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THE EFFECT OF SALES TAXES ON EMPLOYMENT: NEW EVIDENCE FROM CROSS-BORDER PANEL DATA ANALYSIS

Jeffrey P. Thompson and Shawn M. Rohlin

This paper estimates the effect of sales taxes on employment using county-level quarterly data and a "border approach," comparing employment changes for counties in states that raised sales tax rates with their cross-border neighbors. We augment standard distance-related border measures with an economically-oriented border measure based on the share of county residents who work in a neighboring state. We find that the employment effects are larger in the retail trade industry and for female workers, and that they are concentrated in counties with relatively large shares of residents working in another state.

Keywords: sales tax, border models, cross-border shopping

JEL Codes: H2, H7, R5

I. INTRODUCTION

State policy makers are interested in raising revenue in ways that will minimize disruption to economic activity. Economists have long understood that the larger the behavioral response to a tax change, the larger the resulting efficiency cost or deadweight loss. Policy makers and voters are likely to be less interested in deadweight loss, per se, but are keenly interested in the impact of taxes on employment. This is particularly true when the economy is in recession or growing very slowly — as it has been since late 2007, a period when the economy and job growth have consistently been voters' top priorities in national opinion polls.[1]

In this context, state border regions represent a potential concern for policy makers. In areas that border neighboring states it may be relatively easy for residents to take action to avoid paying certain taxes. In the case of sales taxes, large rate differentials

[1] For a collection of opinion polls taken in recent years, see http://www.pollingreport.com/consumer2.htm.

Jeffrey Thompson: Microeconomic Surveys Section, Federal Reserve Board Washington, DC, USA (jeffrey.p.thompson@frb.gov)

Shawn Rohlin: Department of Economics, Kent State University, Kent, OH, USA (srohlin@kent.edu)

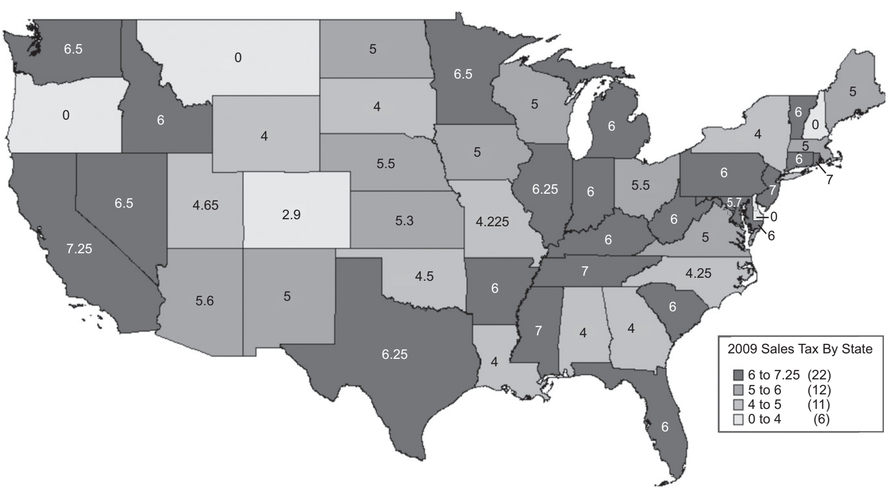
might motivate residents to simply cross the state line to shop, depriving a state of tax revenues, retail sales, and potentially jobs. Sales taxes are the single largest revenue source for state governments (accounting for 46 percent of state tax revenue in 2008), and the rate differentials between neighboring states are large in many cases. General sales tax rates range from zero (in four states) to 8.25 percent, and each of the continental states without a sales tax borders at least one other state with a rate of 6 percent or higher (Figure 1). Compared to other state taxes, it is also relatively easy to avoid the sales tax. For example, cross-border shopping entails considerably less disruption than moving to another state or changing jobs as would be required to avoid a state's income tax.

In part motivated by these factors, there is a relatively large empirical literature exploring "cross-border" shopping, recently surveyed by Leal, Lopez-Laborda, and Rodrigo (2010). Most of these studies, however, focus on taxable sales, tax revenues, and the implied deadweight loss of taxes. Only a handful of papers consider how cross-border shopping influences employment, and those findings have been inconclusive (Fox, 1986; Hoyt and Harden, 2005).

In this study, we use improved data and methods to obtain precise estimates that are fairly robust to alternative specifications. We use quarterly data from the Longitudinal Employer-Household Dynamics program of the U.S. Census Bureau for all counties in 47 states (excluding Alaska, Hawaii, Massachusetts, and Washington DC) between 2004 and 2009, and estimate the employment response in border counties to changes in state sales taxes. We use fixed effects estimators to identify the effect of changes in the general sales tax rate on county-level employment, hiring, and payroll, while

Figure 1

State Sales Tax Rates in 2009



also controlling for changes in the sales tax treatment of food. We first use a panel of counties and then a panel of cross-border county pairs, with the differences between the counties as the variables. We further estimate the impacts on various age, gender, and industry groups.

This study also extends the previous literature by explicitly incorporating variation in economically-determined distance measures into the estimates. Proximity to a state border is an imperfect measure of the cost of cross-state shopping. Some state borders are separated by lakes and rivers making travel difficult. In other cases, traffic and congestion may raise the time and fuel costs of travel considerably. When residents work and live in different states, though, cross-border travel costs and time are minimized. Accordingly, in some specifications we explore differential responses among border counties by the share of residents working in another state.

