure in the analysis is $Store\ in\ 2004_{r,s}$, which is a binary variable indicating whether retailer r had a store in state s in 2004. The average of this dependent variable is 12% (there were 54 instances in

which retailers had entered a state and opened a new store by 2004).

The goal of the analysis is to explain the variation in this dependent measure, and in particular whether retailers with a high percentage of direct sales were less likely to open stores in high tax states. The analysis used logistic regression, and included the following independent variables:

Interaction of Interest	
Tax Rate$_s$ * Percent Direct$_r$	The interaction between *Tax Rate$_s$* and *Percent Direct$_r$*.
Controls for State Characteristics	
Tax Rate$_s$	The 2001 tax rate in state *s*.
Stores in State$_s$	The total number (in hundreds) of retail stores in state *s* in 2001 for the sampled product categories (excluding retailers in the sample).
State Store Growth$_s$	The percentage change in *Stores in State$_s$* between 2001 and 2004.
Controls for Retailer Characteristics	
Percent Direct$_r$	The percentage of retailer *r*'s 2001 sales that comes from direct (Internet and catalog) channels.
Apparel$_r$	A binary variable indicating whether retailer *r* sells apparel.
Controls for State-Retailer Interactions	
Bordering States$_{r,s}$	A binary variable indicating whether in 2001 retailer *r* had stores in any state bordering state *s*.
Other Close States$_{r,s}$	A binary variable indicating whether in 2001 retailer *r* had stores in any state two borders from state *s*.

Summary statistics for all the variables are reported in Table 8.

Our focus is the interaction between *Tax Rate$_s$* and *Percent Direct$_r$*. We predicted that retailers

will avoid opening stores in high-tax states if they conduct a large proportion of their business through direct channels. Therefore we expect the interaction between *Tax Rate*$_s$ and *Percent Direct*$_r$ to yield a negative coefficient: retailers with a high percentage of direct sales are less likely to open stores in high tax states. The findings are summarized in Table 9 where we report two models. Model 1 is a logistic regression model, while Model 2 re-estimates the model using retailer random effects. Standard errors in the first model are clustered by firm.

The coefficient for the interaction between *Tax Rate*$_s$ and *Percent Direct*$_r$ is negative in both models. We conclude that direct retailers appear to avoid opening stores in high tax states. For robustness we also used OLS to re-estimate the same model with firm fixed effects. These coefficients reveal that for a retailer with 100% direct business, a 1% increase in the sales taxes rate decreases the probability of opening a store in that state by approximately 4%. This is similar to the marginal effect from the logistic regression model, which was approximately 3% (evaluated at the average observation). Recall that the benchmark rate at which stores opened is just 12%, indicating that these estimated effects are relatively large.

This finding occurs despite the evidence that other retailers are attracted to high tax states. It also occurs despite our finding in Section 2 that collecting sales taxes has little impact on catalog purchases. It is possible that retailers were simply unaware that collecting sales taxes does not appear to affect catalog sales. Alternatively, while most catalog retailers derived only a small proportion of their revenue from the Internet during this time period, this percentage was growing. Retailers were obviously aware of this growth, and this trend may have contributed to their decisions to avoid opening stores in high-tax states.

Notice that the interaction reported in Table 9 is not subject to the limitation that arises in

previous studies measuring the impact of state tax rates. As we discussed in the literature review, previous studies have grappled with the endogeneity of tax rates. We are interested in the interaction between state tax rates and the percentage of business that the firm conducts through direct channels. It is more difficult to identify alternative explanations for why only firms with a high percentage of direct business avoid entering states with high tax rates.

Although the sample of 450 observations may appear robust, it is drawn from just 14 firms. Augmenting the sample is difficult because it requires knowing the percentage of direct sales. One approach (suggested by a reviewer) is to simply add firms that did not have either an Internet channel or catalog channel during this period. When we add 421 observations from eleven apparel retailers without an Internet or catalog channel, the pattern and significance of the findings remains unchanged.

