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Fiscal Zoning and Sales Taxes: Do Higher Sales Taxes Lead to More Retailing and Less Manufacturing?
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ABSTRACT

We test the hypothesis that local government officials in jurisdictions that have higher local sales taxes are more likely to use fiscal zoning to attract retailing. We find that total retail employment is not significantly affected by local sales tax rates, but employment in big box and anchor stores is significantly increased in jurisdictions where sales tax rates increase. We also find that manufacturing employment is significantly lowered in these jurisdictions. These results suggest that local officials in jurisdictions with higher sales tax rates concentrate on attracting large stores and shopping centers and that their efforts crowd out manufacturing. A rise of one percentage point in a county-level local sales tax rate is predicted to result in 258 additional retail jobs and the loss of 838 manufacturing jobs.

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I. Introduction

Many U.S. states allow local governments to levy sales taxes that add to the state sales tax and to keep some or all of the revenue (Lewis, 2001). These extra sales taxes, which have the same base as the state sales tax, give local government officials an incentive to encourage retailing, since retailing generates more sales tax revenue than other land uses. Correspondingly, these taxes also give local government officials an incentive to discourage other land uses, since they generate less sales tax revenue and could crowd out retailing.

Local government officials have various policy instruments and practices that they can use to encourage retailing: they can zone additional land for retail use, they can allow retail developments to have higher density levels, and they can reduce the often-formidable set of approvals and inspections that are required for construction or renovation. They can use all of these instruments and practices in reverse to discourage other land uses. We use the term "fiscal zoning" to refer to local government officials' efforts to encourage land uses that generate high tax revenue—which in this case we interpret as high sales tax revenue. In this paper, we first develop a model of fiscal zoning that predicts that local government officials are more likely to encourage retailing when the sales tax rate in their jurisdiction is higher. We then test this prediction empirically by examining whether retail employment increases in response to higher sales tax rates.

Manufacturing may compete with large stores and shopping centers for land, because both occupy large tracts of flat land, often close to highways. In addition, manufacturing generates less sales tax revenue than retailing, because all sales by retail stores are subject to sales taxes, while most sales by manufacturing firms are exempt.[1] As a result, local officials' efforts to attract retailing in order to increase sales tax revenue may come at the cost of discouraging manufacturing—an important policy issue since jobs in manufacturing are often better-paid than jobs in retailing (Leroy, 2005).

Our empirical analysis uses data from Florida, which is a useful "laboratory" for analysis because it had a constant state sales tax over our sample period (1992-2006), but also allowed county

[1] See below for data and further discussion.

governments to levy additional sales taxes on the same base—these vary considerably both cross-sectionally and over time. Because we study one state, and because we use panel data models that include year and county fixed effects, we are able to control for fixed differences across counties in local taxation and also for changes over time that are common to all counties. We also take explicit account of other local programs that cause variation in local taxes, including Enterprise Zones and Tax Increment Financing in redevelopment areas (see discussion below).

We have three main findings. First, total retail employment is *not* significantly affected by variation in local sales tax rates. This may be because local government officials do not use fiscal zoning, but it alternately may occur because the positive effect of fiscal zoning on retail employment is fully offset by the dampening effect of higher sales tax rates on consumer demand.[2] Second, and more consistent with the fiscal zoning hypothesis, we find that higher local sales tax rates lead to more employment in "big box" stores and department stores that anchor shopping malls. This result suggests that local officials concentrate their fiscal zoning efforts on attracting large stores and shopping malls and that they compete more heavily for these when local sales tax rates are higher. The fiscal zoning effect on big box and anchor stores may arise because these stores generate particularly high sales tax revenue or because they are particularly sensitive to local officials' efforts to attract them. And third, increases in sales tax rates lead to lower employment in manufacturing, suggesting that local officials' efforts to attract big box stores and shopping centers crowd out manufacturing.

II. Literature Review

The hypothesis that local government officials use zoning to choose land uses based on the relationship between the local tax payments they generate and the cost of providing them with local public goods was first proposed by Hamilton (1975) as an extension to the Tiebout Model (1956). In Hamilton's model, the main source of local government revenue is the property tax, and local governments use zoning to prevent land uses from being developed if they would cost the local government more than they would generate in property tax payments. The model assumes that households

[2] See Mark et al. (2000) for an analysis of the effects of sales taxes and other taxes on economic activity within a single metropolitan area.

can move costlessly between jurisdictions, so that households that are excluded from one jurisdiction because they would pay less than their cost of local public goods move to other jurisdictions and households that would pay more in property taxes than their cost of local public goods choose to move to other jurisdictions. As a result, all jurisdictions are homogeneous with respect to housing and demand for local public goods, so that the property tax becomes a benefit tax. White (1975) and Ohls et al. (1974) extended the model by assuming that local government officials use zoning to select land uses that maximize the local government's tax revenue net of the costs of supplying local public goods.[3] They argued that local government officials tend to prefer single-family houses over apartments, because single-family houses generate higher property tax payments, and prefer commercial and industrial land uses over housing since housing generates high costs for local governments by increasing enrollment in public schools.[4]

Political scientists have also examined local officials' use of zoning to encourage land uses that generate high tax revenues, and they extended the idea to include local sales taxes as well as property taxes. They argue that local officials use zoning to encourage retailing at the expense of other land uses, because retailing brings in the most sales tax revenue. These studies often use California as their setting, since property tax revenues in California have fallen sharply since restrictions were imposed in the late 1970s, and local governments there are allowed to adopt local sales taxes. One such study, by Lewis and Barbour (1999), surveyed city managers in California to determine what types of development they favor and why. The study concluded that city managers strongly favor retailing and do so because it generates additional sales tax revenue. But Lewis and Barbour's study failed to examine whether local officials are more likely to use zoning to attract retail activity when sales tax rates are higher, nor did it examine whether officials' efforts were successful.[5,6]

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3 See Mieszkowski and Zodrow (1989), Fischel (1992, 2001), and Zodrow (2001) for discussion of the Tiebout model with zoning, the conditions for the property tax to be a benefit tax, and other approaches to the incidence of the property tax. A recent paper that uses the concept of fiscal zoning to model the behavior of local governments in China is Gordon and Li (2012).

4 However, commercial and industrial land uses may be undesirable if they generate high pollution or congestion.

5 Political scientists tend to ignore the issue of whether different types of development generate different costs of supplying local public goods. For other discussions by political scientists of fiscal zoning and competition for

III. Theoretical Model

In this section, we develop a theoretical model of fiscal zoning under sales taxes and derive testable implications. Assume that local government officials use their zoning power to determine the types of development that occur within their jurisdictions. They do so by assigning tracts of vacant land to zoning categories that include retailing, manufacturing, office buildings, housing, and others. Although local officials have multiple tools at their disposal for controlling development, in the model we focus exclusively on zoning. Suppose Z_r denotes the number of acres of vacant land zoned for retail use in jurisdiction j and Z_m denotes the number of acres of vacant land zoned for manufacturing use in jurisdiction j. When local officials in jurisdiction j zone additional land for retail use, they may compensate by zoning less land for manufacturing use, less land for any other use, or they may leave less land vacant in the jurisdiction. Similarly, when they zone additional land for manufacturing, they may zone less land for retail use, less land for any other use, or leave less land vacant. This means that the relationship between the amount of land zoned for retailing versus manufacturing can range from complete substitution to no substitution, or $-1 \leq \partial Z_r/\partial Z_m \leq 0$. At the extremes, if $\partial Z_r/\partial Z_m = -1$, then zoning one more acre of land for retail use implies zoning one less acre of land for manufacturing use and, if $\partial Z_r/\partial Z_m = 0$, then zoning one more acre for retail use has no effect on the number of acres zoned for manufacturing use.[7,8]

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retailing, see Misczynski (1986), Fulton (1998), and Schrag (1998).

[6] Another paper related to ours is Wassmer (2002). Wassmer regresses the amount of retailing in U.S. metropolitan area suburbs on sales tax and property tax collections, using data for 55 U.S. metropolitan areas. He finds a positive relationship between suburban retail sales and sales tax collections, but no relationship between retail sales and property tax collections; he argues that this provides evidence of fiscalization of land use. A problem with Wassmer's study is that it ignores the upward bias in the relationship between retail sales and sales tax collections that results from the mechanical relationship between them.

[7] For $\partial Z_r/\partial Z_m$ to take values greater than −1 but less than 0, it must be the case that the total amount of land developed for retail plus manufacturing use is less than the total amount of vacant land in the jurisdiction. In theory, $\partial Z_r/\partial Z_m$ could also take values greater than 0 or more negative than −1, but we ignore these possibilities.

[8] We also assume that all land zoned for retail or manufacturing use is developed quickly for that use, so that zoning is a binding constraint on the amount of retail and manufacturing development. In actuality, local officials might zone more land for particular uses than developers wish to build on, so that zoning is not a binding constraint. In that case, higher sales tax rates would probably have a negative effect on the level of retail sales, since retailers would tend to choose jurisdictions with low sales tax rates. Given that we find evidence of a positive rather than a negative relationship for certain types of retail stores, our results cannot be explained by the lack of a binding zoning constraint on retail development. We also ignore rezoning of already developed land. When developed land is rezoned, it is allowed to remain in the old use ("grandfathered"), subject to some limitations. See Fischel (2001) and

The value of land zoned for retail use and manufacturing use in jurisdiction *j* is denoted V_r and V_m per acre, respectively. V_r depends negatively on the total amount of land zoned for retail use in jurisdiction *j*, Z_r, and may also depend negatively on the local sales and property tax rates in jurisdiction *j*, denoted σ and π.[9] Also, because sites in neighboring counties are substitutes for sites in jurisdiction *j*, V_r depends positively on the sales tax rate in neighboring jurisdictions, denoted τ.[10] Similarly, V_m may depend negatively on the total amount of land zoned for manufacturing Z_m and on the property tax rate in jurisdiction *j*, π. The value of retail sales per acre in jurisdiction *j* is denoted *S*, where *S* could depend either positively or negatively on the amount of land zoned for retailing Z_r. The relationship between *S* and Z_r is likely to be negative because additional stores compete with each other for sales, but could be positive if additional stores increase shopping agglomeration economies—possibly by increasing variety. Because taxes are passed on to consumers in part as retail price increases, *S* also depends negatively on the sales and property tax rates in jurisdiction *j*, σ and π, and positively on the sales tax rate in neighboring jurisdictions, τ. *S* may also have a random component to reflect the fact that stores and shopping centers may be successful or unsuccessful. The cost of providing local public goods to new retail or manufacturing uses is assumed to be *G* per acre.[11]

Local officials are assumed to choose how much land to zone for retail and manufacturing use so as to maximize the net tax revenues (*NTR*) generated by new development. They are assumed to treat local tax rates as fixed, because rates are chosen by voters in a referendum or by the local legislature. *NTR* is given by:

Zodrow (2001) for discussion of whether zoning is binding, and further references.

[9] If local taxes are benefit taxes for retailing firms, then the revenue generated by an increase in the sales tax and/or property tax rate on retail firms would be fully offset by increases in spending on local public goods that benefit retailers. In this case, there would be no relationship between the sales and/or property tax rates and the value of land zoned for retailing. See Zodrow (2001) for discussion of conditions under which local taxes become benefit taxes and no capitalization of local taxes occurs. But this question has been not been investigated for models in which there are multiple land uses or multiple tax sources.

[10] We simplify by assuming there is only one neighboring tax rate.