Using both a county fixed effect model and cross-border differencing model our results show that sales tax changes have a detrimental effect on employment, payroll, and hiring with the effect concentrated in border areas. Specifically, we find that a 1 percentage point increase in the sales tax rate in border counties decreases employment by between 3.8 percent (fixed effect model) and 5.8 percent (differencing model) for all industries. Consistent with the existing literature we find that much of the loss comes from the retail industry with a decrease in retail employment of 7.6 percent. Lastly, using the share of population that works in the neighboring state as a proxy for cross border commuting and therefore the extent of tax competition between the two areas, we find that the negative sales tax effect is predominately experienced in areas with the highest cross-border tax competition.

The next section briefly reviews the previous literature and describes our empirical approach to estimating the employment effect from sales tax changes, including a description of the data used and the sales tax policy changes under study. The following section includes the results from our different specifications. The final section discusses these findings and concludes.

II. IDENTIFYING THE EMPLOYMENT IMPACTS OF CROSS-BORDER SHOPPING CAUSED BY SALES TAXES

Previous studies of sales taxes and cross-border shopping have typically estimated basic local demand functions, where shopping is a function of income and prices in a county and in neighboring counties, as well as the cost of transportation, of the form

$$(1)\quad S_{it} = F(A_i,\ Y_{it},\ P^T_{ijt},\ P^U_{ijt},\ C_{ij}),$$

where P^T_{ijt} is the ratio of after-tax prices between county "i" and neighboring county "j" in the taxed sector, P^U_{ijt} is the ratio of prices in the non-taxed sector, and C_{ij} is the cost of travelling between the two counties. For example,

$$(2)\quad P^T_{ijt} = [\tau_{it} + (p_{it} - \theta\tau_{it})]/[\tau_{jt} + (p_{jt} - \theta\tau_{jt})],$$

where, p_{it} is the pre-tax price and θ is a scalar representing the portion of the sales tax incidence on the seller.[2] If the consumer faces the full incidence of the tax, then θ is equal to zero and the after tax price in county "*i*" is simply $(\tau_{it} + p_{it})$.

The anticipated relationship between relative prices and shopping is negative. As own-prices rise relative to neighboring counties, shopping declines, or

$$(3) \quad \frac{\partial S_i}{\partial P_{ij}} < 0.$$

General demand equations include prices of taxable and non-taxable goods, as in (1), but empirical work on cross-border shopping typically assumes that the ratio of prices of non-taxed items between adjacent cross-border counties is equal to one, and can be excluded from the estimation. Studies differ in how they treat pre-tax prices for taxable items. In the conceptual model used by Fox (1986), the ratio of pre-tax prices for taxable goods is not assumed to be one or to be constant. Lacking data for actual pre-tax prices, though, Fox uses factors influencing pre-tax prices (including automobile travel costs and tax rates) in his empirical model. In his estimates using quarterly data, though, automobile costs are highly collinear and drop out. In their study of cross-border shopping, Walsh and Jones (1988) treat the ratio of pre-tax prices as equal to one, assuming that input costs are equal on both sides of the cross-state border.[3] In this study, we follow Walsh and Jones and effectively assume input costs are equal on both sides of the cross-state border.

Decades ago Mikesell (1970, 1971) and Fisher (1980) examined the influence of sales taxes on cross-border shopping at the county and city level. Like most of the more general cross-border shopping literature, however, these early studies focused on sales, not employment. More recent analysis by Fox (1986) and by Hoyt and Harden (2005) has explored employment effects. The findings from these studies are suggestive, but remain inconclusive. Both Fox (1986) and Hoyt and Harden (2005) find that sales tax increases reduce employment, relative to cross-border counties, but in both cases the findings are statistically insignificant at standard levels, and are sensitive to the particular specification. Fox (1986) uses quarterly county-level panel data, and compares border counties in several Tennessee metropolitan statistical areas (MSAs) to their cross-border counterparts in Kentucky, Georgia, and Virginia.[4] Most of the sales tax coefficients from the various specifications explored by Fox (1986) were statistically insignificant. Fox does report findings from regressions using total

[2] This expression simplifies to ratios of $(1 - T + p)$ in the two areas, but the depiction in (2) makes the separation between the tax and the pre-tax price clear.

[3] Walsh and Jones also assume that firm cost structures are identical on both sides of the border.

[4] Fox (1986) transforms level variables to "relative" variables by, in the case of employment for example, dividing the employment of County_i by the total employment in County_i plus that in its cross-border pair, County_j.

employment as the dependent variable that indicate a 1 percentage point increase in the sales tax rate in Tennessee results in a 4.7 percent reduction in relative employment in the Tennessee portion of the Clarksville/Hopkinsville MSA relative to the non-Tennessee portion, but only a 0.32 percent reduction in the Tri-Cities (Kingsport/Johnson City/Bristol) MSA relative employment from the same size of sales tax change.

Hoyt and Harden (2005) use county-level panel data with annual observations for MSAs in all 50 states. They use county-level fixed effects, and explore the differential response among border and "interior" MSAs by estimating separate equations for the two groups. The results for border MSAs also include variables reflecting the sales tax rate of neighboring counties (weighted by the county share of total MSA population). Coefficients from the main specification are negative but insignificant for own-county sales taxes, and positive and insignificant for neighboring county sales taxes.

