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Local sales taxes, employment, and tax competition☆

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ABSTRACT

Most analysis of the economic impacts of sales taxes does not include local taxes, despite the fact that they account for one-fifth of all sales tax collections and vary widely across states. This paper addresses that omission, estimating the effect of sales taxes on employment at state borders using county-level quarterly data and a newly developed data set of local county tax rates. The findings indicate that sales tax increases, relative to cross-border neighbors, lead to losses of employment, as well as payroll and hiring, but these effects are only found in counties with large shares of residents working in another state. The employment effects are also likely to be relatively short-lived, as they occur in the period before tax competition occurs and competitive jurisdictions are able to strategically respond and minimize cross-border tax differentials. Comparing estimates of sales tax effects with and without local county sales taxes show that omitting county taxes does not lead to meaningfully different estimates.

1. Introduction

State borders are commonly used to explore the impacts of taxation, as they represent both sharp differences in rates as well as heightened opportunity for responses of households and firms (Mikesell, 1970; Mikesell, 1971, Holmes, 1998; Rohlin et al., 2014). This is especially true in the case of sales taxes, where inter-state rate differentials might motivate residents to simply cross the border to shop, depriving a state of tax revenues, retail sales, and potentially jobs (Fox, 1986; Hoyt and Harden, 2005). In fact, the rate differentials between neighboring states are large in many cases, with state general sales tax rates ranging from zero (in four states) to 8.25 percent. Each of the continental states without a sales tax borders at least one other state with a rate of 6 percent or higher (Fig. 1). Furthermore, a relatively large empirical literature, recently surveyed by Leal et al. (2010), provides evidence the households are willing to shop across borders if price differences are sufficiently large.

The study of the economic costs of sales taxes is complicated, however, by a number of factors. First, most studies fail to account for the existence of local county sales taxes, which are often substantial and can either offset or exacerbate actual cross-border rate differences. Thompson and Rohlin (2012), for example, finds a strong negative effect of states sales taxes on employment, but does not control for local county rates, which could bias the results.[2] There is reason to think that the bias could be non-trivial, as two thirds of border counties in states with sales taxes have a local county tax, and rates range from 0.5 to 5.0 percent (in 2009). Local taxes account for one fifth of all state and local government sales tax revenue (Census, 2015). Second, sales taxes, particularly local county taxes, could be endogenously determined. Specifically, there is a concern that changes in state and county sales taxes are correlated with the health of the local economy and are therefore endogenous. The bias is ambiguous because it could be that policy makers in healthy local economies feel their areas can withstand a sales tax increase or it could be that distressed areas more likely to raise their sales taxes because of a need of tax revenue generation. Relatedly, there is a strong possibility that states, counties and cities strategically respond to their cross-border neighbors' tax policies. This strategic response, documented in Agrawal (2015) represents another

☆ Thanks to Arthur Kennickel and Cynthia Rogers for comments on an earlier draft, to Don Bruce for allowing us to use his Tennessee local rate data, and to Thomas Krumel, Bruce Pucci and Jonathan Shapiro for assistance in compiling local rates for other states. This is a revised version of a working paper previously titled "The Effect of State and Local Sales Taxes on Employment at State Borders" (Thompson and Rohlin, 2013). ☆☆The analysis and conclusions set forth are those of the authors and do not indicate concurrence by other members of the research staff or the Board of Governors.

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[1] The analysis and conclusions set forth are those of the authors and do not indicate concurrence by other members of the research staff or the Board of Governors.

[2] Our paper improves upon Thompson and Rohlin (2012) in three important ways: we incorporate county sales taxes and examine the degree of omitted variable bias this causes; we explore alternative forms of the dependent variable to provide information about the size of the effects; and we incorporate tax competition analysis between county and state taxes. These additional contributions are explained in more detail later in the paper.

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Fig. 1. State Sales Tax Rates in 2009.

source of potential bias. Finally, it is unlikely that the most commonly-used distance measures, cross-border contiguity, and linear distance, are very good proxies for the economic costs influencing household shopping behavior.

In this study, we attempt to account for these complicating factors, including endogeneity concerns, in estimating the employment effects of cross-border differences in sales taxes. We use county-level data, including a newly developed data set of local tax rates, and estimate the employment response in border counties to changes in state and local county sales taxes.[3] We estimate fixed-effects models on a panel of cross-border county pairs to identify the effect of changes in the general sales tax rate on employment, hiring and payroll. This border methodology should mitigate differences in local economic conditions in the levels (through the fixed effects) and in trends (by using comparison areas that are contiguous are likely to experience similar changes in local economic conditions). Because the fixed-effect approach accounts for time-invariant economic factors, identification concerns are related to time-varying confounders. Our spatial border approach focuses our analysis to contiguous counties and relies on the assumption that changes in local economic conditions vary smoothly across state borders. The identifying assumption is that two neighboring counties experience similar economic growth such that changes in local tax rates are not driven by differences in local economic activity.

We also contribute to the tax competition literature in a number of ways. First, we utilize local sales taxes for the whole United States in addition to state sales taxes, which is not typical in the literature with the exception of recent work by Agrawal (2014, 2016, 2015).[4] Second, instead of using cross-sectional variation in local sales taxes (Agrawal, 2015) we utilize variation in county and state sales taxes over time. Lastly, most of the tax competition literature in local sales taxes studies its effect on other taxes, while we analyze how county sales tax changes affect employment on both sides of the border.

In addition, this study also represents an enhancement of the previous literature on sales tax effects by using the alternative "distance" measures, which we previously introduced in Thompson and Rohlin (2012). Simply being adjacent to another state or being within a certain distance of the border are unsatisfactory measures of the cost of cross-state shopping. Residents of border areas with physical barriers, such as lakes or rivers, or substantial traffic congestion or limited transportation infrastructure, could face high costs despite proximity. We argue that cross-border commuting for work is a better proxy for cost than either distance or proximity. Commuting workers are already making the trip, and it is relatively costless to react to any incentives from a change in sales taxes. We explore differential responses among border counties using the share of residents working in another state.