[11] For lack of evidence to the contrary, we assume that this cost is the same for retailing and manufacturing. A planning manual for local officials on how to assess the fiscal impact of development (Burchell et al., 1985) does not distinguish between the fiscal impact of new manufacturing versus commercial development, suggesting little difference in the cost of providing local public goods to them. Local public goods provided to non-residential uses include services such as police and fire or infrastructure or both.

$$NTR = \sigma \cdot S(Z_r, \sigma, \pi, \tau) Z_r + \pi \cdot V_r(Z_r, \sigma, \pi, \tau) Z_r + \pi \cdot V_m(Z_m, \pi) Z_m - G(Z_r + Z_m)\,. \qquad (1)$$

The first and second terms in (1) are local sales tax and property tax revenues from newly-developed retail stores, while the third term is property tax revenues from newly-developed manufacturing firms.[12] The fourth term is the cost of supplying local public goods to newly-developed retail stores and manufacturing firms.[13]

Local government officials choose Z_r and Z_m so as to maximize *NTR*.[14] Assuming an interior maximum, the first-order condition determining the amount of land zoned for retailing Z_r is:

$$\sigma \cdot S[\varepsilon_{S,Z_r} + 1] + \pi \cdot V_r[\varepsilon_{V_r,Z_r} + 1] + \pi \cdot V_m[\varepsilon_{V_m,Z_m} + 1]\frac{\partial Z_m}{\partial Z_r} - G[1 + \frac{\partial Z_m}{\partial Z_r}] = 0\,. \qquad (2)$$

The first term in (2) is the increase in local sales tax revenue that occurs when jurisdiction *j* zones more land for retailing and therefore increases the number of stores in jurisdiction *j*. This term will be positive as long as the elasticity of retail sales per acre with respect to the amount of land zoned for retailing, ε_{S,Z_r}, exceeds −1 (that is, as long as sales per acre do not decline too sharply in response to zoning more land for retail use). The second term is the change in property tax revenues from zoning more land for retailing. This term is positive as long as the elasticity of the value of retail land per acre with respect to the amount of land zoned for retailing, ε_{V_r,Z_r}, exceeds −1 (that is, as long as the value of

[12] See Section V below for discussion of our assumption that manufacturing firms pay little or no sales tax.

[13] We also worked through an alternate version of the model which assumes that local governments maximize a utilitarian social welfare function rather than net tax revenue. We defined social welfare over the population of existing residents and assumed that any new development would be non-residential (which means that the population of residents whose preferences must be considered remains constant). New development is assumed to generate enough revenue to pay for the marginal cost of local public goods provided to the development plus a surplus used to provide additional local public goods to residents (e.g., a new park). As in the current model, we assumed that land zoned for retailing and manufacturing may either be substitutes or unrelated.

The results are similar in the sense that all the terms in our model also show up in exactly the same way in the social welfare model. But the social welfare model contains additional terms that capture the negative direct effect on residents' utility of having additional land devoted to retailing or manufacturing (i.e., more pollution or congestion) and the positive indirect effect of having additional local public goods. How these extra terms affect the amount of land zoned for retail or manufacturing use is ambiguous. Since the social welfare version of the model is more complicated and does not change the implications for our main analyses, we use the simpler net tax revenue-maximizing model.

[14] We ignore strategic interactions between local governments, and treat policy in other jurisdictions as captured in τ; we defined τ to be the neighboring jurisdiction's sales tax rate, but we could also introduce the property tax rate in neighboring jurisdictions without changing the results. Although officials may care about net tax revenues per capita, we treat population as fixed rather than modeling population responses to policy changes.

retail land does not decline too sharply when more land is zoned for retail use).

The third term is the indirect effect of zoning more land for retail use on property tax revenues from manufacturing. This term is negative as long as land zoned for retail versus manufacturing use are substitutes ($\partial Z_m/\partial Z_r$ is negative) and the elasticity of the value of manufacturing land per acre with respect to the amount of land zoned for manufacturing, ε_{V_m,Z_m}, is greater than −1 (that is, as long as the value of land zoned for manufacturing does not increase too sharply when less land is zoned for manufacturing use). Finally, the last term measures the direct and indirect effect of zoning more land for retail use on the cost of supplying local public goods to retailing and manufacturing uses. The direct effect must be negative since costs rise when more land is zoned for retail use; while the indirect effect will be positive as long as land zoned for retail use is a substitute for land zoned for manufacturing use, but it is likely to be small.

Similarly, the first-order condition for the amount of land for zoned for manufacturing is:

$$\sigma \cdot S[\varepsilon_{S,Z_r} + 1]\frac{\partial Z_r}{\partial Z_m} + \pi \cdot V_r[\varepsilon_{V_r,Z_r} + 1]\frac{\partial Z_r}{\partial Z_m} + \pi \cdot V_m[\varepsilon_{V_m,Z_m} + 1] - G[\frac{\partial Z_r}{\partial Z_m} + 1] = 0\,. \qquad (3)$$

Each term in equation (3) has the same interpretation as the analogous term in (2), except that direct effects in one expression become indirect effects in the other. As long as $\partial Z_r/\partial Z_m$ is negative, all of the terms in (3) have the opposite signs of the analogous terms in (2). In particular, the first term is negative, so that more land zoned for manufacturing reduces sales tax revenue.

Now consider how an increase in the local sales tax rate affects jurisdiction *j*'s gain from zoning additional land for retailing versus manufacturing, which is central to the question we study. The increase in net tax revenue from zoning additional land for retailing when the sales tax rate σ rises is:

$$\frac{\partial^2 NTR}{\partial Z_r \partial \sigma} = S[1 + \varepsilon_{S,Z_r}] \cdot [1 + \varepsilon_{S,\sigma}] + \pi \frac{\partial V_r}{\partial \sigma}[1 + \varepsilon_{V_r,Z_r}]\,. \qquad (4)$$

where, for simplicity, we have assumed that the elasticity terms in (2) are constants, and that V_m and $\partial Z_r/\partial Z_m$ do not change with changes in σ.

The first term in (4) is the effect of the rise in the sales tax rate on sales tax revenues. It must be positive as long as $\varepsilon_{S,Z_r} > -1$ and $\varepsilon_{S,\sigma} > -1$. These inequalities require that sales per acre not decline so precipitously with increased zoning for retail use that total retail sales in jurisdiction *j* would fall, and that sales per acre not decline so precipitously with a higher sales tax rate that total retail sales in jurisdiction *j* would fall. The fact that jurisdictions zone any land for retail use and that they impose local sales taxes suggest that these two elasticity conditions are satisfied.[15]

The second term in equation (4) is the effect of the rise in the sales tax rate on property taxes collected on land zoned for retailing. $\partial V_r/\partial\sigma$ will be negative, except in the extreme case when local sales tax revenues are used entirely to fund local public goods for retail firms (when it equals zero). The elasticity term that multiplies it captures the additional effect on property tax revenues that comes about because additional land zoned for retail use reduces the value of retail land per acre. This term is negative as long as $\varepsilon_{V_r,Z_r} > -1$, which means that the value of retail land does not decline so precipitously as more land is zoned for retailing that total property tax revenue from retail land use in jurisdiction *j* falls.[16] But the second term is multiplied by the property tax rate—which in Florida is around 2%—so that it is small.[17] Thus, equation (4) implies that the return to zoning more land for retail use increases with the sales tax rate, except in the anomalous case where zoning more land for retail use generates losses in property tax revenue that more than offset the gains from additional sales tax revenue.

Equation (4) being positive does not directly imply that the amount of land zoned for retailing increases in response to a higher sales tax rate, but rather just shows how the effect of zoning more land for retailing on *NTR* varies with the local sales tax rate. To get a more specific prediction of how the

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[15] There are of course other factors that can influence the gain from zoning additional land for retail at a given sales tax rate, such as the size and income of the population in the neighboring jurisdiction, commuting patterns, traffic congestion levels, the level of retail variety in both own and neighboring jurisdictions, and negative or positive externalities. These should drop out of equation (4) because they do not change when a given jurisdiction's sales tax rate changes. However in the econometric analysis, the implication could be that the effects of changes in sales tax rates are heterogeneous. In our empirical work, we incorporate some variables that could be related to heterogeneity, such as population density and sales tax rates of cross-border jurisdictions.

[16] Note that the second term in equation (4) becomes positive if $\varepsilon_{V_r,Z_r} < -1$. If we think of the second term in (4) as the partial with respect to Z_r of $\partial NTR/\partial\sigma$, then this means that the value of retail land falls sharply as Z_r increases, implying that the property tax penalty from raising the sales tax rate becomes smaller.

[17] $\partial V_r/\partial\sigma$ is likely to be a multiple such as 10 to 20 times $\partial S/\partial\sigma$, because the value of retail land equals the net present value of future sales. But $\partial V_r/\partial\sigma$ is multiplied by the small property tax rate.

amount of land zoned for retailing is related to the sales tax rate, we totally differentiate equation (2) with respect to σ and Z_r , which yields:

$$dZ_r / d\sigma = -\left\{ \frac{S \cdot [\varepsilon_{S,\sigma} + 1][\varepsilon_{S,Z_r} + 1] + \pi \frac{dV_r}{d\sigma}[\varepsilon_{V_r,Z_r} + 1]}{\sigma \frac{dS}{dZ_r}[\varepsilon_{S,Z_r} + 1] + \pi \frac{dV_r}{dZ_r}[\varepsilon_{V_r,Z_r} + 1] + \pi \frac{dV_m}{dZ_m}\left(\frac{dZ_m}{dZ_r}\right)^2 [\varepsilon_{V_m,Z_m} + 1]} \right\}. \quad (5)$$

The numerator is the same as equation (4). The denominator represents the additional effect of changing Z_r when taking the total differential. We just argued that the numerator is positive. Under the same types of assumptions as made earlier that all of the elasticity terms are greater than −1, each term in the denominator must be negative, since S and V_r were assumed to be negatively related to Z_r, and V_m was assumed to be negatively related to Z_m. Thus, coupled with the negative sign outside the brackets, the entire expression is positive.

Overall, then, jurisdiction *j* gains from zoning additional land for retailing when the local sales tax rate rises, as long as retail sales per acre do not fall too quickly as zoning for retail use increases, and sales and land values do not fall too quickly as the local sales tax rate increases. Although we do not distinguish in our model between small retail stores versus big-box or anchor stores, this point may provide a reason why local jurisdictions respond to higher sales tax rates by zoning more land for big box or anchor store development. Compared to small stores, these developments are likely to generate higher sales per acre and/or shopping agglomeration economies that benefit small stores in jurisdiction *j*. The higher values of S raises the value of $\partial Z_r/\partial\sigma$ for big box or anchor stores compared to small stores.[18]

We can also solve for the effect on net tax revenues from zoning additional land for manufacturing when the local sales tax rate rises. The result (not shown) is the same as equation (4), multiplied by $\partial Z_r/\partial Z_m$. Assuming that $\partial Z_r/\partial Z_m$ is negative, this effect must be negative as long as (4) is positive. Thus, when the local sales tax rate rises, jurisdiction *j* has an incentive to zone more land for retail use and less land for manufacturing use. Or, more precisely, *if* jurisdiction *j* has an incentive to zone

[18] The value of (5) will also tend to be higher if the jurisdiction's stores have greater retail variety or less competition from stores in neighboring jurisdictions, because sales per acre are higher and/or because sales are less elastic with respect to the sales tax rate.

more land for retail use, then it also has an incentive to zone less land for manufacturing use. Further, suppose we assume that there is a stronger tradeoff between land used for large retail developments and manufacturing than between land used for small retail stores and manufacturing, because large retail developments and manufacturing firms tend to demand the same kinds of land. Then higher sales tax rates are likely to have a larger negative effect on the amount of land zoned for manufacturing because they make it more profitable for local officials to zone for big box stores and shopping centers.