III. OUR APPROACH AND THE DATA

In this paper we use quarterly data at the county level to study the employment effects of sales tax rate changes in 16 states between 2004 and 2009. We use standard border methods, including various fixed effects panel regressions, and also augment the standard border method by considering the effects of the number of county residents who work in another state.

A. Changes in the Sales Tax Rate

Between 2004 and 2009 there were 20 general sales tax changes in 16 states (Table 1). The average cumulative percentage point tax rate change in these states was 1.0 percentage point, with the largest increase in California (2.5 percentage points) and the smallest in Washington DC, which raised its rate 0.25 percentage points in the fourth quarter of 2009. Seven states also modified their sales tax treatment of food purchased for home consumption over this period, with all states lowering their rates. Three of those states fully exempted food from the general sales tax.

Sales tax rate changes between 2004 and 2009 were implemented during each of the calendar quarters. Four of the twenty changes were implemented in the first and second calendar quarters, while six changes were implemented in the third and fourth. Using annual average data following Hoyt and Harden (2005) makes it harder to identify the impact of sales tax changes due to aggregation bias. Sales tax changes can occur in any calendar quarter, and the annual average employment level combines pre- and post-tax change quarters. Because the particular quarter when the policy change is implemented varies over states and over time, using quarterly data provides additional variation for identification.

The sales tax rate we use is the actual statutory rate in the state's general sales tax. This is the same as in most other studies (Fox, 1986; Walsh and Jones, 1988). The

Table 1

States Changing General Sales Tax Rates in 2004–2009

	2003	2004	2005	2006	2007	2008	2009	2010
Arkansas	5.13	5.13	6	6	6	6	6	6
California	6	6	6.25	6.25	7.25	7.25	7.25	8.25
District of Columbia	5.75	5.75	5.75	5.75	5.75	5.75	5.75	6.0
Idaho	5	6	6	6	6	6	6	6
Indiana	6	6	6	6	6	6	6	7
Iowa	5	5	5	5	5	5	5	6
Maryland	5	5	5	5	5	6	6	6
Massachusetts	5	5	5	5	5	5	6.25	6.25
Minnesota	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.875
Nevada	6.5	6.5	6.5	6.5	6.5	6.5	6.85	6.85
New Jersey	6	6	6	6	7	7	7	7
North Carolina	4.5	4.5	4.5	4.5	4.25	4.25	4.50	5.75
Ohio	5	6	6	5.5	5.5	5.5	5.5	5.5
South Carolina	5	5	5	5	5	6	6	6
Vermont	5	6	6	6	6	6	6	6
Virginia	3.5	3.5	4	4	5	5	5	5

Source: Tax Foundation, http://taxfoundation.org:81/article/state-sales-gasoline-cigarette-and-alcohol-tax-rates-state-2000-2010.

sales tax rate used in Hoyt and Harden (2005), though, is the effective sales tax rate which divides sales tax revenue by personal income.[5] This choice of tax rate introduces the possibility that changes in the denominator (a county's personal income) are influencing the effective tax rate in ways unrelated to the costs of shopping in another county. Also, because they smooth the local component of the sales tax over five years, Hoyt and Harden's (2005) tax rate measure dampens the actual variation in statutory sales tax rates, and arbitrarily assigns equal changes over the five years spanned by the Census of Governments, regardless of the year in which an actual law change

[5] Annual State-level sales tax revenue is from the Census Bureau's Survey of State Government Finances (http://www.census.gov/govs/statetax/) while the county level sales tax figures are produced every five years by the Census of Governments (http://www.census.gov/govs/cog2012/). The county level annual collections are estimated by Hoyt and Harden (2005) by smoothing the data over the intervening years.

may have occurred. We use the actual sales tax rate in order to avoid some of these issues.[6]

B. Quarterly UI-based Data (Quarterly Workforce Indicators)

The primary data used in this paper are the Quarterly Workforce Indicators (QWI) from the Longitudinal Employer-Household Dynamics (LEHD) program of the U.S. Census Bureau. These data are based on Unemployment Insurance (UI) wage records made available through a data sharing arrangement between the Census Bureau and 49 states. Over the 2004 to 2009 period, we have quarterly data for 47 continental states (Massachusetts and Washington, DC are excluded).

The QWI data include counts and means of quarterly employment and earnings information by county, age-group, ownership status, gender, and broad-industry group for all workers in all establishments covered by UI in those states.[7] Some of the regressions also include county-level income data from the Bureau of Economic Analysis (BEA). Since these income data are only available annually, these specifications include only one calendar quarter from each year.

There are a number of important advantages of the QWI data. Because the data are based on UI wage records, results even for individual small counties are available and reliable, whereas they would not be in a standard survey.[8] Additionally, because the data are quarterly, empirical tests can be closely tailored to the timing of the policy, instead of relying on annual averages which might dampen the impacts. Also, there are

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6 Note that this paper focuses on state sales tax changes and not local sales tax changes. Local sales tax data are difficult to obtain for all counties, or even all major cities, in the United States, particularly for previous years. Although it would be ideal to include local sales taxes in our analysis, due to some endogeneity concerns with local sales taxes and the difficulty in collecting such data, we follow the existing literature by omitting local sales taxes. To be clear, this omission could potentially lead to biased results. If local sales tax changes are positively correlated with state sales tax changes then we could be overestimating the effect of sales tax changes. On the other hand, if local municipalities decrease sales taxes to counteract the negative effect of the state sales tax then our results could be underestimating the true effect of sales taxes. Finally, if local sales tax changes are uncorrelated with state sales taxes, they could be contributing to measurement error and biasing our results toward zero.