Our results show that sales tax changes have a detrimental effect on employment, payroll, and hiring in border areas, but that these effects are only present in counties with substantial levels of cross-border commuting. Specifically, among cross-border pairs of counties with the highest levels of inter-state commuting (above 22 percent of employed residents), we find that a 1 percentage point increase in the combined state and local sales tax rate results in a 0.2 to 0.3 percentage point decline in the share of total employment in the county pair. For those high-commuting areas we also find that the share of hiring and the share of payroll decline in counties raising sales taxes relative to their cross-border neighbor. Effects for all counties combined or for counties with lower levels of cross-border commuting are smaller and not statistically different from zero, although the coefficients are usually negative.

Lastly, comparing estimates of sales tax effects with and without county sales taxes show that omitting county taxes does not lead to meaningfully different estimates, at least when measured over a short time period. Although we find supporting evidence of county sales tax competition as Agrawal (2014, 2015) discovered, we find it takes times

[3] Throughout the paper when we refer to "local" taxes we mean county taxes. Indeed, there exists sales taxes at smaller jurisdictions, such as cities and towns, which we did our best to collect. However, it was difficult for us to confirm that we collected all city and township sales taxes adequately and felt it best to not include these types of sales taxes. When we include our collected city and township sales taxes the results did not change but wanted to move forward with the data we feel most confident with.

[4] There is also a set of papers that utilize local sales tax papers focusing on a smaller set of geography (typically a state) such as Luna (2004), Sjoquist et al. (2007), Luna et al. (2007) and Burge and Ihlandfeldt (2009).

for local jurisdictions on opposite sides of state borders to respond and alter their local taxes. Therefore, we find that the lack of county sales tax data may not bias state sales tax analysis if the period of analysis is a few years or less.

The next section describes our empirical approach to estimating the employment effect from sales tax changes, including discussions of how we will address strategic tax policy competition and our treatment of economic distance measures. The following section describes the data used and the sales tax policy changes under study. The next section includes the results from our different specifications. The final section discusses these findings and concludes.

2. Empirical strategy, accounting for economic distance, incorporating strategic tax competition

2.1. Previous work using border methods

Recent studies on the employment effects of sales taxes have relied on border methods, comparing outcomes in adjacent counties across state lines with different sales tax rates.[5] Existing studies have made important contributions to our understanding the effects of sales taxes at state border, but also have short-comings (for a more thorough review of this earlier papers, see Thompson and Rohlin (2012)). Both Fox (1986) and Hoyt and Harden (2005) find that sales tax increases reduce employment relative to cross-border counties. However, their findings are not statistically significant at standard levels, and are sensitive to the particular specifications. Fox (1986) only includes data from a few Tennessee MSAs and their cross-border counterparts in Kentucky, Georgia, and Virginia raising concerns of external validity.[6] Hoyt and Harden (2005) use county-level panel data from all 50 states, but their focus on cross-border MSAs effectively excludes most counties. Thompson and Rohlin (2012) uses quarterly county-level data from all states, but does not include any variation in local taxes, suggesting potential bias in statistically significant negative employment impacts linked to cross-border differences in state sales taxes.[7]

This paper extends Thompson and Rohlin (2012) in several ways. In addition to incorporating potentially important variation in county-level sales taxes, this paper improves on the construction of the dependent variable, which likely led to an overstatement of the employment effects. To the extent that sales taxes reallocate shopping across borders, but keep economic activity within the cross-border pair, the estimates in Thompson and Rohlin (2012) are as much as twice as large as the actual effect on employment in counties raising rates. The findings in this paper are not entirely free of that concern, but some alternative specifications are used to explore the extent of cross-border reallocation and discussion of the findings makes clear that the measured effects represent an upper-bound to the employment effects. This paper also includes an investigation into the tax competition dynamics that can help explain our findings.[8]

2.2. Border method augmented with measures of economic distance

This paper uses county-level employment data as well as newly collected data on local county sales tax rates – quarterly rates for all counties between 2004 and 2009 – to study the employment effects of sales taxes at state borders. We explore the differential effect of sales tax increases by the extent of economic contact between county pairs, proxied by the share of county residents who work in another state.

A basic assumption shared by the previous research using border methods to study employment effects of sales taxed is that 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 the sales tax differences on shopping and employment is expected to dissipate as you go from the border to the interior 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. "Spillovers" caused by policy changes on one side of a border, causing employment to rise on the other side of the border, are a complication for identification, but do so in a way that systematically overstates the magnitude of the effects. We discuss spillover in the case of sales taxes at state borders, and how it influences the interpretation of our results, later in the results section (Table 6).

Fig. 2 is a county map of the United States that highlights counties on the state border (shaded in dark gray), and interior counties that are not on the border (shaded in white).[9] Using only the border counties, we calculate the difference in employment and sales tax rates for each cross-border county pair, the employment share for each county in the pair, and include county pair fixed effects in the regressions. 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, which is a strong assumption. This assumption potentially less strong, however, 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 factors, save for policy differences between the states. Similar to Rohlin, Rosenthal, and Ross (2014), we initially use these cross-border differences as the dependent variable and the independent variables of interest and estimate:

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

The "sales tax" is the statutory general sales tax rate, so *Diff_SalesTax* is the difference in sales tax between the two jurisdictions and $Diff\_X_{it}$ is a vector that in some cases includes a measure of personal income. These differenced specifications include year and quarter fixed effects ($\delta_{t1,2}$) as well as county-pair-level fixed effects (γ_i).[10] Effectively, the key coefficient (β_1) reflects differences from the over-time average for the county-pair. In all specifications we use robust

[5] The importance of examining business activity in close geography is highlighted in recent papers Azraghi and Henderson (2008); Rosenthal and Strange (2008), and Duranton et al. (2011)).