Now turn to the question of how local sales tax rates affect employment in retailing in jurisdiction *j* (because we have data on employment, rather than land use or zoning). To analyze this question, we must shift from analyzing the behavior of local officials to analyzing the behavior of retail store owners. To keep the analysis simple, we treat all retail stores in jurisdiction *j* as though they were a single store occupying all of the land zoned for retailing in jurisdiction *j*. Retail profits are then:

$$\frac{1}{(1+\sigma)}Q^r(Z_r, L_r, \sigma) - wL_r - (r+\pi)Z_r V_r(Z_r, \sigma, \pi, \tau). \qquad (6)$$

Here Q^r denotes revenue from retail sales including sales taxes, which depends positively on inputs of land zoned for retailing Z_r, and labor in retailing L_r. Q^r is also assumed to depend on the sales tax rate, since retailers may pass on sales tax increases by raising prices. We make the usual convexity assumptions for the retail production function, i.e., $Q^r_{LL} < 0$ and $Q^r_{LZ} > 0$, and we assume that the marginal revenue product of labor falls when the local sales tax rate rises, i.e., $Q^r_{L\sigma} < 0$. The second term in (6) is the cost of labor, where the wage rate is denoted w, and third term in (6) is the rental cost of land per acre per year, $(r + \pi)V_r$, where r is the interest rate. Retail store owners hire labor until the marginal revenue product of labor equals the wage rate, or

$$\frac{1}{(1+\sigma)}Q^r_L = w\ . \qquad (7)$$

To consider how retail store owners change the level of retail employment when jurisdiction *j*'s sales tax rate rises, we totally differentiate (7) and solve for $dL_r/d\sigma$. The wage rate w is assumed to be fixed on the assumption that it is set in a larger market. The result is:

$$\frac{dL_r}{d\sigma} = \frac{Q_L^r}{(1+\sigma)Q_{LL}^r} - \frac{Q_{L\sigma}^r}{Q_{LL}^r} - \frac{Q_{LZ}^r(dZ_r / d\sigma)}{Q_{LL}^r}. \tag{8}$$

This equation has three terms. The first term captures the negative effect on retail employment from loss of revenue due to the rise in the sales tax rate, which must be negative. The second term captures the effect of the higher sales tax rate on the marginal revenue product of labor, which is also negative since $Q_{L\sigma}^r < 0$, but is likely to be small. The third term captures the effect of the sales tax change through zoning. When the sales tax rate is higher, local officials zone more land for retailing ($dZ_r/d\sigma > 0$), which causes the cost of retail land to fall and the land-to-labor ratio in retailing to rise. Retail employment therefore rises because the marginal revenue product of labor is higher $(Q_{LZ}^r > 0)$. The third term therefore is positive and offsets the negative first and second terms, so that fiscal zoning either makes the retail employment-sales tax relationship less negative or changes its sign from negative to positive. We test for this in our empirical work.

Finally, if the fiscal zoning response to an increase in the local sales tax rate is stronger for big box and anchor stores, then $dZ_r/d\sigma$ in equation (8) will be more positive for these stores, and we will be more likely to find a positive employment response to higher sales tax rates when we examine large store employment.

We can similarly model manufacturing employment using a single manufacturing firm that is assumed to occupy all of the land zoned for manufacturing in jurisdiction *j*. Manufacturing profits are then:

$$Q^m(Z_m, L_m) - wL_m - (r+\pi)Z_m V_m(Z_m, \pi) \ . \tag{9}$$

In equation (9), Q_m denotes manufacturing revenue, which depends on inputs of land Z_m and labor L_m. We also assume the convexity conditions $Q_{LL}^m < 0$ and $Q_{LZ}^m > 0$. We follow the same procedure as above to derive $dL_m/d\sigma$, using the indirect relationships between land zoned for manufacturing, land zoned for retailing, and the local sales tax rate. The result is:

$$\frac{dL_m}{d\sigma} = \frac{-Q_{LZ}^m \frac{dZ_m}{dZ_r} \frac{dZ_r}{d\sigma}}{Q_{LL}^m}. \tag{10}$$

This condition is analogous to the third term in (8) multiplied by $\partial Z_m/\partial Z_r$. Thus the local sales tax rate affects manufacturing employment only through its effect on the amount of land zoned for retailing and manufacturing use. If there is no substitutability between the amount of land zoned for manufacturing versus retailing ($\partial Z_m/\partial Z_r = 0$), then (10) equals zero and manufacturing employment is predicted to be unrelated to the sales tax rate. But if there is some substitutability ($\partial Z_m/\partial Z_r < 0$), then local officials respond to an increase in the sales tax rate by zoning more land for retailing and less land for manufacturing. As a result, the cost of manufacturing land rises, so that the labor-to-land ratio in manufacturing rises and the marginal product of labor falls. Thus owners of manufacturing firms are predicted to reduce the number of manufacturing jobs. We also test this prediction in our empirical work.

IV. Institutional Environment for Local Sales Taxes in Florida

We use Florida data from 1992-2006 for our study. Florida has a state sales tax, which during our sample period was always 6%. Counties in Florida can also adopt local sales taxes that are added to the state sales tax; they are adopted or changed either by county voters in a referendum or by adoption of a county ordinance (The Florida Legislature's Office of Economic and Demographic Research, 2011). Although in the aggregate, property taxes are a more important source of revenue for Florida local governments than are local sales taxes, the latter are growing much more rapidly. After correcting for inflation, local sales tax revenue in Florida increased nearly three times as fast as property tax revenue between 1992 and 2008, rising from 1.9% of local government tax revenue to 3.8% (U.S. Census Bureau Census of Governments, 1992 and 2007). Moreover, local sales taxes are an important share of the taxes that retail stores pay to local governments. Using data for 2008-09 payments of sales tax and property tax by the commercial sector in Florida, we estimate that during our study period commercial properties paid 56% as much in local sales taxes as they paid in property taxes—and the figure would be higher if we could separate retail stores from other commercial properties. Overall, these figures suggest that Florida is a good test case for studying the effects of increased reliance by local governments on sales taxes.[19]

Local sales taxes consist of seven separate taxes. All are levied at the county level, apply uniformly across the county, and have the same tax base as the Florida state sales tax. The overall local sales tax rate in a county is capped at 1.5%. Tax revenues from two of these taxes—the infrastructure tax and the small county surtax—go directly to local governments (which in Florida are called municipalities).[20] These two taxes are the most commonly-used local sales taxes in Florida and they account for most of the revenues. During our sample period, an average of 44 out of 67 Florida counties imposed one or both of these taxes and the average sales tax rate for the two taxes in counties using them was 0.96%. Revenues from these two taxes are shared among municipalities either based on their shares of county population or on where the sales occurred. For revenues to be distributed according to where sales occurred, the county must adopt an interlocal agreement.[21] During our sample period, around 35% of counties that used these two taxes had interlocal agreements (Florida Legislative Committee on Intergovernmental Relations, 2003).

The other five local sales taxes are levied by counties but go to county-level special-purpose authorities, such as school boards, and health, welfare or transportation authorities. During our sample period, an average of only eight school boards, two health or welfare authorities, and one transportation authority levied local sales taxes. The average local sales tax rate for these taxes in counties that levied them was 0.48% during our sample period. See Table 1A for information on local sales tax rates by type of tax.[22]

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[19] Sales tax and property tax payments by the commercial property sector in Florida were $6,393,000,000 and $1,193,000,000, respectively, in 2008-09. (We were unable to obtain these figures for earlier years.) The figure for sales tax payments includes both state and local sales taxes. Using average figures for our sample period, the average total sales tax rate was 6.7%, of which the local sales tax rate was 0.7%. Thus the ratio of local to total sales tax payments was 10.4%. (See Table 3.) This implies that the ratio of local sales tax payments to property tax payments by the commercial property sector is approximately .104 (5.4) = .56. This information comes from Florida Department of Revenue, http://dor.myflorida.com/dor/property/taxpayers/cmdata/08table2.html (viewed November 4, 2012) and http://dor.myflorida.com/dor/taxes/colls_from_7_2003.html (viewed October 7, 2012).

[20] The small county surtax can be used only by counties with populations less than 50,000.

[21] Although county-specific interlocal agreements can be proposed by either the county or municipal governments, the governing bodies representing the majority of the respective county's municipal population must ultimately approve the agreements.

[22] School boards in Florida differ from other county-level authorities because they have independent authority to levy local sales taxes; other authorities' local sales taxes must be levied by the county. The school board tax is the

Formally, land-use policy in Florida is set at the municipal level, while local sales tax policy is set at the county level. One issue is that officials in most Florida municipalities might appear to have little incentive to engage in fiscal zoning, because the sales tax revenue that additional retailing would generate either goes to county-level authorities or is shared among municipalities within a county according to relative population. However counties that adopt local sales taxes clearly have an interest in attracting retailing and county officials have various ways of encouraging municipal officials to adopt zoning policies that favor retailing. One method is for the county to adopt an interlocal agreement that distributes revenues from the local sales tax to municipalities based on where retail sales occur. Another method is for the county to directly reward municipalities that engage in fiscal zoning by giving them additional infrastructure. A third method is for county officials to engage in fiscal zoning directly by encouraging retail development in unincorporated areas; in these areas, counties rather than municipalities are responsible for zoning.[23]

In our empirical work, we ignore the county-municipality distinction and treat counties as though they both decide the local sales tax rate and determine zoning policy. We therefore examine the fiscal zoning hypothesis using county-level data as our unit of analysis. This potentially raises endogeneity concerns, since sales tax rates could be set in response to changes in retailing or manufacturing activity. We examine this issue further in Section VI.

Finally, an important advantage of using Florida data is that counties adopted local sales taxes or changed their rates fairly frequently during our study period. As of 1992, nearly half of Florida counties had no local sales taxes; by 2006, this figure had dropped to 13%. Also, between 1992 and 2006, there were 75 changes in aggregate local sales tax rates and 32 instances of counties imposing a local sales tax where there was none before.

School Capital Outlay Tax, the transportation tax is the Charter County Transit System Tax, and the health/welfare authority taxes are the Indigent Care and Trauma Center Tax (Dade County only), the County Public Hospital Tax (Dade County only), and the Indigent Care Tax. In addition, there are several other local sales taxes that we exclude from our study, including a tourist development tax, a convention center tax, and a professional sports facility tax. These are excluded because they are imposed on hotels/motels and therefore do not create incentives for local governments to expand retail activity. See Florida Legislative Committee on Intergovernmental Relations (2009 and earlier years).

[23] Municipalities may nonetheless benefit by subsequently annexing the newly-developed land along with nearby areas (e.g., King, 2010; Shoer Roth, 2010).

Tables 1B and 2 give a history of local sales tax rate changes by county over this period. Given that our empirical approach is based on a difference-in-differences analysis relating changes in employment to changes in county tax rates, this extensive within-county variation in sales taxes is essential to identifying the effects of sales taxes. Aggregating across all of the changes in Table 2, there are 56 sales tax increases and 19 decreases. The table also shows a relatively consistent pattern of increases throughout the sample period, rather than any kind of cyclical pattern that could potentially confound our estimation of the effects of sales tax rates.[24]

V. Data and Approach

We use panel data from the National Establishment Time Series (NETS) on the universe of individual business establishments in Florida from 1992 to 2006. For each establishment, we know employment each year and the establishment's name, industry, and location.[25] We aggregate these data to get total employment in retailing and manufacturing by county for each of the 67 Florida counties for each year from 1992 to 2006.