Retailers that Already Have a Physical Presence

The physical presence rule is invoked when the first store is opened in a state, so there is no incremental change in the tax policy if a retailer opens additional stores. We would not expect state tax rates to dissuade a retailer from opening additional stores, even if the retailer has a large percentage of direct sales. We can use the same sample of 14 retailers to test this prediction. In particular, we repeat our analysis focusing on states in which the 14 retailers already had a physical presence in 2001. Our dependent variable measures whether the firms had opened an additional store in these states by 2004. As expected, the interaction between the state tax rate and the percentage of business conducted through direct channels (*Tax Rate * Percent Remote*) no longer approaches significance. We caution, however, that because the *Retail Tenants'*

Directory does not always report the number of stores that a retailer has in a state, we were not always able to detect whether the number of stores had changed between 2001 and 2004. This limits the sample size for this additional analysis to 98 observations.

Anecdotal Evidence

Anecdotal evidence offers further support for the conclusion that state tax rates influence firm decisions. In 2004 Gateway Computers closed all its retail stores. Among the stated reasons for the move, sales taxes for online sales had apparently put Gateway at a disadvantage to other computer sellers. Cabela's and Bass Pro Shops, both large retailers of outdoor goods, have diverted corporate funds to lobbying to ensure they obtain concessions from many states on the collection of sales taxes.[5] Some firms have used "entity isolation," or the tactic of incorporating a subsidiary that owns all direct sales operations, to avoid having to collect taxes due to retail locations. This practice, however, is becoming more difficult as states become more aggressive in pursuing sales tax revenues. For example, in a recent case in California, the State Equalization Board found that Borders.com owed $167,000 in sales tax on Internet sales from 1998-2001. The Board based its reasoning on the fact that Internet customers had been allowed to return books to Borders' physical stores, despite the legally separate status of Borders and Borders.com.

Conclusions

In this paper, we have shown how current sales tax rules influence both customer and firm behavior. On the customer side, we study how an obligation to collect sales taxes affects purchases from a retailer's Internet and catalog channels. We find that while Internet orders

decrease by 11.6%, there is no apparent effect on catalog orders. Further investigation reveals that the absence of any change in catalog orders can be partly attributed to the difficulty of searching for lower prices at competing retailers. We show that even for the Internet channel, the introduction of sales taxes has little impact on sales when competitive search is unlikely to reveal lower prices. In particular, the introduction of sales taxes had no impact on sales for deeply discounted items in either channel.

Our firm-level analysis investigates whether a sample of multi-channel retailers' store expansion decisions are affected by state tax rates. We find that retailers that have a larger proportion of direct sales are less likely to enter a state with high sales taxes. We interpret these results as evidence that store expansion decisions are affected by the current discrepancy in tax rules.

References

Anderson, Eric T. and Duncan I. Simester (1998), "The Role of Sale Signs," *Marketing Science*, 17 (2), 139–55.

——— and ——— (2001), "Are Sale Signs Less Effective When More Products Have Them?" *Marketing Science*, 20 (2), 121–42.

Brynjolfsson, Erik, Michael D. Smith, and Alan L. Montgomery (2004), "The Great Equalizer? An Empirical Study of Consumer Choice at a Shopbot," mimeo, MIT.

Chetty, Raj, Adam Looney, and Kory Koft (2007), "Salience and Taxation: Theory and Evidence," NBER Working Paper No. 13330

Ellison, Glenn and Sara Fisher Ellison (2003), "Tax Sensitivity and Home State Preferences in Internet Purchasing," mimeo, MIT.

Goolsbee, Austan D. (2000), "In a World Without Borders: The Impact of Taxes on Internet Commerce," *The Quarterly Journal of Economics*, 115 (2), 561–76.

Manuszak, Mark D. and Charles C. Moul (2008), "How Far for a Buck? Tax Differences and the Location of Retail Gasoline Activity in Southeast Chicagoland," Mimeo, Washington University

Stemberg, Thomas G. (2005), "Staples Letter of Support for S. 2152," December 21, http://www.e-fairness.org/coalition/coalition.htm#westfield.

FOOTNOTES

1. 430 U.S. 274 (1977)
2. 504 U.S. 298 (1992)
3. In preliminary analysis (available from the authors) we also investigated whether the store opening affected the composition of customers' orders. We found no change in the average price of the items that customers purchased, or the average number of items in their orders.
4. For example, we excluded Lands End as it was acquired by Sears in the middle of our analysis period. Similarly, we excluded Orvis, because it charges sales taxes even in states in which it does not have a physical presence. This (conservative) policy is apparently designed to avoid liability for tax penalties that may arise because of ambiguities in the definition of a "physical presence".
5. New York Times, November 30, 2005.