[6] 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, $County_j$.

[7] 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. 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. 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. This is consistent with the finding that taxes are fully shifted onto consumers, and is a standard assumption in incidence analysis. In Thompson and Rohlin (2012) and in this paper we follow Walsh and Jones (1988) and effectively assume input costs are equal on both sides of the cross-state border. To the extent, however, that pre-tax prices are not equal, our estimates will be biased. If some portion of the sales tax is borne by producers, then our findings will understate the employment response to the tax change. If sales tax changes are "over-shifted" onto consumers, our findings will overstate the employment response.

[8] Also, this paper uses only border counties, while Thompson and Rohlin (2012) also explored some regressions using interior counties as well. In addition, this paper excludes all Louisiana counties.

[9] Border counties with more than one cross-border neighbor will appear in multiple cross-border pairs. The findings are robust to including or excluding these observations.

[10] Regressions are estimated in STATA, using xtreg, fe.

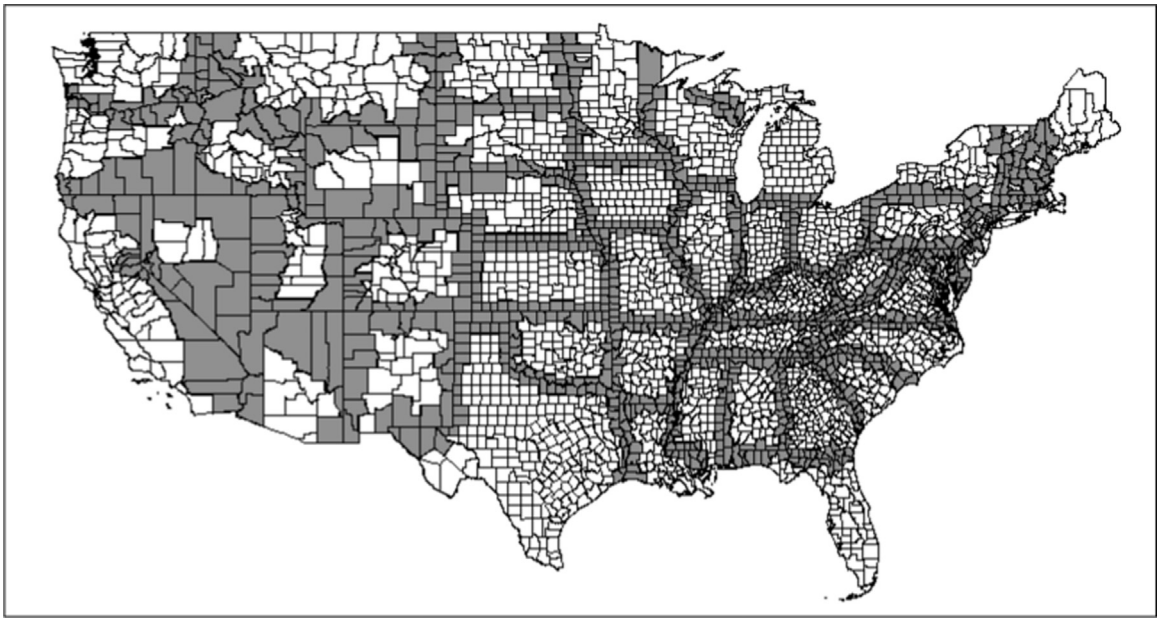

Fig. 2. U.S. County Map showing border counties.

standard errors to allow for unknown forms of heteroskedasticity. We also cluster standard errors at the state-level to allow for an arbitrary variance-covariance structure within each state.[11] The regressions are also weighted by the square root of the combined total population of the county pair.[12]

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:

$$EMP_{ijt} = \ln(EMPLOYMENT_{it}) - \ln(EMPLOYMENT_{jt}) \quad (2)$$

In some specifications, however, we use the county share of employment in the cross-border pair as the dependent variable:

$$EMP_{ijt} = \frac{EMPLOYMENT_{it}}{EMPLOYMENT_{it} + EMPLOYMENT_{jt}} \quad (3)$$

To the extent that some of the jobs lost to one county are gained by other counties – as shopping relocates across the state border – using either the differenced version of the dependent variable or the share version will overstate the employment effect of sales tax changes, giving us an upper bound of those effects. Regressions using either dependent variable produce equivalent results, but the share version makes clear, for exposition purposes, that we are measuring changes in the share of employment, not necessarily lost employment for the county or, for that matter, the cross-border pair.

This paper uses a border method that is similar to some previous research, but cross-border county pairs 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 (as in Hoyt and Harden (2005)). In some cases, though, travelling between counties within an MSA is time consuming (congestion, limited public transportation) and costly (tolls, gas, and parking). The potential after-tax cost savings is the factor motivating 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 border counties in our data, the share working in another state also ranges from 0 to 66, 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 percent to 56 percent, with a mean of 12 percent.[13]

Residents crossing the border to work have already taken on the cost of getting to the other state, so 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 commuting first by including interactions between the sales tax variable and the share of cross-state commuters, and then by separately estimating (1) for high and low cross-state employment groups. Breaking the number of cross-border pairs roughly into thirds, we estimate (1) for pairs with less than 11 percent (combined) working in another state, from 11 percent up to 22 percent, and 22 percent or higher.

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 the additional variables in the QWI to explore each of these additional outcomes.

2.3. Exploring and accounting for strategic tax competition

There is a large sales tax literature that investigates the impact of sales tax changes on a number of outcomes. However, most papers using sales tax rates either only use easier to obtain state tax rates or use both county and state sales tax rates for a small geographic area (typically a single state) due to the difficulty of obtaining local county

[11] Differing levels of clustering doesn't substantively change the significance of our estimates.