The NETS data are not the only source of county-level employment information. The County Business Patterns data, closely related to the Quarterly Census of Employment and Wages, also provide such information. However, the data are often suppressed or reported as ranges for confidentiality reasons. Moreover, much of our interest focuses on big box and anchor retail stores, which can be identified by name in the NETS data. This is not possible in the County Business Pattern data, because company names are not included and there are no sub-categories within the retail sector. We therefore use the NETS data for our analysis.

Our basic model estimates how changes in local sales tax rates affect retail employment. As discussed in connection with equation (8) above, the predicted sign of this relationship is ambiguous: fiscal zoning can make it positive, but it will be negative if there is no zoning response to a rise in the

[24] Nonetheless, our models include fixed effects which control for aggregate cyclical effects. But because economic conditions across counties can diverge, we also estimated our models including a control for county-level unemployment rates—the results were virtually identical to those reported below.
[25] A detailed discussion of the NETS data along with assessment of its quality is provided in Kolko and Neumark (2007) and Neumark et al. (2007).

sales tax rate. Another factor not discussed in the model is that competition across counties to attract retailing may be a zero-sum game, since one county's success may come at the expense of nearby counties if the area can only support a limited number of stores. To the extent that counties compete with each other for retailing in response to sales taxes, the estimated relationship will tend to be weaker. Finally, if fiscal zoning has little overall effect on land use (because most land zoned for retailing remains vacant), then the estimated relationship will tend to be small/insignificant. Given all these considerations, finding a positive relationship between changes in local sales tax rates and the level of retail activity would clearly provide evidence of fiscal zoning.[26]

Another question is whether the level of retail employment is well suited to measuring the strength of fiscal zoning. A more direct measure would be the amount of land zoned for retail use. But land zoned for retailing is an imperfect measure of the level of retail activity, both because stores may be vacant and because some land zoned for retailing may be vacant. Vacancies of both types may result if local government officials zone too much land for retailing (contrary to the assumptions of our model). In addition, as noted in Section I, local government officials can also encourage retailing by allowing land zoned for retailing to be developed at higher density levels or by expediting permit and inspection procedures. Using retail employment as our measure of the effect of fiscal zoning has the advantage that it encompasses all three policies and that it measures retail activity only for stores that are actually in operation.

Because large retail concentrations are particularly valuable in areas with higher local sales tax rates, we also examine the effect of fiscal zoning on retail employment in big box stores, such as Wal-Mart, Home Depot, and Costco, and department stores of the type that anchor large shopping malls, such as Macy's. Our hypothesis is that local government officials in jurisdictions that have high local sales taxes are particularly likely to compete for these stores and the associated shopping malls.[27]

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[26] An additional factor is that the price elasticity of demand for goods bought from brick-and-mortar stores has been increasing over time as sales migrate to the internet. See Goolsbee and Zitran (1999).

[27] The stores in our big box/anchor category are: Best Buy, Big Lots, Bloomingdales Inc., Circuit City, Costco, Federated Retail Holdings (Lord & Taylor), Home Depot, J.C. Penney, K-Mart, Lowes Home Centers, Macy's, May Department Stores, Montgomery Ward, Neiman Marcus Group Inc., Nordstrom Inc., Office Depot, Office Max,

We also estimate similar models for manufacturing employment.[28] As discussed in connection with equation (10) above, county-level manufacturing employment is predicted to fall in response to higher local sales tax rates if local officials engage in fiscal zoning. This is because local government officials zone less land for manufacturing and more for retailing when the sales tax rate is higher, causing manufacturing to be crowded out by retailing. In contrast, if there is no fiscal zoning then our prediction is that there will be no relationship between manufacturing employment and local sales tax rates.

Table 3, Panel A, gives summary statistics for our county-level sample. All sales tax rates are the sum of the state sales tax rate of 6% and local sales tax rates. The neighboring sales tax rates in the county-level sample are a weighted average of the sales tax rates in all bordering counties, using the lengths of the borders with each neighboring county as weights.[29] Sales tax rates in Florida ranged from 6.0% to 7.5% during our sample period, while sales tax rates in the neighboring counties in Alabama and Georgia (which also use local sales taxes) ranged from 5.0% to 7.0%.[30] The table also shows that industry employment shares vary widely across areas. For example, in the county-level sample, the share of employment in retail ranges from 4.17% to 29.30%, and the share of employment in manufacturing ranges from 1.55% to 47.29%. Employment levels also vary greatly—for example, from near zero to 241,000 jobs in retailing and from near zero to 112,000 jobs in manufacturing at the county level.

Finally, as we describe in more detail in Section VII, we also examine a separate sample in which

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Saks & Company, Sears Roebuck, Staples, Target Corporation, and Wal-Mart. The list of big box retailers is taken from Mazzolari and Neumark (2012) and we added additional stores identified as those that anchor malls in Florida, from Oyston (2007).

[28] Our model simplified by assuming that manufacturers do not pay any local sales tax; but in fact manufacturers in Florida are required to pay sales tax on sales to contractors, but not on sales to resellers (the latter resell the product in the same form or after further processing). Figures given above in footnote 19 imply that the ratio of local sales tax payments to property tax payments for the retail/commercial property sector in Florida is 56%. If we follow the same procedure for the manufacturing sector, we find that ratio of local sales tax payments to property tax payments for manufacturer/industrial property in Florida is only 12%. The more than four-fold difference in local sales tax payments across the two sectors justifies the fact that we ignore sales tax payments by manufacturers. Additional evidence suggesting that sales tax payments by manufacturing firms are low comes from Hawkins and Murray (2004), who run a regression explaining sales tax collections as a function of manufacturing employment and other variables, using data for six states including Florida. They find that sales tax collections have little relationship to the level of manufacturing employment. Ring (1999) and Traeger and Williams (1997) both calculate the fraction of general sales tax revenue paid by Florida businesses versus consumers, but they do not distinguish between retail versus manufacturing firms.

[29] To account for coastline and not overweight land borders, we include coastline as part of the border, using the county's own tax rate as the tax rate on this border.

[30] Data are from Georgia Department of Revenue (2011) and Alabama Department of Revenue (n.d.).

counties are divided into border versus interior regions. The statistics for these border and interior regions are reported in Table 3, Panel B. Because these regions are subdivisions of counties, the sales tax rates are the same, but employment levels are lower.

VI. County-level Specifications and Results

Empirical Approach

Our base case model, for the analysis of the effects of sales tax rates on county-level retail employment, is the following:

$$\ln RE_{ct} = \alpha + \beta_1 tax_{ct} + \beta_2 tax_{c,t-1} + \gamma_1 neighbortax_{ct} + \gamma_2 neighbortax_{c,t-1}$$
$$+ D_c\delta + D_t\lambda + D_c t\theta + \varepsilon_{ct} \quad . \qquad (11)$$

In equation (11), RE_{ct} is retail employment in county c in year t. The variables tax_{ct} and $tax_{c,t-1}$ are the state plus local sales tax rates in county c in the current year and the previous year, respectively, while $neighbortax_{ct}$ and $neighbortax_{c,t-1}$ are the weighted averages of the same sales tax rates in county c's neighboring counties (including counties across state borders) in the current year and the previous year, respectively. D_c and D_t are county and year fixed effects, and $D_c t$ is a set of county-specific linear time trends.

The coefficients of interest are β_1 and β_2 and their sum, or the percentage change in retail employment when the sales tax rate in the current year and/or the previous year rises by one percentage point, holding neighboring counties' sales tax rates fixed. Note that because the Florida general sales tax rate was 6% throughout our sample period, all changes in sales tax rates are due to changes in local sales tax rates. Therefore, what we identify is the effect of variation in local sales tax rates only, and identification comes from changes in individual counties' sales tax rates over time. This regression captures the key equations from our theoretical model—equations (5) and (8). Equation (5) implies that a jurisdiction is likely to zone additional land for retailing when the sales tax rate rises. Equation (8) implies that retail employment will be negatively related to the sales tax rate in the absence of fiscal zoning, but can become positively related to the sales tax rate if zoning is sufficiently strong.

The county fixed effects in equation (11) control for time-invariant differences between counties in unmeasured county-specific characteristics that may affect the level of retail employment, while the year fixed effects and county-specific time trends capture any national changes and local trends in retail employment. The county-specific trends, in particular, are intended to capture sources of endogenous variation stemming from trends in retail growth that could affect sales taxes, an issue we discuss more below. We estimate equation (11) using OLS. Robust standard errors are clustered at the county level to allow for arbitrary patterns of serial correlation within counties and for heteroscedasticity across counties.[31]

For the county-level analysis, we report both unweighted regressions and regressions weighted by 1992 county population levels, which are closely related to county employment levels at the beginning of our sample period.[32] Because county population and employment levels vary widely in Florida, the weighted estimates of how sales tax rates affect retail employment are more representative of how the average individual is affected. Moreover, the unweighted data give us some high estimates of marginal effects, because increases in employment levels can be very high in percentage terms when they start from a low base. Using weights in effect reduces the influence of these extreme measurements in small counties.[33]

Basic Results

Results of regressions explaining total retail employment are shown in Table 4A. The results in columns (1) and (1') include only the current sales tax rates, while those in columns (2) and (2') include

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[31] We would have liked to incorporate property tax rates as well as sales tax rates in our regressions. However property tax rates in Florida are set by 600+ municipalities rather than by counties and we were not able to locate historical data. Property taxes in Florida are paid mainly by residential rather than non-residential property and therefore are set based on residential property assessments. Because Florida residential property assessment cannot rise by more than 3% per year or the inflation rate (whichever is lower)—since a property tax limitation measure implemented in 1995—changes are likely to be similar across counties. In contrast, local sales tax rates are the sum of the seven local sales taxes discussed above, and counties vary widely in which taxes they levy. As a result, changes in these tax rates vary substantially across counties, as documented in Tables 1-2. Because of these differences, we expect property tax rates to be accounted for by the county and year fixed effects and we do not expect property tax rates and sales tax rates to be highly correlated.

[32] County population estimates for 1992 were supplied by the Bureau of Economic and Business Research, University of Florida. We use weights at the beginning of the sample period to avoid any responses of population to the policy variation we study. However, we also re-estimated our key models using population weights as of 2000—just past the middle of the sample period—and the results were nearly identical.

[33] When researchers use a dataset that constructs aggregates from a sample, they must worry about heteroscedasticity driven by variation in the number of observations from which the aggregates are estimates. However in the case of the NETS, we have the universe of business establishments, not a sample.

both the current and lagged sales tax rates. Column numbers without primes give results from unweighted regressions, while those with primes give results from weighted regressions. The results show that total retail employment is not significantly related to sales tax rates, regardless of whether weights are used and whether we include the lagged sales tax rate in addition to the current rate. This is true for the individual coefficient estimates, as well as for the sums of the current and lagged sales tax rates, which are shown in the second-to-last row of the table. The summed effect of the current plus lagged sales tax rate on total retail employment is positive—which is consistent with fiscal zoning—but statistically insignificant. In columns (3) and (3') we add the neighboring counties' sales tax rates, both current and lagged, to isolate the effects of independent variation in local sales tax rates. The coefficients of the own local sales tax variables remain insignificant, and are largely unchanged. Thus our results suggest that fiscal zoning by counties with high local sales tax rates does not result in a significant increase in total retail employment.