Table 1. A Comparison of Transactions by State During the Historical Period

	Focal State	Neighboring State	Difference
Days since last purchase (Recency)	-612.14 (2.75)	-609.92 (2.56)	2.22 (3.75)
Number of orders placed (Frequency)	3.06 (0.05)	3.20 (0.05)	0.14 (0.08)
Average dollar value of orders (Monetary Value)	$85.65 (0.92)	$88.03 (0.87)	$2.38 (1.27)
Sample Size	4,963	5,682	

The data represent the mean of each variable. Standard errors are in parentheses.

Table 2. Average Number of Orders in the Pre and Post-Opening Periods

	Pre-Opening Period	Post-Opening Period	Difference	Differences-in-Differences
Internet Orders				
Focal State	0.197 (0.009)	0.164 (0.008)	-0.033** (0.009)	-0.023* (0.012)
Neighboring State	0.159 (0.008)	0.149 (0.008)	-0.011 (0.008)	
Catalog Orders				
Focal State	0.977 (0.022)	0.782 (0.022)	-0.195** (0.018)	-0.002 (0.024)
Neighboring State	0.915 (0.018)	0.722 (0.018)	-0.193** (0.016)	

The table presents the mean values of the number of orders placed during the pre and post-opening periods (12-months before and after the new store opened) calculated using 6,057 customers in the focal state and 6,964 customers in the neighboring state. The "difference" measure uses a paired-sample t-test. The "differences-in-differences" measure is derived by calculating the difference between the pre and post-opening periods in the number of orders placed by each customer, and then comparing the mean of these differences between customers in the focal and neighboring states. Standard errors are in parentheses.
*Difference significantly different from zero, $p < 0.05$.
**Difference significantly different from zero, $p < 0.01$.

Table 3. Poisson Regression

	Internet Purchases	Catalog Purchases	Internet + Catalog Purchases
Post-Opening * Focal State	-0.116* (0.056)	0.014 (0.028)	-0.006 (0.023)
Focal State	0.212* (0.095)	0.066 (0.039)	0.089* (0.041)
Post-Opening	-0.069 (0.044)	-0.237** (0.018)	-0.210** (0.016)
Constant	-1.836** (0.083)	-0.089** (0.025)	0.072* (0.030)
Log Likelihood	-10,667	-30,773	-34,083

The table reports the Poisson regression coefficients from estimating Equation 2 using pre and post-opening observations for the 6,057 customers in the focal state and 6,964 customers in the neighboring state (the sample size in each model is 26,042). The model includes random effects at the household level. The model is estimated separately using the number of orders placed through the Internet or catalog channels as the dependent variable. Statistical significance is computed using a panel bootstrap clustered by ZIP code, with 1,000 replications. Bootstrap standard errors are in parentheses.
*Significantly different from zero, $p < 0.05$.
**Significantly different from zero, $p < 0.01$.

Table 4. Linear Regression:
All Purchases, Internet Purchases and Catalog Purchases

	Internet Purchases	Catalog Purchases	Internet + Catalog Purchases
Post-Opening * Focal State	-0.023*	-0.002	-0.025
	(0.010)	(0.025)	(0.024)
Focal State	0.038*	0.062	0.100*
	(0.016)	(0.036)	(0.047)
Post-Opening	-0.011	-0.193**	-0.204**
	(0.007)	(0.014)	(0.016)
Constant	0.159**	0.915**	1.074**
	(0.013)	(0.023)	(0.031)
Log Likelihood	-24,806	-45,840	-48,285

The table reports the linear regression coefficients from estimating Equation 3 using pre and post-opening observations for the 6,057 customers in the focal state and 6,964 customers in the neighboring state (the sample size in each model is 26,042). The model includes random effects at the household level. The model is estimated separately using the number of orders placed through the Internet or catalog channels as the dependent variable. Statistical significance is computed using a panel bootstrap clustered by ZIP code, with 1,000 replications. Bootstrap standard errors are in parentheses.
*Significantly different from zero, $p < 0.05$.
**Significantly different from zero, $p < 0.01$.