7 The QWI data are described in detail in working papers by principal investigators and staff at the LEHD, including Abowd et al. (2006). Access to the underlying LEHD "infrastructure" files is limited. Two public-use versions of the data, referred to as the Quarterly Workforce Indicators, are available. Eight QWI variables, including employment, earnings, turnover, separations, and hires can be accessed at a website targeted to "workforce development" practitioners. For this study, the full QWI data were accessed through the Cornell Institute for Social and Economic Research using the Cornell VirtualRDC. Only data for private sector employment are used.

8 The QWI data are subject to a distortion procedure designed to protect confidentiality of the underlying data, but also retain "analytic validity" for researchers. As Abowd et al. (2006, pp. 45–46) explain, "the statistical properties of [the primary means of] distortion are such that when the estimates are aggregated, the effects of the distortion cancel out for the vast majority of the estimates, preserving both cross-sectional and time-series analytic validity." Estimates based on three or fewer persons or firms are suppressed entirely in the QWI.

several variables in the QWI that are not present in other data sets that can be explored as possible responses to the sales tax, including hiring decisions and payroll, which would reflect joint changes in employment as well as hours.

But there are also several disadvantages of the QWI data. Notably, the QWI do not include the detailed demographic information available in the confidential LEHD files, let alone standard labor market surveys. Previous studies in this literature, however, have not controlled for demographics, and the QWI does include employment totals by gender and age group, which we explore in this paper. The 47 continental states that are included in the QWI over the full range of years that we study contain 3,094 counties.

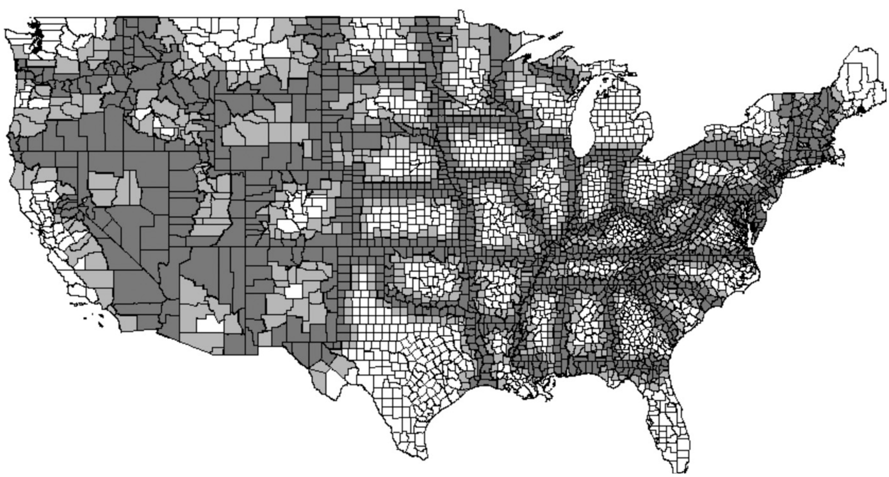
C. Distance Measures — Geography and Economy

Similar to Fox (1986) and Hoyt and Harden (2005), this study employs a border approach. Cross-border shopping is more prevalent when transportation costs are low. It is typically easier for residents of border counties to travel across the state line to take advantage of lower after-tax prices than it is for residents of the interior of the state. The impact of sales tax differences on shopping and employment is expected to dissipate with distance from the border of the state. For the purposes of identification, the border method, as emphasized in the analysis by Holmes (1998), allows comparisons between neighboring areas that are part of the same labor market and presumably differ only as a result of the time-varying cross-state tax differential we are studying.

Figure 2 is a county map of the United States that highlights counties on the state border (shaded in dark gray), "interior border" counties that are immediately adjacent

Figure 2

U.S. County Map, Showing Border, Interior Border, and Interior Counties



to the border counties (shaded in light gray), and interior counties that are neither on or next to the border (shaded in white). In some portions of the empirical analysis we focus exclusively on the cross-border county pairs, and in other parts of the analysis we include all counties and consider the differential responses based on proximity to the border.

Initially, we estimate a standard fixed effects model on the panel of counties

$$(4) \quad EMPLOYMENT_{it} = \alpha + \beta_1 SalesTax_{it-1} + \beta_2 X_{it} + \gamma_i + \delta_{t1} + \delta_{t2} + \varepsilon_{it},$$

where $SalesTax_{it-1}$ is the statutory general sales tax rate in county i in period $t-1$, and X_{it} is a vector including the tax treatment of food, and, in some cases, a measure of personal income. These level specifications also include fixed effects for county (γ_i) and for year and quarter ($\delta_{t1,2}$). Hoyt and Harden (2005) estimate separate equations for border MSAs and interior MSAs, similar to (4) estimated separately for the two groups of MSAs.[9] We include all counties, and interact the sales tax rate variable with a border county indicator, or

$$(5) \quad EMPLOYMENT_{it} = \alpha + \beta_1 (SalesTax_{it-1} * BORDER_i) + \beta_2 X_{it} + \gamma_i + \delta_{t1} + \delta_{t2} + \varepsilon_{it}.$$

First we use a binary border indicator where a county either is or is not on the state border. We then consider a categorical interaction term, which allows a county to be on the border ("border"), immediately adjacent to the border county ("interior border"), or in the interior of the state. If the effect of the sales tax dissipates as anticipated, the employment effect should be smallest in the interior (here represented as the "main" effect, as the interior counties are the excluded group in the border indicator), somewhat larger on the "interior border," and largest on the border.