[12] Results from unweighted regressions are not shown in this paper, but the coefficient magnitudes and the statistical significance for most regressions, as well as the overall pattern of results, are not dependent on the use of weights. Results from the unweighted regressions are available on request from the authors.

[13] 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.

sales tax rates.[14] One question in this literature is whether omitting county sales taxes biases estimates of sales tax effects. If local jurisdictions are immediately responding to neighboring state sales tax changes by increasing their local taxes then the difference in combined (state and county) sales taxes are smaller than the state sales tax difference and researchers with local county sales tax data could be underestimating the true effect of the sales tax change. However, if there is sufficient lag time for local jurisdictions to respond to cross border state sales tax changes then state sales tax estimates could be unaffected by tax competition if their time frame is shorter than the time it takes to the local jurisdictions to respond. We investigate this issue by presenting some correlations between sales tax changes and cross-border local county sales tax changes over time as well as directly estimate results of sales tax effects on employment with and without local county sales tax measures.

3. Local county sales tax rate and employment data

3.1. Changes in state and county sales tax rates

Between 2004 and 2009 there were 20 general sales tax changes in 16 states (see Table 1). The average cumulative point change in these states was 1.0 percent, 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.

State 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 like 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.

Unlike most previous analysis, this study considers combined state and county rates for all states. The previous studies that have included local rates have either been cross-sectional (Agrawal,2015), or focused on single states or regions (Burge and Rogers, 2017; Fox, 1986; Luna et al., 2007; Walsh and Jones, 1988). We have data for 3003 counties, although we focus on the 1092 counties on a state border. Nearly all of the border counties are in states with sales taxes (1046) and sixty percent (634) of those counties also have county sales taxes.[15] The local taxes we collect are at the county-level for the whole county, and do not include city-specific taxes, although this distinction is not always clear in the statistical reports made available by state tax and revenue offices. These local rates are also intended to reflect taxes collected at the local level, whether or not the local jurisdiction determines those rates.

County sales taxes are very common in states with sales taxes, but the rates are typically quite low. In the 1790 counties with county taxes (2009 Q2) the rate ranged from 0.5 to 5.0 percent, averaging just 1.18 percent (Table 2). Between 2003 (Q2) and 2009 (Q2), more than one fourth of the counties with a local tax changed their local rate. Changes in local rates ranged from −1 to +2 percent, averaging .09.

The sales tax rate we use in this paper is the combined state and county general sales tax rate. This is the same as in most other studies (Fox, 1986; Walsh and Jones, 1988). The sales tax rate used in Hoyt and Harden (2005), though, is the effective sales tax rate which divides sales tax revenue by personal income.[16] 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

Table 1
States changing their general sales tax rates in 2004–2009.
Source: Tax Foundation

	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
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

Table 2
Local Sales Tax Rates (in Q2).
Source: County-level sales taxes collected by Thompson and Rohlin (with research assistance from Thomas Krumel) from state-level revenue and tax agency documents and reports.

	2003			2006			2009		
	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max
AL	1.89	0	4	1.93	0	5	2.03	0	5
AR	1.26	0	3	1.37	0	3.75	1.44	0	3
AZ	0.68	0	1.5	0.73	0.25	1.125	0.77	0.25	1.5
CA	1.45	1.25	2.5	1.25	1	2.5	1.30	1	2.5
CO	1.43	0	5	1.44	0	5	1.45	0	5
FL	0.78	0	1.5	0.81	0	1.5	0.85	0	1.5
GA	2.77	1	3	2.89	2	3	2.94	2	3
IA	0.78	0	1	0.90	0	1	0.99	0	1
IL	0.14	0	1.75	0.22	0	1.75	0.30	0	2.75
KS	0.74	0	2	0.84	0	2	0.89	0	2.25
MN	0.01	0	1	0.01	0	1	0.02	0	0.4
MO	1.23	0.5	2.5	1.38	0.5	2.5	1.51	0.5	3
NC	2.45	2	2.5	2.50	2.5	2.5	2.52	2.5	2.75
ND	0.00	0	0.25	0.02	0	1	0.03	0	1
NE	0.00	0	0	0.01	0	0.5	0.01	0	0.5
NM	0.71	0.125	1.4375	0.92	0.125	1.6875	1.05	0.375	2.0625
NV	0.26	0	0.75	0.36	0	1.25	0.38	0	1.25
NY	3.56	2.5	4.25	3.89	3	5.5	3.91	3	4.75
OH	1.12	0	1.5	1.20	0.25	1.5	1.25	0.5	1.5
OK	0.88	0	2	0.99	0	2	1.12	0	2
PA	0.03	0	1	0.03	0	1	0.03	0	1
SC	0.71	0	1	0.71	0	1	0.71	0	1
SD	1.60	0	2	1.60	0	2	1.60	0	2
TN	2.45	1.5	2.75	2.46	1.5	2.75	2.48	1.5	2.75
TX	0.24	0	1.5	0.24	0	1.5	0.24	0	1.5
UT	1.00	1	1	1.00	1	1	1.00	1	1
VA	1.00	1	1	1.00	1	1	1.00	1	1
WA	1.22	0.5	2	1.27	0.5	2.1	1.41	0.5	3
WI	0.40	0	0.5	0.40	0	0.5	0.42	0	0.5
WY	1.21	0	2	1.23	0	2	1.27	0	2
Total	1.09	0	5	1.14	0	5.5	1.18	0	5

[14] Recent work by Agrawal (2014, 2016, 2015) does utilize local sales tax but focuses on how tax changes affect neighboring or own jurisdictions tax rates.

[15] These data were gathered by the authors and research assistant Thomas Krumel and Jonathon Shapiro over the internet from state tax and revenue office statistical reports. The data for Tennessee were provided by Don Bruce.