In Table 4B we report estimates of the same regression models, but our dependent variable is now employment in big box and anchor stores only. Here the coefficient of the current sales tax rate is positive and significant at the 5% level in the weighted regression shown in column (1'), and at the 10% level in the unweighted results in column (1). Also the combined coefficient of the current and lagged sales tax rates is positive and significant in the weighted regressions in columns (2') and (3'), regardless of whether the neighboring sales tax rate is included or not. For the latter specification, the sum of the unweighted estimates is positive and statistically significant as well (column (3)). The weighted regression results suggest that employment in big box and anchor stores rises by 15% to 17% for each percentage point increase in the local sales tax rate. In elasticity terms, the increase is around 0.9 to 1.1. The unweighted regression results are larger, but they may be influenced by large employment changes from a small base in sparsely-populated counties (which are down-weighted in the weighted estimates).

In terms of the model, in discussing equation (8) we showed that the employment response to a higher sales tax rate could become positive if local officials increase the amount of land zoned for retail use. Thus, these results for big box and anchor stores indicate that local officials focus their fiscal zoning efforts on big box stores and shopping centers when sales tax rates rise, and their efforts are successful.[34,35]

Another implication of the strong positive relationship between higher sales tax rates and employment in big box and anchor stores is that large retail establishments and shopping malls may crowd out smaller retail establishments in counties that impose a local sales tax. If this were true in general, it might be a contributing factor to our finding of an insignificant relationship between the local sales tax rate and total retail employment. To test this hypothesis, we estimated the effect of the local sales tax rate on small store retail employment, which we define as total retail employment minus employment in big box and anchor stores. The results, which are reported in Table 4C, show that small store retail employment is not significantly related to local sales tax rates, although the point estimates are negative. These results suggest that big box/anchor stores do little to crowd out smaller stores (and that local government officials do not try to attract small stores in response to higher sales taxes).

Up to this point, we have found no effect of the sales tax rate on retail employment overall, but a positive effect of the sales tax rate on employment at big box and anchor stores. We attribute these results to the fact that big box and anchor stores provide local officials with the strongest incentive to use zoning to encourage development. As a falsification test, we estimated similar models for employment at grocery stores. Under Florida law, sales of grocery stores are untaxed, except for prepared foods and non-food items (Fla. Stat. Section 212.08 (2012)). Therefore, as long as we exclude grocery stores that sell many other items, we should find no effect of the sales tax rate on retail employment at these stores.[36] In contrast, if the mechanism underlying our evidence for big box/anchor stores had more to do with

[34] An alternative explanation for the fact that we find stronger effects of sales taxes on employment at big box/anchor stores than on overall retail employment is that demand for the products sold by big box/anchor stores is less price-elastic. Since the negative effect of an increase in the local sales tax rate on retail employment offsets the positive fiscal zoning effect, our results could be explained by the weaker negative price effect for big box/anchor stores than for retailing in general. We think this explanation of our results is unlikely, in part because our big box and anchor stores range from Wal-Mart to Neiman Marcus, for which the elasticity of demand is likely to be quite varied. Even if the alternative explanation based on different price elasticities is correct, our results would still suggest that fiscal zoning plays an important role for big box/anchor stores, since for these stores the *net* effect of a higher sales tax is increased employment.

[35] As suggested by a referee, our evidence that higher sales tax rates lead to more employment in big box/anchor stores, but no increase in retail employment overall, could be explained by higher sales per worker at big box/anchor stores, coupled with higher sales tax rates leading to a substitution of big box/anchor stores for smaller stores. In that case overall retail employment need not increase and could even decline despite retail sales (and tax revenue) rising.

[36] In our data, Wal-Mart and Costco are sometimes classified as grocery stores. Because they sell many taxable items we classified them as non-grocery stores.

spurious correlation between changes in sales tax rates and the building of larger stores (possibly driven by changes in residential patterns), we might expect to find similar results for grocery stores as for big box/anchor stores. As reported in Table 4D, we find no evidence that a higher sales tax rate increases employment at grocery stores. The point estimates are near zero and statistically insignificant, bolstering our interpretation of the results for big box/anchor stores as reflecting fiscal zoning.

Table 4E reports the results of regressions explaining manufacturing employment. These regressions test the prediction, captured in equation (10), that a higher sales tax rate will reduce employment in manufacturing because local officials respond by zoning more land for retailing and less for manufacturing. (As we noted, this prediction holds if there is a tradeoff between these two land uses, or $\partial Z_m/\partial Z_r$ in the model is negative; if there is no tradeoff, or $\partial Z_m/\partial Z_r = 0$, then the sales tax rate should not affect manufacturing employment.) The estimated effects—including the contemporaneous effects in columns (1) and (1') and the summed effects in columns (2)-(3')—are always negative, but never significant. However, in these latter specifications, the lagged effect of the sales tax rate is always significant at the 5% or 10% level. We therefore re-estimated the models with only a lagged effect. As shown in the columns (4) and (4') of the table, the estimated effect of the lagged sales tax rate is negative and significant at the 10% level in the unweighted estimate, and negative and significant at the 5% level in the weighted estimate. The point estimates suggest that when the sales tax rate rises by one percentage point, employment in manufacturing falls by 7% to 9%. The implied elasticities are −0.5 to −0.6.[37,38]

Finally, although our focus – and the discussion in the theory section – is on the use of land for

[37] Note that the evidence of negative effects of sales tax rates on manufacturing employment does not imply that manufacturing plants close as a result of sales tax increases. First, the estimates are relative, so a negative effect can simply mean that manufacturing employment grows more slowly in counties that raise sales tax rates. Second, establishments frequently die, and are replaced by other establishments. Changes in use of particular sites may reflect changes in land use after manufacturing facilities close.

[38] While our results suggest that manufacturing and retailing land uses are substitutable, the extent of substitutability may vary. In particular, one possibility is that they may be less substitutable in urban than rural areas, since less land in urban areas is suitable for manufacturing. On the other hand, the simple availability of undeveloped land in rural areas could reduce the degree of substitutability in these areas. We explored this question in two ways. First, we split our sample into urban versus rural counties, where counties were classified as rural if they met the U.S. Census definition of rural (average population density less than 100 people per square mile) at any time during our sample period. We reran our models separately on the urban and rural subsamples and found little difference in the results. Second, using the full sample we introduced interactions between the sales tax variables and measures of either the level or the log of population density. The estimated interaction terms were very small and statistically insignificant. (Results are available from the authors upon request.)

retailing versus manufacturing, the predictions for manufacturing also apply to other industries that use land intensively and produce untaxed services. Many services in Florida are taxed, but those of the finance, insurance, and real estate (FIRE) sector are taxed quite lightly, and the revenues do not go to localities.[39] Also, the FIRE sector occupies office space on land that could be used for retailing. We therefore estimate similar models for employment in FIRE and the results are in Table 4F. Similar to our results for manufacturing, the effect of the sales tax on FIRE sector employment are negative. In our view, this is further evidence that land use shifts to big box and anchor store retail employment in response to a higher sales tax rate, and away from other uses that do not generate sales tax revenue.

Since our results are sometimes sensitive to weighting (particularly the results for big box and anchor store employment), they may be influenced by large employment changes from a small base in sparsely-populated counties. We verified this by re-estimating the models for big box/anchor store and manufacturing employment with additional terms that interact the sales tax rate and (linear and quadratic terms in) the same measure of population that we used as weights. When we did this, the estimated interactions confirmed that the key results in Tables 4B and 4E are in fact stronger in areas with smaller baseline populations. Moreover, the results were less sensitive to whether or not we used weights, and the qualitative conclusions were the same based on the weighted and the unweighted estimates.[40]

Threats to Identification

The key identifying assumption for the specifications reported thus far is that local sales tax rates are uncorrelated with the residuals in our equation (11) explaining retailing and manufacturing employment. One potential source of correlation is other policies that could have changed over time at different rates in different counties, yet influenced (or been associated with) both sales tax rates and retail or manufacturing employment. The policy most likely to fit this description is the Florida Enterprise Zone program, which offers tax credits against sales taxes to firms that locate within a zone and create new jobs. An earlier program was terminated in 1994, and a new program was launched in 1995, beginning

[39] Taxes on sales in the FIRE sector include an insurance premium tax of 1.75% (with a tax credit for 10% of salaries paid to Florida insurance company employees) and a 0.7% tax on real estate transfers. These taxes go to the state. See Florida Senate Committee on Finance and Tax (2007).

[40] These results are not reported in the tables, but are available from the authors upon request.

with zones in 18 counties, and spreading to 43 counties by the end of our sample period.[41] It is possible that Enterprise Zones were established in areas that were losing manufacturing jobs and that they generated new retailing jobs. Given the tax credit, Enterprise Zones would have been more valuable to employers in areas with higher sales tax rates, hence potentially generating our results via a mechanism different from the fiscal zoning hypothesis.[42]

To test whether the effects of sales taxes are confounded with the effects of Enterprise Zones, we coded a dummy variable equal to one in county-years in which an Enterprise Zone exists and we added that variable to our models explaining employment in big box/anchor stores and in manufacturing. The results are reported in Table 5A. The time period covered is 1996-2006,[43] and because of the shorter sample period, for each specification we first report the baseline estimates for this shorter sample period without including the Enterprise Zone control (the columns without primes), which change a bit from Tables 4B and 4E. The key question is the sensitivity of the estimates to including the Enterprise Zone control. The table makes clear that there is no evidence that Enterprise Zones account for the effects of sales taxes on big box/anchor store employment or manufacturing employment. For both types of employment, the estimated effects of sales taxes with and without the Enterprise Zone control are virtually identical.

A second policy that could confound other tax variation with sales tax variation is the use of tax increment financing (TIF), which dedicates future tax revenues in an area to pay for infrastructure improvements within the area. In Florida, TIFs can only be used in Community Redevelopment Areas (CRAs), which are areas designated for economic redevelopment.[44] We obtained information from the Florida Department of Economic Opportunity on which county-years during our sample had CRAs and

[41] Data on enterprise zones by county comes from the Florida Department of Economic Opportunity (personal communication with Burt C. Von Hoff, January 22, 2012) and Executive Office of the Governor (2007). To the best of our knowledge, there are no other subsidies targeted to retailers in Florida.

[42] On the other hand, there could be a bias in the opposite direction. Because the Florida Enterprise Zone program allows the hiring credit to be taken against the sales tax that a business owes, an enterprise zone could reduce the attractiveness of a retail business to a county (at least from the perspective of sales tax revenue). In this scenario, if Enterprise Zones are created where sales tax rates are high, the estimated effects of sales taxes on retail employment could be biased toward zero, which would strengthen our findings.

[43] Although the new Enterprise Zones went into effect in 1995, we start our analysis in 1996 because we include one-year lags of sales tax rates.

[44] 1977 amendment (Section 163.387 Fla. Stat. Section 163.387) to the 1969 Community Redevelopment Area Act.

added a dummy variable to our specification that equals one for county-years in which one or more CRAs were in operation. The results are reported in Table 5B; they are comparable to those in the last two columns of Table 4B (for big box/anchors stores) and the last two columns of Table 4E (for manufacturing). A comparison of the estimates shows that adding the CRA variable has essentially no effect on our estimated effects of sales taxes.