We also lag the policy change by one quarter, to allow some time for households to adjust their shopping behavior. We use the natural log of county-level employment as the dependent variable. We use robust standard errors to allow for unknown forms of heteroskedasticity, and cluster standard errors at the state-level to allow for an arbitrary variance-covariance structure within each state.

Next we estimate a differenced panel fixed effects estimator. Using only the border counties, we calculate the difference in employment and sales tax rates for each cross-border county pair and include fixed effects for the cross-border county-pair. The identifying assumption in all of these fixed effects specifications is that it is the sales tax variation that is driving the observed employment differences, not other factors the vary across counties and over time, and are hence not absorbed by the county fixed effect. This assumption is more likely to hold when we include only counties adjacent to the state border, and directly compare cross-border pairs of counties. Cross-border pairs are assumed to be part of the same labor market and influenced by the same economic

[9] Hoyt and Harden (2005) also include the sales tax rate of neighboring counties. We incorporate neighboring county sales tax rates in the difference-in-differences specifications, described below.

factors, other than policy differences between the states. Similar to Rohlin, Rosenthal, and Ross (2012), we use these cross-border differences as the dependent variable and the independent variables of interest and estimate

$$(6) \quad DIFF\_EMP_{ijt} = \alpha + \beta_1 DiFF\_SalesTax_{ijt-1} + \beta_2 X_{ijt} + \gamma_{ij} + \delta_{t1} + \delta_{t2} + \varepsilon_{ijt}.$$

The dependent variable, similar to the other differenced variables in the specification, is calculated as the difference in employment between the two counties in each cross-border county-pair

$$(7) \quad DIFF\_EMP_{ijt} = EMPLOYMENT_{it} - EMPLOYMENT_{jt}.$$

Other differenced covariates ($DIFF\_X_{it}$) include the food sales tax rate, and in some specifications personal income. These differenced specifications include year and quarter fixed effects ($\delta_{t1,2}$) as well as county-pair-level fixed effects (γ_i). Effectively, the key coefficient (β_1) reflects differences from the average over time for the county-pair.

Cross-border county pairs, however, may be imperfect measures of the feasible alternative shopping locations. In some cases, cross-border counties are separated by rivers or lakes with no available bridge or commercial ferry service. These cases can be excluded, at the cost of losing most observations in the data, by focusing exclusively on MSAs (Hoyt and Harden, 2005). In some cases, though, traveling between counties within an MSA is time consuming (due to congestion or limited public transportation) and costly (due to tolls, gas, and parking). The potential after-tax cost savings is the factor that motivates cross-border shopping, and geographic proximity to the border is simply a proxy for cost. We explore an alternative proxy based on the share of county residents working outside of the state. More cross-state employment among cross-border pairs is a further sign of the relative ease of transportation between the states. The share of employed residents working in another state ranges from 0 to 66 percent, with a mean of 4.2 percent. Among the 1,135 border counties in our data, the share working in another state also ranges from 0 to 66 percent, with an average of 9.2 percent. Limiting the data to only the 286 border counties in MSAs, the share working in another state ranges from 0.6 to 56 percent, with a mean of 12 percent.

Residents crossing the border to work have already incurred the costs of traveling to the other state, so the additional costs associated with taking advantage of sales tax rate differentials should be low. Cross-border county pairs with greater concentrations of out-of-state employment are expected to exhibit larger reactions to cross-state tax differentials. We explore the influence of cross-state employment by separately estimating (6) for high and low cross-state employment groups. Dividing the number of cross-border pairs into four roughly equal groups, we estimate (6) for pairs with less than 9 percent (combined), with 9 to less than 16 percent, with 16 to less than 26 percent, and with 26 percent or more working in another state.[10]

[10] The share of county residents working in another state is calculated using the 2000 Census, and is calculated separately for both counties in each pair, so the combined out-of-state work share could be as high as 200 percent if all residents in both counties worked in a state other than the state of residence.

D. Effects by Age and Gender and Industry

While some previous studies have explored differences in sales tax effects across industries (Fox, 1986), none have explored the existence of a differential impact on various worker-types. Being able to identify effects across sub-groups might be important both in more precisely identifying the employment effects of sales tax changes, and also in helping policy makers weigh the potential trade-offs between tax revenue and job loss. If sizeable job losses are only present for a small sub-set of workers, data including all types of workers will make it difficult to estimate employment effects precisely. Also, job losses among groups with weak labor force attachment might be weighed more heavily than for groups fully attached to the labor force. The QWI include employment data for different age and gender groups. We exploit these data and estimate separate specifications (both county fixed effects and cross-border pair fixed effects with differenced variables) for all combinations of male, female, workers age 14 to 18, and age 19 to 21, as well as for both genders combined, and all ages combined.