Table 5. Robustness Check
Repeating the Analysis in Time Periods Without Store Openings

	1-year Before		1-year After	
	Internet	**Catalog**	**Internet**	**Catalog**
Pseudo Post-Opening * Focal State	0.016 (0.035)	0.024 (0.014)	-0.071 (0.074)	-0.025 (0.039)
Focal State	0.269** (0.071)	0.064 (0.039)	0.248** (0.078)	0.064 (0.045)
Pseudo Post-Opening	-0.333** (0.027)	-0.720** (0.010)	0.154** (0.034)	0.011 (0.029)
Constant	-2.207** (0.051)	0.728 (0.030)	-1.826** (0.061)	-0.426** (0.036)
Log-Likelihood	-6,578	-39,006	-11,702	-26,330

The table reports the Poisson regression coefficients from estimating Equation 2 using pre and post periods centered 1 year before and 1 year after the new store opened. The models are estimated using the 6,057 customers in the focal state and 6,964 customers in the neighboring state (the sample size in each model is 26,042) and include random effects at the household level. The dependent variable is the number of orders placed through the Internet or catalog channels. Statistical significance is computed using a panel bootstrap clustered by ZIP code, with 1,000 replications. Bootstrap standard errors are in parentheses.
* Significantly different from zero, $p < 0.05$.
** Significantly different from zero, $p < 0.01$.

Table 6. Poisson Regression:
Mail Purchases, Telephone Purchases, and Internet Purchases

	Mail	Telephone	Internet
Post-Opening * Focal State	-0.025 (0.116)	0.017 (0.029)	-0.116* (0.056)
Focal State	0.181 (0.117)	0.060 (0.044)	0.212* (0.095)
Post-Opening	-0.491** (0.097)	-0.227** (0.018)	-0.069 (0.044)
Constant	-3.210** (0.086)	-0.134** (0.027)	-1.836** (0.083)
Log Likelihood	-3,468	-29,938	-10,667

The table reports the Poisson regression coefficients from estimating Equation 2 using pre and post-opening observations for the 6,057 customers in the focal state and 6,964 customers in the neighboring state (the sample size in each model is 26,042). The model includes random effects at the household level. The model is estimated separately using the number of items purchased from the Internet, mail or telephone channels as the dependent variable. Statistical significance is computed using a panel bootstrap clustered by ZIP code, with 1,000 replications. Bootstrap standard errors are in parentheses.
*Significantly different from zero, $p < 0.05$.
**Significantly different from zero, $p < 0.01$.

Table 7. Purchases at Full Price and Discount in Both Channels

	Purchases at a Discount		Purchases at Full Price	
	Internet	Catalog	Internet	Catalog
Post-Opening * Focal State	0.044 (0.214)	0.059 (0.114)	-0.204* (0.084)	-0.006 (0.041)
Focal State	0.134 (0.200)	0.048 (0.084)	0.197* (0.100)	0.067 (0.049)
Post-Opening	0.016 (0.114)	0.052 (0.094)	0.045 (0.057)	-0.247** (0.030)
Constant	-2.533** (0.131)	-1.131** (0.070)	-1.250** (0.081)	0.602** (0.032)
Log Likelihood	-4,180	-14,577	-13,223	-41,789

The table reports the Poisson regression coefficients from estimating Equation 2 using pre and post-opening observations for the 6,057 customers in the focal state and 6,964 customers in the neighboring state (the sample size in each model is 26,042). The model is estimated separately using purchases made at the full price or at a discounted price from the Internet or catalog channels. Statistical significance is computed using a panel bootstrap clustered by ZIP code, with 1,000 replications. Bootstrap standard errors are in parentheses.
*Significantly different from zero, $p < 0.05$.
**Significantly different from zero, $p < 0.01$.

Table 8. Descriptive Statistics for the Retail Data

	Mean	Minimum	Maximum	Standard Deviation
Open Store in $2004_{r,s}$	0.12	0.00	1.00	0.33
Tax Rate_{s}	0.05	0.00	0.07	0.02
$\text{Percent Direct}_{r}$	0.67	0.11	1.00	0.30
$\text{Stores in State}_{s}$ (hundreds)	1.05	0.15	8.37	1.14
$\text{State Store Growth}_{s}$	0.08	-0.12	0.36	0.10
$\text{Bordering States}_{r,s}$	0.50	0.00	1.00	0.50
$\text{Other Close States}_{r,s}$	0.61	0.00	1.00	0.49
Apparel_{r}	0.54	0.00	1.00	0.50

The table reports summary statistics for the dependent and independent variables used in the firm analysis (Section 3). The sample size for all statistics is 450.