A second threat to identification is the possibility that local sales tax rates could respond to changes in retailing or manufacturing employment. For example, an exogenous increase in retail sales might cause counties to raise their local sales tax rates in order to capture more revenue. In this case, endogeneity would generate a positive bias in the estimated relationship between sales tax rates and retail (or big box/anchor store) employment. However, the bias could alternately go in the opposite direction, because an exogenous *decrease* in retail sales might cause counties to raise their local sales tax rates in order to maintain tax revenues at the past level. These possibilities could bias our results by generating spurious evidence that could be either consistent with or contrary to the fiscal zoning hypothesis.

As noted earlier, we included county-specific time trends in the specifications in Tables 4A-4F to capture the possibility that sales tax rates might have changed endogenously in response to underlying changes in employment. When we re-estimate the models in Table 4A without county-specific time trends, the effect of sales tax rates on total retail employment remains small, slightly positive, and insignificant; these results suggest that for overall retail employment there is not an endogeneity problem. When the models for big box/anchor store employment in Table 4B are re-estimated without county-specific trends, the effect of sales tax rates remains positive, but becomes smaller and insignificant. This is the opposite of what we would expect if trend growth in employment led to the adoption of higher sales tax rates, and instead is consistent with a slight tendency for tax rates to increase when trend growth is negative. The findings for big box/anchor retail employment imply that it is important to include the county-specific time trends as a partial control for endogeneity. More substantively, they suggest that the positive effects of sales tax rates on big box/anchor retail employment that we find are *not* driven by endogeneity.

We also did a more direct analysis of whether local sales tax rates are endogenous, by testing whether lagged changes in employment are related to changes in county-level sales tax rates. In these regressions, the dependent variable is the change in the local sales tax rate and the independent variables are the first, second, and third lags of the change in total retail employment, big box/anchor employment, or manufacturing employment; other control variables are the same as in the preceding county-level analyses.[45] As reported in Table 5C, we found no evidence that lagged changes in any of the employment measures predict changes in local sales tax rates: the estimated coefficients of the lagged employment variables were small and always individually and jointly insignificant. These results provide additional evidence that local sales tax rates are not endogenously determined by changes in employment; in other words, counties do not change their sales tax rates in response to increases in total retail sales or big box/anchor store sales. Overall, these additional analyses indicate that the county-level results are driven by exogenous changes in sales taxes.

Another potential identification issue is whether the key assumption underlying the difference-in-differences approach holds. Namely, this approach assumes that the policy change affects the area where the policy is implemented, but does not affect the "control" areas. If this assumption is violated and there are important cross-border effects, then the estimated sales tax coefficient could be biased; for example, if an increase in one county's local sales tax rate reduces sales in that county but increases sales in neighboring counties, then any negative effect of the sales tax rate would be overstated. In the preceding tables we found effects of neighboring counties' tax rates that are near zero and statistically insignificant, suggesting that these cross-border effects do not exist. However, we return to this issue in Section VII when we discuss the effects of sales taxes in border versus interior regions of counties.

Dynamics

The results for big box and anchor store employment and for manufacturing employment, in Tables 4B and 4E, sometimes point to evidence that the effects of sales taxes arise with a one-year lag.

[45] Since the baseline models have county fixed effects and county-specific linear time trends, in the first-differenced model the original county fixed effects drop out, and the county-specific time trends become county dummy variables.

We would not expect the effects of sales tax changes to be instantaneous, so the evidence of some lag in the effect is plausible; on the other hand, we might also expect some anticipation effects, as behavior begins to respond to future sales tax changes that are already enacted.

The standard within-group estimator may not pin down the dynamics of the effects of sales taxes very well, because the estimator uses deviations from means over all years for each county. For example, even if all of the effect occurs, say, two years after the sales tax changes, if the two-year lag is omitted one will still find an apparent "effect" of the contemporaneous sales tax rate, because its coefficient is identified from the partial correlation between the deviation of big box employment (for example) from its sample average for the county, and the deviation of the sales tax rate from its sample average for the county. To better pin down the dynamics, we also estimated first-difference models with additional lags. We estimate these models using one-year first differences, which are standard, but also using two-year first differences, which may be less noisy.[46] The results are reported in Panel A of Table 6 for big box/anchor retail employment and for manufacturing employment. The results are robust, and point to effects that are not instantaneous, but that emerge over two to three years. For manufacturing, the cumulative effects of the lagged sales tax variables are larger than the estimates in Table 4E, although the sample is a bit smaller because we have to drop some observations.

Table 6, Panel B, reports specifications where we add one- or two-year leads to the model. We report the lead effect, the sum of the contemporaneous and lagged effects, and the sum of all the effects including the lead. We also report, for comparison, the sum of the contemporaneous and lagged effects using the same sample but omitting the lead. There is sometimes evidence of a lead effect, which is not unanticipated. But the contemporaneous and lagged effects are robust to the inclusion of this lead. Thus, the combined results indicate that there is some anticipation effect of sales taxes, but most of the effect unfolds over two to three years after sales taxes increase.

A different perspective on dynamics concerns how the changes in big box and anchor store

[46] Taking first differences induces serial correlation in the errors. (This is also a problem, although less severe, in standard within-group estimation.) However, clustered standard errors take account of this non-independence among observations for a county.

employment come about. If the effects on sales taxes arise through fiscal zoning and other efforts by local government officials to attract retail stores, then we should find that higher local sales taxes are associated with more creation or relocation of new businesses. The NETS data are nicely suited to this question, since we can identify when an establishment first has positive employment in a county; see Neumark et al. (2007) for details. To verify that employment growth is generated by new establishments rather than growth of existing establishments, we measure the employment change created by new establishments in a county, and we estimate similar models as those in Table 4B. Here the dependent variables are in levels rather than logs, since there are many zeros.

The results for new establishments, defined over both one-year and two-year windows, are reported in Table 7. The estimates always indicate positive effects of sales tax increases, which are statistically significant in three out of the four cases shown in the table. Moreover, the magnitude of the estimates suggests that a one-percentage point increase in the sales tax rate leads to the opening of one or two big box or anchor stores (108-170 employees).[47] We also estimated these models for changes in retail employment at *non*-big box/anchor stores.[48] The estimates were uniformly negative, pointing to adverse effects on retail employment growth in small stores, presumably because local officials do not encourage establishment of new small stores. However, the latter estimates were very imprecise and never statistically significant.

VII. Border-Interior Specifications and Results

Local government officials' incentives to encourage retailing may differ in interior versus border regions of counties, and evidence of such differences can help confirm and refine the fiscal zoning hypothesis. Since sales tax rates may differ on either side of county borders, consumers have an incentive to shift their shopping to the lower-tax side. This means that the price elasticity of demand will tend to be higher near county borders than in county interiors, which undermines the effectiveness of fiscal zoning in border areas. We therefore predict that the relationship between local sales tax rates and retail employment will be less positive/more negative near county borders than in county interiors, taking

47 Descriptive statistics indicate that new big box or anchor stores have about 87-90 employees.
48 Results available upon request.

account of cross-border sales tax differentials.

To construct border versus interior regions, we use ArcGIS software to determine whether each establishment in the NETS is within 1 mile of a county border, and we assign each establishment to the county's interior versus its border. We further subdivide each county's border into separate regions for each neighboring county and we identify the neighboring county for each region. Parts of the border that are within one mile of more than one neighboring county are deleted, so that each border region has a unique cross-border county (which may be in a different state).[49] Border regions along the coast are combined with county interiors. Finally, we aggregate each type of employment within each border and interior region. This procedure increases the sample size from 67 counties to 277 border or interior regions. For each border region, the neighboring sales tax rate is now the actual sales tax rate across the border, while for interior regions, the neighboring sales tax rate is set equal to the own sales tax rate. Table 3, Panel B, gives summary statistics for the border-interior sample.

In the border-interior regression models, employment by sub-county region-year replaces employment by county region-year as the dependent variable, fixed effects for sub-county regions replace county fixed effects in equation (11), and we still include interactions between a time trend and county dummies. We also create a dummy variable that equals one for border regions and we add interactions between the border dummy and the sales tax rate. These interaction terms allow us to estimate (and test for) differences between border versus interior regions in the effects of sales tax rates on employment. We report only unweighted estimates for the border-interior regressions. Because the sub-county regions tend to be small, many individuals live and work in different regions. Thus population-based weights are less accurate in representing employment levels and weighted regressions are therefore less informative. Standard errors are clustered at the sub-county region level.

The results of the border-interior regressions explaining total retail employment are shown in

[49] To illustrate our procedure, suppose a county is shaped like a square with sides of 10 miles in length, and has one neighboring county on each side. Our procedure divides the county into nine sub-county regions: one square interior region having sides of length 8 miles, four rectangular border regions that are each one mile by 8 miles, and four square corner regions that are each one mile square. The corner regions are dropped because they border more than one neighboring county.

Table 8A. Columns (1)-(3) repeat the earlier specifications at the sub-county region level, without interactions between the sales tax rate and border dummies. As in Table 4A, none of the sales tax rate coefficients or their sums are significant. In columns (4)-(6) we add the border dummy-sales tax rate interactions. The estimated interaction coefficients capture the difference between the effects of sales tax rates on retail employment in border versus interior regions. The estimated interaction terms are small and insignificant, whether looking at individual coefficient estimates or summed current and lagged effects. Thus they indicate no detectable difference between border versus interior regions in the effect of sales tax rates on overall retail employment.

However when we turn to employment in big box/anchor stores, in Table 8B, we find pronounced differences between border versus interior regions. Columns (4)-(6) show the key results. In column (4), the effect of the current sales tax rate on big box/anchor store employment in interior regions is positive and significant at the 5% level (the 0.406 estimate); while in columns (5) and (6), the combined effect of the current and lagged sales tax rate is positive and significant at the 10% or 5% level (the estimates of 0.400 and 0.480). In contrast, the results for border regions show that an increase in the current and lagged sales tax rate causes big box/anchor employment to fall; the estimated effect in columns (4)-(6) is around −0.15 and significant at the 5% level (the estimates of −0.151, −0.169 and −0.149).

Thus, our results show that a one percentage-point increase in the sales tax rate leads big box/anchor store employment to rise by 40-48% in interior regions and to fall by 15-17% in border regions. The implied elasticities are around 3.0 and −1.1, respectively. The fact that our estimates in interior regions are positive is contrary to the general literature on the effects of sales taxes, which almost uniformly finds that they have negative effects on economic activity.[50] While our point estimates are perhaps implausibly large, we also found rather large point estimates when we did not use weights in the county-level analysis of big box/anchor employment (Table 4B). Thus, although our point estimates are high, we have confidence in our qualitative conclusions.

These estimates suggest that local officials concentrate their fiscal zoning efforts on attracting big

[50] See Wasylenko (1997) for a survey and Thompson and Rohlin (2012) for a recent contribution.

box stores and shopping centers and that their efforts are successful in interior regions, where there is little competition from across the border. But in border regions, fiscal zoning is either counter-productive or it cannot overcome the negative effect of tax-induced price increases in the presence of cross-border competition.[51,52]

Table 8C reports results of the border-interior analysis for manufacturing. Interestingly, the results are the opposite of those we found for retail employment at big box/anchor stores. Columns (4)-(6) show that the estimated interaction between the sales tax rate and the border dummy is positive and, for the current tax rate, significant. Computing the implied effects in border and interior regions, we find that an increase in the sales tax rate has a significant negative effect on manufacturing employment in interior regions and a positive effect in border regions. The estimated effects in border regions are never significant, but the differences between border and interior regions are strongly significant. Although this evidence is statistically weaker than the evidence for big box/anchor store employment, it is consistent with the existence of tradeoffs between employment in big box/anchor stores and employment in manufacturing. Thus, fiscal zoning leads to both increased big box/anchor store employment and reduced manufacturing employment in interior regions. These results presumably reflect the fact that land in interior regions is more valuable for generating sales tax revenue, since there is less competition from stores across the border.[53]

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[51] We also estimated the border-interior model for grocery stores as a falsification test. Again, the estimates pointed to no effect of a higher sales tax rate on grocery store employment in either interior or border regions. (Results available upon request.)