E. Alternative Dependent Variables

If employers reduce employment in response to tax-induced reductions in sales, then payroll should also be expected to decline. Firms reducing their overall employment will also reduce their hiring. We use additional variables in the QWI to explore each of these possible outcomes.

IV. RESULTS

We begin by presenting results for the panel fixed-effects models including various interactions between border indicators and sales tax rates specified in (5). We present results for various age and gender groups. However, most of the results shown below will be based on our panel of cross-border county pairs using differenced variables (6). These specifications will also be estimated separately for age, gender, and industry group, as well as for different groups based on the extent of cross-border work in the county-pair.

A. Panel Fixed Effects Results

Table 2 column 1 presents results from (4) on our sample of counties without using any "border effect" and indicates that sales tax changes have an extremely small and insignificant effect on employment. However, after accounting for the county's relationship to the state border, estimated with (5), column 2 shows a larger negative and statistically significant effect. The coefficient on the interaction between the sales tax variable and an indicator for state borders suggests that employment declines 3.6 percent in response to a one point increase in the sales tax rate. If we allow for a somewhat more nuanced border relationship that reflects not only border counties, but also those interior counties that are adjacent to the border counties ("interior borders"), we see the familiar geographic attenuation pattern observed in much of the urban literature (Rosenthal and Strange, 2008; Arzaghi and Henderson, 2008). As indicated in column 3, there is a relatively large and significant impact for border counties and a smaller

Table 2

County Panel Regression Results Using Border-Relation Interactions, by Age and Gender Groups

Age	14–99	14–99	14–99			14–18			19–21		
Gender	Both	Both	Both	Men	Women	Both	Men	Women	Both	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Sales tax	–0.0101	0.0318	0.0657	0.0673	0.0751	0.0748	0.0645	0.0799	0.0751	0.0632	0.0634
	(0.0475)	(0.0507)	(0.0666)	(0.0680)	(0.0645)	(0.0594)	(0.0566)	(0.0580)	(0.0669)	(0.0610)	(0.0579)
*Sales tax **			–0.0479	–0.0463	–0.0545	–0.0512	–0.0412	–0.0654	–0.0523	–0.0417	–0.0385
Interior border			(0.0491)	(0.0513)	(0.0479)	(0.0420)	(0.0404)	(0.0407)	(0.0543)	(0.0440)	(0.0423)
*Sales tax **		–0.0680**	–0.104*	–0.102*	–0.109**	–0.0841*	–0.0825*	–0.0910*	–0.104*	–0.0835*	–0.0927**
State border		(0.0322)	(0.0529)	(0.0538)	(0.0519)	(0.0478)	(0.0436)	(0.0454)	(0.0560)	(0.0478)	(0.0436)
Interior border			0.237	0.233	0.259	0.263	0.202	0.319	0.273	0.217	0.200
			(0.261)	(0.274)	(0.255)	(0.224)	(0.213)	(0.220)	(0.286)	(0.228)	(0.216)
State border		0.338*	0.519*	0.521*	0.528*	0.395	0.375	0.420	0.493	0.374	0.434*
		(0.179)	(0.287)	(0.289)	(0.286)	(0.264)	(0.238)	(0.252)	(0.301)	(0.258)	(0.237)
Observations	72,888	72,888	72,888	72,888	72,864	72,227	71,571	71,679	72,389	71,741	71,462

Notes: The sales tax variable is lagged one quarter. All regressions include state, year, and quarter fixed effects. Asterisks denote p-values of p<0.01 (***), p<0.05 (**), and p<0.1 (*). Robust standard errors are in parentheses.

effect for interior borders (still negative, but not significant). The coefficients in column 3 indicate a 3.8 percent reduction in employment (the border county effect added to the main effect) in response to a one point increase in a state sales tax rate — an elasticity of 0.19 — but no change in the interior border counties.[11] None of the coefficients on the interior counties are significant, and all of the signs are positive.

The remaining columns in Table 2 estimate this specification separately for different age and gender groups. These results suggest that employment effects are somewhat larger for females, and smaller for youngest workers — age 14 to 18. These same specifications are replicated using only data from one quarter for each year, and also including a covariate for county personal income (Appendix Table 1). Personal income is part of the local demand function (1), but is not available quarterly at the county (or state) level, and thus has not been included in the specifications presented in Table 2. The coefficient on personal income is positive and highly significant for each age-group and gender combination. The sales tax coefficients, however, are nearly identical to the previous specifications which included all calendar quarters, but did not include personal income. Coefficients for the sales tax treatment of food are not shown in these tables (or later ones) for both space reasons and because the effects are very small and never statistically different from zero.

B. Cross-Border Differenced Panels

Including the information from the sales tax rates for cross-border counties and restricting the sample to only include border counties yields the coefficients shown in Table 3, which suggest somewhat larger employment effects. Including all ages of workers, both genders and all industries, we see that a one point increase in the sales tax relative to the cross-border pair results in an employment loss of 5.8 percent, or an implied elasticity of 0.29 (Panel A, column 1). The other specifications in Table 3 show the differenced panel results for different age, gender and industry groups. For all but one of the age and gender groups, the estimate of the employment impact is larger in the retail trade sector (Panel B) than for all industry sectors combined (Panel A). For the broadest group of workers, the impact on retail trade is 30 percent larger than for all industries combined (Panel A, column 1 relative to Panel B, column1). The coefficient for females is also somewhat larger for both industry groups and for each age group. Among teens (age 14 to 18) in retail trade, for example, the employment effect is 30 percent larger for females.