Journal of Marketing Research
Article Postprint, Volume XLVI

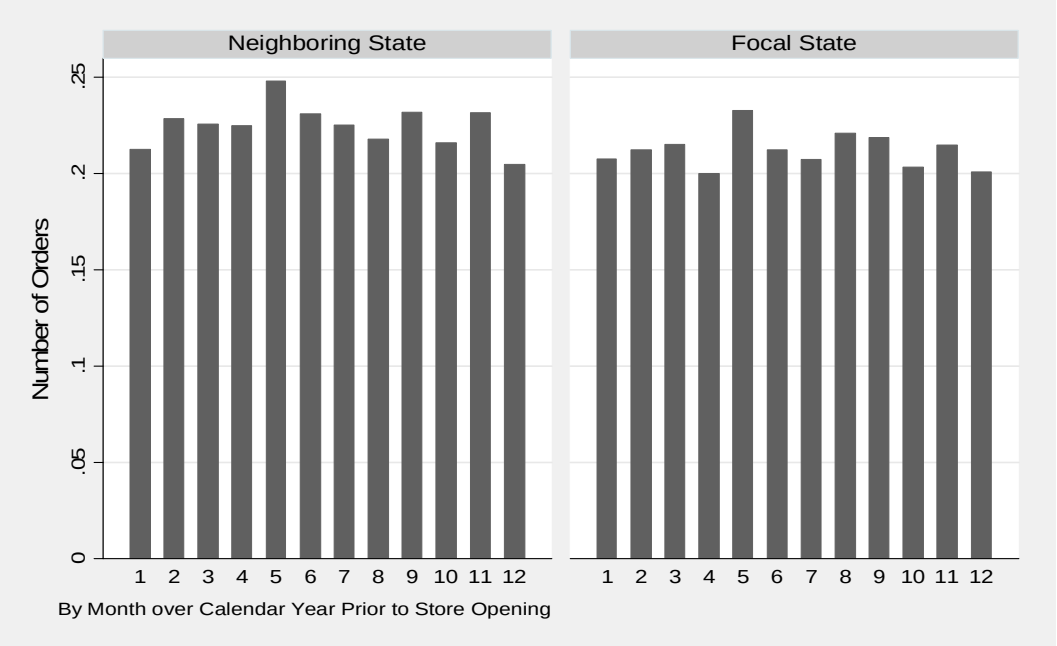
Table 9. Logistic Regression:
The Probability of Opening a First Store in a State

	Model 1	Model 2
Tax Rate$_s$ * Percent Direct$_r$	-116.403** (34.737)	-199.390* (82.862)
Tax Rate$_s$	74.552** (25.692)	133.233* (53.567)
Percent Direct$_r$	5.861** (1.882)	8.106 (6.257)
Stores in State$_s$	0.181 (0.163)	0.598* (0.278)
State Store Growth$_s$	5.808** (1.549)	7.132** (2.712)
Bordering States$_{r,s}$	1.820** (0.414)	0.560 (0.713)
Other Close States$_{r,s}$	1.692** (0.331)	0.983 (0.629)
Apparel$_r$	2.236** (0.784)	1.983 (3.358)
Constant	-10.349** (1.603)	-12.081** (3.620)
Log Likelihood	-126	-102
Sample size	450	450

The table reports logistic regression coefficients using the sample of states in which a retailer did not have a store in 2001. The dependent variable measures whether the retailer had opened a store in that state by 2004. The standard errors in Model 1 are clustered by firm, with asymptotic standard errors in parentheses. Model 2 includes firm random effects, with standard errors computed using a panel bootstrap clustered by firm.
*Significantly different from zero, $p < 0.05$.
**Significantly different from zero, $p < 0.01$.

Journal of Marketing Research
Article Postprint, Volume XLVI

Figure 1: Time-Trend of Items Purchased by Households in Each State by Month in the Calendar Year Prior to Store Opening



The data in the figure summarizes the average number of orders by customers in the focal and neighboring states in the 12 months before the new store opened. The averages are calculated separately for each of the 12 months in the pre-opening period.