[52] We also explored whether these effects at the border depend on characteristics of the bordering county. We measured population density on each border, and included an interaction of density with the sales tax rate-border interaction, thus allowing the effect in border regions to vary with population density in neighboring counties. (Population density, which we measure by county and year, was defined in different ways: a dummy variable for density being in the top five percent of the distribution in the universe of border neighbors; the level of population density; and the log of population density). The evidence indicated that the negative effect of the sales tax rate on big box and anchor store retail sales in border areas was a bit *less* negative when the neighboring county was dense. If county residents were simply crossing the border to shop when their sales tax rose, one might expect more of this when the opposite county is dense because there is more retail. On the other hand, an increase in the sales tax rate might spur retail construction across the border by more when the neighboring county is less dense. Regardless, the main conclusions in Table 8B were unaffected by this extension of the specification.

[53] Endogeneity is less of a concern for the border-interior analysis, because these smaller regions clearly have less influence over sales tax rates. Consistent with this expectation, when we estimated the key specifications excluding the county time trends, the qualitative conclusions were unchanged, although the significance of some of the estimates changed.

The 1-mile width that we used to define border areas is somewhat arbitrary; our idea was to isolate areas very close to county borders where nearby residents would not regard cross-county travel as costly. In order to explore the sensitivity of our results to how we defined the borders, we re-estimated the same specifications as in Tables 8B and 8D, but redefined the border regions to be 2 miles wide. The resulting estimates were qualitatively similar, although statistically weaker.

Finally, we revisited the question of dynamics, using the one- and two-year first-difference specifications to estimate similar models for big box and anchor retail employment and for manufacturing employment, paralleling what we did in the county-level analysis in Table 6. The estimates were qualitatively similar to those in Tables 8B and 8C. For manufacturing, there is statistically significant evidence of negative effects in the county interior, again developing over a few years, although in this case (and the sample period is shorter) the contrast between the border and interior regions is not as clear. For big box/anchor store employment, there are positive effects in the county interior that evolve over three years (significant in the two-year first difference) and negative effects in border regions that similarly evolve over time.[54]

VIII. Conclusion

We have three main findings. First, total retail employment is not significantly affected by local sales tax rates. Second, however, local sales tax rates have a strong positive effect on employment in big box stores and department stores that anchor shopping centers. And third, local sales tax rates have a negative effect on manufacturing employment, although this evidence is weaker statistically.

Our results provide evidence that local officials engage in fiscal zoning and, more specifically, the evidence suggests that local officials concentrate their fiscal zoning efforts on attracting large stores and shopping centers. This is presumably because these stores generate high sales tax revenues, both directly and indirectly by attracting small stores. These stores also require large sites, which means that

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For this border-interior analyses, the results for FIRE did not hew as closely to those for manufacturing. (Results available upon request.) As a result, we are inclined to believe that the tradeoffs between land use for retail and manufacturing are sharper, perhaps because office employment can be concentrated in a fairly small geographic footprint, given the absence of either sales inventory in the retail sector or equipment and materials in the manufacturing sector, and more severe transportation demands in both sectors.

[54] Results are available upon request.

they need the types of zoning changes or other assistance that local officials can provide. In contrast, stores in neighborhood shopping centers and stores that occupy downtown or "main street" locations are less likely to require zoning changes or other assistance from local officials.

We also find that the effects of fiscal zoning on employment in big box/anchor stores differ substantially in border versus interior regions of counties. Fiscal zoning has strong positive effects in interior regions, but not in border regions. This may be because shoppers in interior regions are captive, making it easier for local officials to attract retail development despite higher sales tax rates. But in border regions, tax competition appears to be more important, making local officials' efforts less productive.

In some ways manufacturing employment provides a cleaner test of the effects of fiscal zoning, because local officials have little incentive to use zoning to attract manufacturing since it generates little sales tax revenue and because manufacturing does not have the contaminating direct price effect that pulls in the opposite direction. We therefore expect manufacturing employment to be lower in jurisdictions that have higher local sales tax rates, assuming that manufacturing and retailing are substitute land uses. Our results suggest that sales taxes in fact have the opposite effects on manufacturing employment as on big box/anchor store employment, so that local officials' efforts to attract shopping centers crowd out manufacturing.

There is more research that could be done to test the effects of fiscal zoning. Our test is indirect, since we examine the implications of our zoning model for employment levels, rather than examining the implications of the model for land use and related policy decisions. Information concerning land use, while very difficult to assemble, would be highly complementary to the type of evidence we present in this paper.

Our evidence feeds into a larger debate about the effects of using different types of taxes to fund local government. Over the past several decades, many states have moved to limit local governments' reliance on property taxes—their traditional funding source—and to substitute other sources of revenue, including local sales taxes. Florida is among the states that have done so.[55] But while property taxes give

[55] See Advisory Commission on Intergovernmental Relations (1995) for information on property tax limitation

local governments an incentive to encourage all non-residential land uses (because non-residential land uses do not increase the number of children in schools), local sales taxes give them an incentive to favor retail stores in particular. Our results provide empirical evidence that when sales tax rates are higher, local governments use fiscal zoning to encourage retailing and discourage manufacturing. Using the elasticities from the weighted estimates discussed above,[56] our basic results predict that a one percentage point increase in the local sales tax rate in a county leads to approximately 258 additional jobs in big box/anchor stores (plus possibly some additional employment in small stores), but approximately 838 fewer manufacturing jobs. Thus, our results suggest that fiscal zoning causes both a large substitution of jobs in retailing for jobs in manufacturing and a substitution of lower-wage jobs for higher-wage jobs.

Finally, it should be noted that if a local value-added tax were substituted for the local sales tax, the results for land use would be less distorting: local officials engaged in fiscal zoning would have incentives to zone additional land for non-residential uses, but would no longer have an incentive to favor retailing over manufacturing and services.

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measures in all U.S. states.

[56] See the discussion of Tables 4B and 4E.

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Table 1A: Number of Florida Counties Imposing Local Sales Taxes

Year	All Local Sales Taxes		Infrastructure or Small County Surtax		School Capital Outlay Tax		Indigent Care, Trauma Center, or Hospital Tax		Charter County Transit System Tax	
1992	35	(.87)	32	(.91)	0	(0)	2	(.50)	1	(.50)
1993	37	(.93)	34	(.97)	0	(0)	2	(.50)	1	(.50)
1994	42	(.94)	39	(.97)	0	(0)	2	(.50)	1	(.50)
1995	47	(.91)	44	(.94)	0	(0)	2	(.50)	1	(.50)
1996	50	(.96)	46	(.98)	3	(.42)	2	(.50)	1	(.50)
1997	51	(.95)	47	(.97)	4	(.44)	2	(.50)	1	(.50)
1998	50	(.98)	46	(.97)	7	(.45)	2	(.38)	1	(.50)
1999	52	(.95)	46	(.96)	8	(.50)	2	(.38)	1	(.50)
2000	50	(.97)	44	(.98)	8	(.50)	2	(.38)	1	(.50)
2001	50	(.97)	45	(.96)	8	(.50)	2	(.41)	1	(.50)
2002	51	(.97)	45	(.97)	9	(.50)	2	(.50)	1	(.50)
2003	54	(.97)	46	(.96)	13	(.50)	2	(.50)	2	(.50)
2004	54	(.96)	45	(.97)	13	(.50)	2	(.50)	2	(.50)
2005	58	(.94)	45	(.97)	16	(.50)	4	(.44)	2	(.50)
2006	58	(.94)	46	(.96)	16	(.50)	4	(.44)	2	(.50)

Notes: There are 67 counties in Florida. Figures in parentheses are the average local sales tax rate for counties that impose each local sales tax.
Sources: State of Florida Department of Revenue (2010), and Florida Legislative Committee on Intergovernmental Relations (2009).

Table 1B: Local Sales Tax Rate Changes in Florida, 1993-2006

Total number of counties changing their tax rates							
Year	Aggregate Local Sales Tax Rate	Infrastructure Tax or Small County Tax	School Capital Outlay Tax	Indigent Care, Trauma Center, or Hospital Tax	Charter County Transit System Tax	Total number of sales tax increases	Total number of sales tax decreases
1993	9	9	0	0	0	7	2
1994	7	7	0	0	0	7	0
1995	9	9	0	0	0	7	2
1996	8	6	3	0	0	8	0
1997	4	2	2	0	0	3	1
1998	6	2	4	1	0	3	3
1999	5	4	2	0	0	2	3
2000	2	2	0	0	0	0	2
2001	3	2	0	1	0	2	1
2002	5	3	1	1	0	4	1
2003	7	4	4	0	1	6	1
2004	3	1	2	0	0	1	2
2005	6	2	3	2	0	5	1
2006	1	1	0	0	0	1	0

Sources: State of Florida Department of Revenue (2010), and Florida Legislative Committee on Intergovernmental Relations (2009).

Table 2: Florida Local Sales Tax Rates and Changes, 1992-2006

County	1992 rate	Changes	County	1992 rate	Changes
Alachua	0	1% in 2002, 0 in 2003, .25% in 2005	Lee	0	
Baker	0	1% in 1994	Leon	1%	1.5% in 2003
Bay	0.50%	1% in 1994, .5% in 1995, 1% in 1998, .5% in 2004	Levy	0.25%	1% in 1993
Bradford	1%		Liberty	0	1% in 1993
Brevard	0		Madison	1%	
Broward	0		Manatee	1%	0 in 1993, .5% in 1994, 1% in 1995, .5% in 1999, 0 in 2000, .5 in 2003
Calhoun	0	1% in 1993	Marion	0	1% in 2003, .5% in 2005
Charlotte	0	.75% in 1995, 1% in 2006	Martin	0	.583 % in 1996, .417% in 1997, 0 in 1998, 1% in 1999, 0 in 2002
Citrus	0		Miami-Dade	0.50%	1% in 2003
Clay	1%		Monroe	1%	1.5% in 1996
Collier	0		Nassau	0	.5% in 1994, 0 in 1995, 1% in 1996
Columbia	0	1% in 1994	Okaloosa	0	.42% in 1995, 1% in 1996, .58% in 1999, 0 in 2000
Desoto	1%		Okeechobee	0	.25% in 1995, 1% in 1996
Dixie	1%		Orange	0	.5% in 2003
Duval	0.50%	1% in 2001	Osceola	1%	
Escambia	0.58%	1% in 1993,1.5% in 1998	Palm Beach	0	.5% in 2005
Flagler	1%		Pasco	0	1% in 2005
Franklin	0		Pinellas	1%	
Gadsden	1%		Polk	0	.5% in 2004, 1% in 2005
Gilchrist	0.25%	1% in 1993	Putnam	0	1% in 2003
Glades	1%		Saint Johns	0	
Gulf	0	.25% in 1997, .5% in 1998, 1% in 2006	Saint Lucie	0	.5% in 1996
Hamilton	1%		Santa Rosa	0	.33% in 1993, 1% in 1994, .79% in 1998, .5% in 1999
Hardee	1%		Sarasota	1%	
Hendry	1%		Seminole	1%	.75% in 2001, 1% in 2002
Hernando	0	.5% in 1999, 0 in 2004, .5% in 2005	Sumter	1%	
Highlands	1%		Suwannee	1%	
Hillsborough	0.50%	.94% in 1997, .75% in 1998, .81% in 2001, 1% in 2002	Taylor	1%	
Holmes	0	1% in 1995	Union	0	1% in 1993
Indian River	1%		Volusia	0	.5% in 2002
Jackson	0.5%	0 in 1993, .58% in 1995, 1.25% in 1996, 1.5% in 1997	Wakulla	1%	
Jefferson	1%		Walton	0	1% in 1995
Lafayette	1%		Washington	0	1% in 1994
Lake	1%				

Sources: State of Florida Department of Revenue (various years).