C. Results by Extent of Working Out of State

Previous studies have used border methods to study the sales tax because retail establishments have to compete with their neighbors in the adjacent state, and cross-border neighbors are arguably similar except with respect to state and local polices. However,

[11] With a mean sales tax rate of 5 percent over the sample period, a 1 percentage point change is equivalent to a 20 percent increase in the sales tax rate.

Table 3

Differenced County-Pair Panel Regression Results, by Age, Gender, and Industry Groups

Panel A. All Industries									
Age	14–99			14–18			19–21		
Gender	Both	Male	Female	Both	Male	Female	Both	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sales tax Rate Difference	–0.0583*	0.0634**	–0.0743**	–0.0573**	–0.0533*	–0.0588*	–0.0642**	–0.0570*	–0.0685**
	(0.0309)	(0.0282)	(0.0300)	(0.0261)	(0.0275)	(0.0307)	(0.0283)	(0.0304)	(0.0304)
Observations	26,111	26,048	26,139	26,064	26,026	25,936	26,046	26,040	26,018
Panel B. Retail Industries									
Sales tax Rate Difference	–0.0759**	–0.0774**	–0.0810**	–0.0763**	–0.0513*	–0.0671**	–0.0853***	–0.0817**	–0.0850**
	(0.0334)	(0.0306)	(0.0304)	(0.0285)	(0.0302)	(0.0274)	(0.0281)	(0.0312)	(0.0318)
Observations	25,576	25,572	25,662	25,402	24,954	25,081	25,263	24,417	24,933

Notes: Asterisks denote p-values of p<0.01 (***), p<0.05 (**), and p<0.1 (*). Robust standard errors are in parentheses.

the border approach implicitly assumes that all border areas have similar accessibility to the neighboring state and therefore the cross-border competition for consumers is the same for all border pairs. This implies that the effect of the sales tax should be uniform for all border pairs. However, geographical attributes, such as rivers and mountains, can impede access to the neighboring state, potentially decreasing tax competition and dampening the effect of sales tax changes. In addition, variations in transportation infrastructure and traffic congestion result in differences in travel costs and time for similar geographic distances and miles traveled.

To address these concerns, we use the county's share of employment that works in a neighboring state as a proxy for accessibility between the two counties. We then use this measure to determine whether the effect of sales tax changes is greater in county pairs that have more cross border commuting and therefore are in greater competition. Table 4 presents analysis of the effects of the sales tax on employment by the extent of cross-state work share. Panel A shows the results across all industries while Panel B presents estimates for retail trade. Column 1 reproduces our original results, which do not consider the extent of working out of state. Both panels show modest negative and statistically significant effects from sales tax increases, with an employment loss of 5.8 percent for all industries and 7.6 percent for retail firms from a 1 percentage point increase in the sales tax. Interestingly, columns 2 and 3 show that there is no measurable effect of the sales tax in border areas with few people working in the adjacent state.

Table 4

Differenced County-Pair Panel Regression Results, by Industry Group and Share of Employed Working in Another State

		Cross-State Work Share			
	All (1)	<9 Percent (2)	9 to <16 Percent (3)	16 to <26 Percent (4)	≥26 Percent (5)
Panel A. All Industries					
Difference in sales tax rate	–0.0583*	0.00251	0.0190	–0.133**	–0.138**
	(0.0309)	(0.0606)	(0.0481)	(0.0656)	(0.0553)
Panel B. Retail Industries					
Difference in sales tax rate	–0.0759**	0.0290	–0.00184	–0.164**	–0.172**
	(0.0334)	(0.0649)	(0.0476)	(0.0670)	(0.0655)

Notes: Regressions Include county-pair, year, and quarter fixed effects. The results are also for all age groups and both genders. Asterisks denote p-values of p<0.01 (***), p<0.05 (**), and p<0.1 (*). Robust standard errors are in parentheses.

Conversely, columns 4 and 5 indicate that the sales tax disproportionately affects areas that have a larger share population share working in the neighboring state. Specifically, columns 4 and 5 show that in county pairs with at least 16 percent of the working population employed in another state, a 1 percentage point increase in the sales tax decreases employment in all industries by roughly 13 percent, with both coefficients statistically significant. Column 5 of Panel B indicates that these employment losses are more heavily pronounced in retail trade with a statistically significant employment loss of 17 percent. Overall, these results suggest that border areas are disparately affected and ignoring this factor could lead to biased estimates.

D. The Effect of Sales Tax on Payroll and Hiring

Thus far, we have focused on employment changes to understand how sales taxes affect the local labor markets. In this section we extend the analysis to determine whether payroll and hiring in these border counties have been affected by changes in sales taxes. Both payroll and hiring are important to local policymakers. Changes in local payrolls are important because they directly affect local tax revenues, while hiring is critical because it is a main driver of growth in the local economy. Therefore, Table 5 shows the effects of sales tax changes on payroll in Panel A and on hiring in Panel B, using cross-border county pairs fixed effects and differenced variables.