[16] Annual State-level sales tax revenue is from the Census Bureau's Survey of State Government Finances, while the county level sales tax figures are produced every five years in the Census of Governments. The county level annual collections are estimated by Hoyt and Harden (2005) by smoothing the data over the intervening years.

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 may have occurred. We use the actual sales tax rate in order to avoid some of these concerns.

3.2. 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 at the US 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; Massachusetts is the only state not included in the most recent data. Over the 2004 to 2009 period, we have quarterly data for forty-seven continental states and the District of Columbia.[17] We exclude Louisiana, and all of the cross-border county pairs that include Louisiana counties, from the analysis due to the timing of Hurricane Katrina, which hit in August 2005, in the middle of the period we are analyzing. The 46 continental states that are included in the QWI over the full range of years that we study contain 3003 counties. The regressions include as many as 1233 pairs of cross-border neighbor counties.[18]

The QWI data include counts and means of quarterly employment and earnings information by county, ownership status, and broad-industry group for all workers in all establishments covered by UI in those states.[19] Because the data are based on Unemployment Insurance wage records, results even for most individual small counties are available and reliable, whereas they would not be in a standard survey.[20] 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 several variables in the QWI that are not present in other data sets that can be explored as possible responses to the sales tax: hiring decisions and payroll, which reflect joint changes in employment as well as hours. 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.[21]

4. Results

We begin by presenting results using the combined state and county rate with two versions of the dependent variable: the difference in employment across county pairs and the county share of employment in the cross-border pair (employment share). Throughout the paper we highlight the differential impact of sales taxes by the extent of cross-state commuting in the county pair. We then examine whether results are biased when researchers omit county sales taxes. Next, we show how the employment impacts are influenced by the inclusion of an additional variable reflecting county-level personal income, and compare the results from several different approaches to parameterizing the employment dependent variable before settling on the employment share. The section concludes by presenting impacts on payroll and hiring.

4.1. Baseline results by extent of working out of state

We begin by examining how combined county and state sales taxes affect employment in panel A of Table 3 using the difference in employment levels across counties as the dependent variable. Results indicate that counties in states that raise the sales tax rate by one percentage point experience an employment fall of 1.11 percent (significant at the 5 percent level) relative to their cross-border neighbors (Column 1). The remaining results in Panel A suggest, however, that the employment effects from a combined sales tax increase are isolated to those county pairs with relatively high levels of cross-border commuting. When we include an interaction between a continuous measure of the commuting share (the percent of employed county residents who work in another state) and the sales tax difference measure, we find that the interaction term is negative and significant (at the 10% level), while the main effect is small and not statistically significant (Column 2). The implication that the employment effects are larger for, and only statistically significant in, counties with higher levels of cross-border commuting is supported in specifications that use a discrete interaction term for “low” (less than 11 percent), “medium” (from 11 up to 22 percent) or “high” (22 percent and higher) level of cross-state commuting (Column 3) as well as specifications which estimate (1) separately for each group.[22] The results on the separately estimated specifications indicate that a one percent increase in a state sales tax rate reduces border county employment by 1.97 percent relative to cross-border neighbors in high-level commuting counties but has no effect in county pairs with lower levels of cross-border commuting.[23]

Panel B of Table 3 uses an alternative form (the share of employment) of the dependent variable and finds a similar story but with smaller magnitudes. Column 1 suggests that a state increasing their combined sales taxes by one percentage point experiences a rather small loss of 0.192 percent (statistically significant at the 1 percent level). Results separately estimating effects based on the levels of cross-border commuting show that this loss in employment share is driven by those counties with the highest level of cross-border commuting to work.

4.2. How does omitting county sales tax rates affect estimates of employment impacts?

Most nation-wide papers that study sales tax effects on different outcomes utilize state tax rates and omit county tax rates because of the extreme labor-intensiveness of collecting all local rates across the country. To better understand what effect omitting county tax rates

[17] Data for the District of Columbia were first brought into the QWI system in early 2012, with data reaching back to the second quarter of 2005.

[18] None of the counties bordering Massachusetts can be used, and all of the county-pairs including Louisiana are also dropped.

[19] 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 web-site 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.

[20] 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) 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.

[21] In regressions reported below we use the second quarter, but the results do not depend on the choice of quarter.

[22] These categories split the sample of county pairs into three roughly equal sized groups.

[23] These results for employment effects from state sales taxes are considerably lower than what was previously reported in Thompson and Rohlin (2012). The differences are due to a number of factors. Counties from Louisiana, not included here due to concerns over the impact of Hurricane Katrina, were previously included in Thompson and Rohlin (2012). Data for Washington DC are include here, but were not included in Thompson and Rohlin (2012) since they were not available at the time the files were constructed for analysis. Also, a coding error in the program merging the personal income covariate into the QWI data produced an error which inadvertently resulted in larger coefficients in Thompson and Rohlin (2012).

Table 3
County Pair Employment Regressions using Combined Rate by Extent of Cross-border Commuting.

				Cross-State Work Share		
				< 11%	11–22%	≥ 22%
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Employment difference - combined rate						
Sales Tax Rate	−0.0111 (0.00534)**	0.00353 (0.0104)	0.00359 (0.0118)	0.00321 (0.0111)	−0.00918 (0.00706)	−0.0197 (0.00623)***
Sales Tax * Commute Share		−0.0613 (0.0316)*				
Sale Tax * Mid-level Commute			−0.0123 (0.0122)			
Sales Tax * High level Commute			−0.0248 (0.0119)**			
Panel B. Employment share - combined rate						
Sales Tax Rate	−0.00192 (0.000688)***	−0.000185 (0.00150)	−0.000328 (0.00160)	−0.000430 (0.00150)	−0.00173 (0.00134)	−0.00277 (0.000748)***
Sales Tax * Commute Share		−0.00729 (0.00454)				
Sale Tax * Mid-level Commute			−0.00138 (0.00197)			
Sales Tax * High level Commute			−0.00265 (0.00188)			
# County Pairs	1233	1233	1233	418	416	399

Clustered standard errors in parentheses.
* p < 0.1
** p < 0.05,
*** p < 0.01,

Table 4
County Pair Employment Regressions using Only State Rates by Extent of Cross-border Commuting.