Table 5

Differenced County-Pair Panel Regression Results, Effects on Payroll and Hiring, by Share of Employed Working in Another State

		Cross-State Work Share			
	All (1)	Less than 9 Percent (2)	9 to 15 Percent (3)	16 to 25 Percent (4)	26 Percent and Higher (5)
Panel A. Payroll					
Difference in sales tax rate	–0.0554	–0.00204	0.0334	–0.119	–0.150**
	(0.0340)	(0.0643)	(0.0534)	(0.0770)	(0.0593)
Panel B. Hiring					
Difference in sales tax rate	–0.0674**	0.00601	0.00690	–0.156**	–0.141**
	(0.0319)	(0.0619)	(0.0465)	(0.0628)	(0.0543)

Notes: Regressions include county-pair, year, and quarter fixed effects. The results are also for all industries, age groups and both genders. Asterisks denote p-values of p<0.01 (***), p<0.05 (**), and p<0.1 (*). Robust standard errors are in parentheses.

Column 1 displays the estimates for all counties, while columns 2 through 5 analyze the effects when the degree to which the population works in the neighboring state is considered. Using all counties we find that a 1 percentage point increase in sales tax rate causes a 5.5 percent decrease in payroll (although the coefficient is not statistically significant), and a 6.7 percent decrease in the number of new hires (statistically significant at the 5 percent level). Columns 2 through 5 illustrate a similar pattern. In counties with relatively little cross border traveling there is no clear effect of a sales tax increase on the neighboring county. However, the results differ for counties with large amounts of cross-border competition. For payroll, column 4 shows that a 1 percentage point increase in the sales tax rate decreases payroll by roughly 11.9 percent (not statistically significant), while column 5 shows a loss of 15 percent of payroll (statistically significant at the 5 percent level) for the counties with the most cross border competition. The amount of hiring that occurs in a county seems to be similarly affected with no discernable effect in counties with less cross border commuting (columns 2 and 3) and statistically significant negative effects of roughly 14 to 15 percent (columns 4 and 5) for those counties with at least 16 percent of their population working in the bordering state. In general the results from Table 5 indicate that payroll and the amount of new hiring are negatively impacted by changes in sales taxes, but primarily in areas with greater cross-border commuting.

V. DISCUSSION AND AREAS FOR FUTURE WORK

The primary reason states raise sales tax rates is to generate additional revenue to finance basic public services. Those public services, including public safety and education, are generally valued by residents, but the employment impacts we identify in this paper make clear that those revenue increases come at an economic cost for a state's border region.

These findings should be of interest to policy makers and researchers alike, but they are also limited in some important ways. The results in this study are based on state-level policy changes, and do not include information on local option sales taxes. Recent research has found that shopping behavior is influenced by intra-state sales tax differences (Cornia et al, 2010). Also, Agrawal (2011) uses border analysis and finds that local taxes are lower on the side of the border with higher state-level sales taxes. If local governments respond to state law changes in ways that reduce the combined state-local sales tax differential across state borders, the findings in this paper could underestimate the true effect of sales taxes. In future work, we will incorporate variation over time into this analysis.

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Appendix Table 1

Annual Data, Panel Regressions Using Border-Relation Interactions, by Age Group and Gender

Gender	Both				Women			Men		
Age	14–99 (1)	14–99 (2)	14–18 (3)	19–21 (4)	14–99 (5)	14–18 (6)	19–21 (7)	14–99 (8)	14–18 (9)	19–21 (10)
Sales tax	–0.0595	0.0148	0.0529	0.0353	0.0220	0.0506	0.0343	0.00402	0.0419	0.0357
	(0.0743)	(0.0850)	(0.0672)	(0.0851)	(0.0826)	(0.0676)	(0.0809)	(0.0886)	(0.0661)	(0.0822)
*Sales tax * Interior border county*		–0.0293	–0.0413	–0.0411	–0.0258	–0.0467	–0.0297	–0.0249	–0.0327	–0.0331
		(0.0420)	(0.0376)	(0.0459)	(0.0424)	(0.0392)	(0.0451)	(0.0418)	(0.0368)	(0.0425)
*Sales tax * State border county*		–0.104**	–0.0924**	–0.107**	–0.104**	–0.0930**	–0.106**	–0.0998**	–0.0851**	–0.0964**
		(0.0459)	(0.0408)	(0.0489)	(0.0451)	(0.0414)	(0.0452)	(0.0464)	(0.0403)	(0.0462)
Interior border county		0.234	0.273	0.296	0.215	0.296	0.231	0.211	0.230	0.252
		(0.214)	(0.196)	(0.231)	(0.215)	(0.207)	(0.229)	(0.212)	(0.192)	(0.214)
State border county		0.579**	0.486**	0.581**	0.570**	0.492**	0.571**	0.564**	0.451**	0.524**
		(0.246)	(0.218)	(0.257)	(0.242)	(0.224)	(0.240)	(0.249)	(0.214)	(0.244)
Personal income	2.931***	2.927***	2.644***	2.793***	2.904***	2.633***	2.802***	2.966***	2.624***	2.761***
	(0.269)	(0.272)	(0.282)	(0.290)	(0.275)	(0.280)	(0.291)	(0.278)	(0.278)	(0.287)
Observations	18,222	18,222	18,055	18,097	18,216	17,919	17,868	18,222	17,900	17,935

Notes: Asterisks denote p-values of p<0.01 (***), p<0.05 (**), and p<0.1 (*). Robust standard errors are in parentheses.