				Cross-State Work Share		
				< 11%	11–22%	≥ 22%
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Employment difference - state rate						
Sales Tax Rate	−0.0135 (0.00541)**	0.00284 (0.0107)	0.00657 (0.0138)	0.00549 (0.0128)	−0.0132 (0.00603) **	−0.0227 (0.00616)***
Sales Tax * Commute Share		−0.0669 (0.0323)**				
Sale Tax * Mid-level Commute			−0.0192 (0.0135)			
Sales Tax * High level Commute			−0.0309 (0.0133)**			
Panel B. Employment share - state rate						
Sales Tax Rate	−0.00231 (0.000694)***	−0.00119 (0.00122)	−0.00145 (0.00123)	0.000240 (0.00193)	−0.00265 (0.00119)**	−0.00324 (0.000779)***
Sales Tax * Commute Share		−0.00477 (0.00342)				
Sale Tax * Mid-level Commute			−0.000467 (0.00166)			
Sales Tax * High level Commute			−0.00166 (0.00137)			
# County Pairs	1233	1233	1233	418	416	399

Clustered standard errors in parentheses.
* p < 0.1.
** p < 0.05.
*** p < 0.01.

have on estimates of sales taxes Table 4 displays the same set of regressions as Table 3 using only state tax rates. Comparing the results in the two tables estimates only using state rate variation are roughly the same (only slightly larger in general). Column 1 of Panel A finds a one percentage point increase in state sales taxes leads to an employment reduction of 1.35 percent while the combined tax rate effect in Table 3 as a loss of 1.11 percent (both statistically significant at the 5 percent level). Stratifying counties based on their cross-state worker share illustrates the same pattern that this negative effect of sales taxes is driven largely by counties with the greatest cross-border commuting. In columns 6 of Tables 3 and 4 show an employment loss of 2.27 percent for the state rate estimates and a loss of 1.97 percent including the county tax rates. Panels B of Tables 3 and 4 show that changing the dependent variable to employment shares as in Eq. (3) still produce similar estimates regardless of the inclusion of the county sales tax rates.

Although finding that the omission of county taxes does not dramatically bias sales tax employment effect estimates is encouraging to the previous literature, it may seem counter-intuitive in light of recent evidence of tax competition between bordering jurisdictions particularly at the border (Agrawal, 2016, 2015). To investigate this further Table 5 presents some simple correlations to shed some light on how tax competition within and across state borders could affect estimates of sales tax effects on employment.

Panel A shows the correlation between a county's local tax rate and its own state rate in levels on the left and changes over time on the right. In the cross-section, the correlation between a county sales tax and its own state sales tax is negative. This means that states with large state sales taxes tend to have smaller county sales tax rates at a given point in time. So cross-sectional papers with only state tax rates may be overstating the tax rate differences that households and firms face and understating the true sales tax effect because borders with big state tax rate differences may have smaller combined tax rate changes. These patterns are confirmed when we look at tax changes over time. We find a negative correlation between changes in county rates and changes in a county's own state sales tax, but the magnitudes are considerably

Table 5
Local sales tax rate correlation coefficients cross section and panel (Over Time).

A. Correlations between own local tax rate and own state tax rate			
Cross-Section	Correlation	Panel (Tax Rate Changes)	Correlation
Average for All Years	−0.177	All Years - Demeaned from Within- County, Over-Time Average	−0.001
2006 Q2	−0.157	2006 Q2–2009 Q2	−0.024
2009 Q2	−0.200	2004 Q2–2009 Q2	−0.120
B. Correlations between own local tax rate and opposite combined tax Rate			
Cross-Section	Correlation	Panel (Tax Rate Changes)	Correlation
Average for All Years	0.103	All Years - Demeaned from Within-County, Over-Time Average	0.027
2006 Q2	0.100	2006 Q2–2009 Q2	0.003
2009 Q2	0.099	2004 Q2–2009 Q2	0.040

smaller. Only the change measured over the longest period in our data results in a correlation anywhere near as large as what we see in the cross section.

Panel B of Table 5 displays simple correlation coefficients between a county's local sales tax rate and the combined county and state rate in the cross-border county. We find that correlations are positive in the cross-section, which means that local jurisdictions are more likely to have higher county sales taxes when the combined sales tax across the border in the opposing state is higher. We also find positive correlations between changes in county sales tax rates and changes in the cross-border combined sales tax rate, further supporting the recent literature that tax competition existing along state borders. However, note that the correlation between changes over 3 years (2006 to 2009) were very close to zero while sales tax rate changes over 5 years (2004 to 2009) had a stronger positive correlation. This suggests that papers studying the effect of changes in sales taxes on employment over a shorter time period are less affected by changes in county sales tax changes due to tax competition.

4.3. Including personal income

Personal income is part of the local demand function (1), but is not available quarterly at the county (or state) level, so has not been included in the specifications presented in Table 3. We can include county-level personal income as a covariate if we include only one quarter from each year. Panel A in Table 6 contains results from specifications using the employment difference dependent variable using all quarters without the income covariate (columns 1 through 4) alongside the results from specifications using only the second quarter and including personal income (columns 5 through 8). In all of these specifications the coefficient on income is positive and highly significant. The sales tax coefficients in those specifications are quite similar to what we see in columns 1 through 4. The signs on the sales tax coefficients are negative, and the magnitude is somewhat larger for counties with more cross-state commuting. After including the personal income covariate, the combined sales tax coefficient for high-level commuting areas rises from −1.97 percent to −2.22 percent (Panel A, Columns 4 and 8).

4.4. Interpreting the employment impacts in the presence of spillover effects

If employment shifts across state borders in response to sales tax increases – following the flow of shopping dollars as they shift from the high-tax side to the low-tax side – then calculating the employment effects using cross-border differences will overstate the employment effects. Without knowing the extent of the shopping spillover, we do not know how much our specifications overstate the employment impacts.

Table 6
Employment Regressions Including Personal Income Covariate and Assessing Cross-border Employment Spillover Using Different Employment Dependent Variables.

	Full quarterly data				Single quarter data			
	Cross-State Work Share				Cross-State Work Share			
	(1)	< 11% (2)	11 to 22% (3)	≥ 22% (4)	(5)	< 11% (6)	11–22% (7)	≥ 22% (8)
Panel A. Employment difference								
Combined Sale Tax	−0.0111 (0.00534)**	0.00321 (0.0111)	−0.00918 (0.00706)	−0.0197 (0.00623)***	−0.0103 (0.00671)	0.00458 (0.0128)	−0.00400 (0.00659)	−0.0222 (0.00906)**
Personal Income					0.483 (0.0716) ***	0.407 (0.131)***	0.514 (0.0788)***	0.573 (0.0796)***
Panel B. County employment								
Combined Sale Tax	−0.00694 (0.00578)	−0.00395 (0.0106)	−0.00355 (0.00625)	−0.0115 (0.00685)	−0.00789 (0.00731)	−0.00373 (0.0125)	−0.00309 (0.00606)	−0.0136 (0.00897)
Personal Income					0.314 (0.0506)***	0.338 (0.0830)***	0.271 (0.0798)***	0.330 (0.0712)***
Panel C. County pair employment total								
Combined Sale Tax	0.00125 (0.00397)	−0.00287 (0.00803)	0.00552 (0.00479)	−0.000654 (0.00434)	0.000779 (0.00506)	−0.00207 (0.00977)	0.00281 (0.00564)	1.10e-05 (0.00446)
Personal Income					0.0638 (0.0483)	0.146 (0.0806)*	0.00507 (0.0704)	0.00495 (0.0569)
Panel D. Employment share								
Combined Sale Tax	−0.00192 (0.000688)***	−0.000430 (0.00150)	−0.00173 (0.00134)	−0.00277 (0.000748)***	−0.00220 (0.000807)***	−0.00100 (0.00163)	−0.000814 (0.00111)	−0.00355 (0.00104)***
Personal Income					0.0767 (0.0125)***	0.0584 (0.0185)***	0.0937 (0.0141)***	0.0884 (0.0103)***
# County Pairs	1233	418	416	399	1233	418	416	399

Clustered standard errors in parentheses.
* p < 0.1
** p < 0.05.
*** p < 0.01.

With no cross-border employment spillover in response to a sales tax change on one side of the border we would expect specifications using the employment difference dependent variable to produce the same results as specifications using only the county-level employment ($\ln(EMPLOYMENT_{it})$) as the dependent variable in (1). If all of the measured employment difference is due to cross-state spillovers, we would expect the specifications using the county employment level dependent variable to result in coefficients as little as one half the magnitudes of coefficients using the differenced dependent variable. Panel B includes the sales tax coefficients from specifications similar those from in Panel A, but instead using county employment level as the dependent variable. The coefficients from employment regressions for high-level commuting area – the only specifications that are consistently different from zero statistically – are two thirds the size of those using the differenced dependent variable, consistent with a substantial amount of spillover. If measured employment losses are due solely due to spillover, we would also expect specifications using the combined employment of the cross-border pair ($\ln(EMPLOYMENT_{it} + EMPLOYMENT_{jt})$) as the dependent variable in (1) to find zero employment effect. The coefficients from specifications using combined employment level as the dependent variable (Panel C) are very small and not significantly different from zero.

Given that the results from these regressions using different ways of characterizing the employment variable are consistent with very high levels of cross-border employment spillovers, the employment effects we measure are best viewed as indicating changes in the employment share within the cross-border pair of counties. For the remainder of the paper, we use the employment share dependent variable for purposes of exposition. As expected, specifications using the employment share dependent variable yield similar results. Each of the sales tax coefficients is negative (Panel D), but they are larger and only statistically different from zero in county pairs with higher levels of cross-state commuting. In the highest commuting areas, the employment share declines .35 percent, when the personal income covariate is included (Column 8).

4.5. Considering the impacts on payroll and hiring

In addition to employment, both payroll and hiring are important to local policymakers, making them potentially important outcomes in their own right. Payroll and hiring also represent other means of detecting the impact of general of sales tax changes on economic activity. Panels B and C in Table 7 show results from specifications using share of payroll and share of hiring, respectively, as the dependent variable. In both cases, the coefficients tend to be negative, with larger magnitudes in counties with more cross-border commuting. Results from the specifications including personal income (Columns 6 through 10) indicate that a one-point increase in the combined state and county sales tax rate lowers the county share of payroll by 0.316 percent and the share of hiring by 0.685 percent (Column 10 in Panels B and C).

For each potential outcome (employment, payroll, and hiring) the coefficient for higher commuting areas is larger in the specifications including personal income. In the specifications without personal income the employment effects for mid-level commuting areas statistically significant and as large, or larger, than those from the highest commuting areas. Sales tax coefficients from the lowest commuting areas (Columns 3 and 8), however, are always the lowest and never statistically different from zero.

5. Discussion and conclusion

This paper examines the effects of increases in state and county sales taxes on employment (and payroll and hiring) in state border areas. Combined sales tax rates appear to influence employment, payroll, and hiring, but those effects are concentrated in counties with relatively high levels of cross-state commuting. One key result highlighted in the analysis is that for county pairs with the highest levels of cross-state commuting among the workforce – with 22 percent or more of employed residents traveling to another state for work – the county share of employment declines 0.34 percentage points following a one point increase in the combined state and county sales tax rate. The construction of the employment share dependent variable in the presence of employment spillovers following shifts in cross-border shopping suggests that these effects represent an upper bound on the actual employment decline an individual county will face. Regressions exploring the effects using several alternative dependent variables indicate that spillovers are likely, and that the effects on county employment will be between two thirds and one half as large as our results suggest.

Despite the concern implicitly raised by Agrawal's (2015) research on the strategic response by border-area local governments to state-level sales taxes – where local policy changes work to diminish cross-state differences – our results suggest that using only state-level rates does not impart a downward bias to our estimates. When we include local county rates, the employment effects actually rise modestly. Our state and county sales tax rate data do confirm the presence of the relationship described in Agrawal (2015). In the cross-section we observe a negative correlation between local county rates and the own-state rate, and a positive correlation between the county rate and the combined rate in the cross-border neighbor (Table 5). The correlation coefficients in our rates for 2009 Q2, for example, are −0.2 and +0.1, respectively.

When we look to changes over time, however, these correlations are absent across the spans of time we explore in this paper. The correlation coefficient between 2006 Q2 to 2009 Q2 rate changes in rates is just −.024 between the county rate (of border counties) and the own-sate rate, and only 0.003 between the county rate and the combined rate of the cross-border neighbor (Panel B). Since the regressions in this paper analyze over time changes using county pair fixed effects, we are differencing from any given quarter and the over-time average within that county pair. Correlation coefficients between rate changes constructed to be equivalent to that over-time differencing are nearly as small as those looking at the 2006 to 2009 change.

The only case where we find correlations between rate changes that are consistent with the mechanism described by Agrawal (2015) is when we measure correlations between changes over the longest period available in our rate data (2003 to 2009). Measured over six years we do observe coefficients indicating that changes in county rates are negatively correlated with changes in the own-state rate (−0.12) and are positively correlated with changes in the combined rate of the cross-border neighbor (.04), with magnitudes roughly half as large as what we observe in the cross-section. The employment effects we measure in this paper apparently occur over the relatively short-term, shifting across the state border along with shopping, before local governments respond. Because they ultimately do respond, however, it is likely that the effects we measure are not permanent features, but will instead be at least partially counteracted by offsetting policy changes on the other side of the border.

In the near term, though, the primary objective for state and local governments in raising sales tax rates – to generate additional revenue to finance basic public services – will be achieved, though with some leakages due to increased cross-border shopping. The ability of state and local governments to raise tax revenues in the near term is particularly important during periods of economic distress. Those public services, including public safety and education services, are generally valued by residents, but the employment effects we identify in this paper make clear that those revenue increases come at an economic cost for a state's border region. These costs, however, are primarily limited to geographic shifts in employment, hiring and payroll within the broader region with little net reduction for the combined region. The extent to which residents and policy makers

Table 7
All Industry regressions using alternative dependent variables, combined sales tax rates, by frequency of data and extent of cross-border commuting.

	Full quarterly data					Single quarter data				
	Cross-State Work Share					Cross-State Work Share				
			< 11%	11– 22%	≥ 22%			< 11%	11–22%	≥ 22%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Employment Share										
Sales Tax Rate	−0.00192	−0.000185	−0.000430	−0.00173	−0.00277	−0.00220	−0.000289	−0.00100	−0.000814	−0.00355
	(0.000688)***	(0.00150)	(0.00150)	(0.00134)	(0.000748)***	(0.000807)***	(0.00143)	(0.00163)	(0.00111)	(0.00104)***
Sales Tax *		−0.00729					−0.00803			
Commute Share		(0.00454)					(0.00432)*			
Personal Income						0.0767 (0.0125)***	0.0765 (0.0124)***	0.0584	0.0937 (0.0141)***	0.0884 (0.0103)***
								(0.0185)***		
Panel B. Payroll Share										
Sales Tax Rate	−0.00153	0.000546	0.00111	−0.00185	−0.00281	−0.00163	0.000266	0.000301	−0.000616	−0.00316
	(0.000789)*	(0.00157)	(0.00168)	(0.00150)	(0.000866)***	(0.000820)*	(0.00121)	(0.00130)	(0.00132)	(0.00107)***
Sales Tax *		−0.00873					−0.00801			
Commute Share		(0.00509)*					(0.00404)*			
Personal Income						0.0946 (0.0156)***	0.0944 (0.0155)***	0.0740	0.115 (0.0210)***	0.105 (0.0165)***
								(0.0226)***		
Panel C. Hiring Share										
Sales Tax Rate	−0.00333	−0.00148	−0.000930	−0.00435	−0.00340 (0.00175)*	−0.00356 (0.00207)*	0.00134	−0.00114	−0.000656	−0.00685
	(0.00149)**	(0.00250)	(0.00286)	(0.00193)**			(0.00360)	(0.00444)	(0.00206)	(0.00199)***
Sales Tax *		−0.00775					−0.0207			
Commute Share		(0.00630)					(0.00869)**			
Personal Income						0.0727 (0.0174)***	0.0724 (0.0172)***	0.0578	0.0846 (0.0295)***	0.0860 (0.0170)***
								(0.0254)**		
# County Pairs	1233	1233	418	416	399	1233	1233	418	416	399

Clustered standard errors in parentheses.
* p < 0.1.
** p < 0.05.
*** p < 0.01.

value own-state (or county) economic opportunities relative to those in the broader region will influence perceptions of the tradeoff between revenue and services on the one hand and taxes and economic costs on the other.

This paper extends the literature by gathering and incorporating county-level sales tax rates into the analysis. Counties are the predominant source of local sales taxes, but many cities also have rates, and that variation could have still further implications for the measured employment effects of sales taxes at state borders. Future work in this area will be directed toward collecting over-time changes in those city-level taxes.

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