Table 3: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.
A. County-Level				
Sales tax rate, local plus state, current	6.70	0.46	6.0	7.5
Sales tax rate, local plus state, lagged	6.69	0.46	6.0	7.5
Neighboring sales tax rate, current	6.65	0.30	5.69	7.27
Neighboring local sales tax rate, lagged	6.64	0.31	5.66	7.27
Total retail share of employment	18.17%	4.41%	4.17%	29.30%
Big box/anchor retail share of employment	1.31%	1.10%	0.00%	6.91%
Manufacturing share of employment	9.08%	6.22%	1.55%	47.29%
Total retail employment	22,670	38,744	81	240,868
Big box/anchor retail employment	1,678	2,720	0	16,975
Manufacturing employment	9,913	18,805	25	111,510
B. Interior Regions				
Total retail employment share	18.88%	4.48%	2.49%	35.61%
Big box/anchor retail employment share	1.36%	1.29%	0.00%	9.31%
Manufacturing employment share	8.47%	6.43%	0.73%	50.98%
Total retail employment	19,603	35,197	16	219,529
Big box/anchor retail employment	1,472	2,504	0	14,928
Manufacturing employment	8,563	17,410	10	109,190
C. Border Regions				
Neighboring sales tax rate, current	6.62	0.524	5.0	7.5
Neighboring sales tax rate, lagged	6.61	0.526	5.0	7.5
Total retail employment share	19.05%	16.67%	0.03%	100.00%
Big box/anchor retail employment share	0.42%	1.51%	0.00%	14.66%
Manufacturing employment share	7.17%	15.65%	0.00%	99.03%
Total retail employment	524	1,647	0	16,002
Big box/anchor retail employment	35	153	0	2,600
Manufacturing employment	187	664	0	9,194

Notes: In Panel A, there are 1,005 county-year observations (15 years of data for 67 counties) on the contemporaneous variables, and 938 county-year observations (14 years of data for 67 counties) on the lagged variables. In Panel B, there are 1,005 county interior-year observations (15 years of data for 67 county interior regions). In Panel C, there are 2,863 locality-year observations with positive overall employment in the border regions. Border regions extend one mile inward from county borders; their construction is explained in Section VII.

Table 4A: Regression Results Explaining Total Retail Employment, County-Level Analysis

Explanatory variables	(1)	(1')	(2)	(2')	(3)	(3')
	Unwgt.	Wgt.	Unwgt.	Wgt.	Unwgt.	Wgt.
Sales tax rate, current	0.016 (0.017)	0.002 (0.016)	-0.014 (0.015)	-0.017 (0.012)	-0.013 (0.020)	-0.005 (0.013)
Sales tax rate, lagged	-	-	0.030 (0.021)	0.019 (0.013)	0.028 (0.021)	0.014 (0.012)
Neighboring sales tax rate, current	-	-	-	-	-0.010 (0.056)	-0.059** (0.020)
Neighboring sales tax rate, lagged	-	-	-	-	0.011 (0.023)	0.023* (0.014)
Effect of a unit increase in current plus lagged sales tax rate			0.016 (0.024)	0.002 (0.019)	0.015 (0.023)	0.009 (0.020)
R^2	0.99	0.99	0.99	0.99	0.99	0.99

Notes: The dependent variable is the log of total retail employment. The sample period covers 1992-2006. There are 1,005 observations for the contemporaneous specifications, and 938 observations for the specifications with lags. The sales tax rate variable is the sum of the local sales tax plus the 6% general Florida sales tax, measured in units of 0-100. All specifications include county and year fixed effects, and county-time trend interactions. Standard errors are clustered at the county level. 1992 population levels are used as weights in the columns labeled "Wgt." ** indicates statistical significance at the 5% level, and * at the 10% level.

Table 4B: Regression Results Explaining Retail Employment at Big Box and Anchor Stores, County-Level Analysis

	(1)	(1')	(2)	(2')	(3)	(3')
Explanatory variables	Unwgt.	Wgt.	Unwgt.	Wgt.	Unwgt.	Wgt.
Sales tax rate, current	0.208* (0.113)	0.134** (0.056)	0.047 (0.090)	0.080 (0.049)	-0.004 (0.177)	0.074 (0.053)
Sales tax rate, lagged	-	-	0.256 (0.224)	0.085* (0.044)	0.313 (0.233)	0.076 (0.048)
Neighboring sales tax rate, current	-	-	-	-	0.293 (0.534)	0.029 (0.102)
Neighboring sales tax rate, lagged	-	-	-	-	-0.288 (0.225)	0.033 (0.072)
Effect of a unit increase in current plus lagged local sales tax rate			0.303 (0.192)	0.166** (0.064)	0.309** (0.135)	0.150** (0.063)
R^2	0.98	0.98	0.98	0.98	0.98	0.99

Notes: The dependent variable is the log of the sum of big box and anchor store employment. See the text for a list of stores. Other notes to Table 4A apply here.

Table 4C: Regression Results Explaining Total Retail Employment Excluding Big Box and Anchor Store Retail Employment, County-Level Analysis

Explanatory variables	(1)	(1')	(2)	(2')	(3)	(3')
	Unwgt.	Wgt.	Unwgt.	Wgt.	Unwgt.	Wgt.
Sales tax rate, current	-0.0006 (0.014)	-0.008 (0.014)	-0.022 (0.014)	-0.025** (0.011)	-0.016 (0.015)	-0.012 (0.011)
Sales tax rate, lagged	-	-	0.014 (0.014)	0.015 (0.014)	0.011 (0.015)	0.010 (0.012)
Neighboring sales tax rate, current	-	-	-	-	-0.032 (0.032)	-0.064** (0.019)
Neighboring sales tax rate, lagged	-	-	-	-	0.022 (0.021)	0.022* (0.013)
Effect of a unit increase in current plus lagged sales tax rate			-0.008 (0.018)	-0.010 (0.018)	-0.005 (0.019)	-0.002 (0.019)
R^2	0.99	0.99	0.99	0.99	0.99	0.99

Notes: The dependent variable is the log of total retail employment excluding big box and anchor store retail employment. Other notes to Table 4A apply here.

Table 4D: Regression Results Explaining Retail Employment at Regular Grocery Stores, Falsification Test, County-Level Analysis

Explanatory variables	(1)	(1')	(2)	(2')	(3)	(3')
	Unwgt.	Wgt.	Unwgt.	Wgt.	Unwgt.	Wgt.
Sales tax rate, current	0.011 (0.032)	0.015 (0.029)	-0.050 (0.031)	-0.009 (0.020)	-0.053 (0.038)	-0.005 (0.021)
Sales tax rate, lagged	-	-	0.044 (0.034)	0.015 (0.016)	0.028 (0.034)	0.015 (0.018)
Neighboring sales tax rate, current	-	-	-	-	-0.004 (0.085)	-0.020 (0.048)
Neighboring sales tax rate, lagged	-	-	-	-	0.058 (0.045)	0.001 (0.048)
Effect of a unit increase in current plus lagged sales tax rate			-0.006 (0.034)	0.005 (0.029)	-0.025 (0.034)	0.009 (0.027)
R^2	0.99	0.997	0.996	0.997	0.996	0.997

Notes: The dependent variable is the log of retail employment at grocery stores; Wal-Mart and Costco were excluded because they are classified as grocery stores but sell many non-grocery items that are taxed. Other notes to Table 4A apply here.

Table 4E: Regression Results Explaining Manufacturing Employment, County-Level Analysis

Explanatory variables	(1) Unwgt.	(1') Wgt.	(2) Unwgt.	(2') Wgt.	(3) Unwgt.	(3') Wgt.	(4) Unwgt.	(4') Wgt.
Sales tax rate, current	-0.037 (0.041)	-0.023 (0.029)	0.025 (0.032)	0.008 (0.023)	0.034 (0.042)	-0.003 (0.025)	-	-
Sales tax rate, lagged	-	-	-0.084* (0.046)	-0.051** (0.024)	-0.102** (0.048)	-0.068** (0.026)	-0.089* (0.052)	-0.070** (0.033)
Neighboring sales tax rate, current	-	-	-	-	-0.060 (0.111)	0.043 (0.041)	-0.037 (0.098)	0.041 (0.036)
Neighboring sales tax rate, lagged	-	-	-	-	0.081 (0.079)	0.065 (0.045)	0.075 (0.082)	0.066 (0.047)
Effect of a unit increase in current plus lagged local sales tax rate			-0.059 (0.058)	-0.043 (0.037)	-0.068 (0.068)	-0.071* (0.042)	-	-
R-Squared	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99

Notes: The dependent variable is the log of manufacturing employment. Other notes to Table 4A apply here.

Table 4F: Regression Results Explaining FIRE Employment, County-Level Analysis

Explanatory variables	(1) Unwgt.	(1') Wgt.	(2) Unwgt.	(2') Wgt.	(3) Unwgt.	(3') Wgt.
Sales tax rate, current	-0.052* (0.032)	-0.035* (0.019)	-0.033** (0.016)	-0.033** (0.014)	-0.029 (0.021)	-0.029 (0.018)
Sales tax rate, lagged	-	-	-0.041 (0.036)	-0.021 (0.023)	-0.048 (0.039)	-0.026 (0.024)
Neighboring sales tax rate, current	-	-	-	-	-0.027 (0.060)	-0.021 (0.037)
Neighboring sales tax rate, lagged	-	-	-	-	0.030 (0.055)	0.019 (0.037)
Effect of a unit increase in current plus lagged local sales tax rate			-0.075* (0.044)	-0.054* (0.029)	-0.077 (0.052)	-0.055* (0.033)
R-Squared	0.997	0.998	0.997	0.999	0.997	0.999

Notes: The dependent variable is the log of employment in finance, insurance, and real estate (FIRE). Other notes to Table 4A apply here.

Table 5A: Regression Results Explaining Big Box and Anchor Store Retail Employment and Manufacturing Employment with Enterprise Zone Dummies, County-Level Analysis, 1996-2006

	Big box and anchor store retail				Manufacturing			
Explanatory variables	(1) Unwgt.	(1') Unwgt.	(2) Wgt.	(2') Wgt.	(3) Unwgt.	(3') Unwgt.	(4) Wgt.	(4') Wgt.
Sales tax rate, current	0.156 (0.099)	0.154 (0.099)	0.078 (0.052)	0.077 (0.051)	-	-	-	-
Sales tax rate, lagged	0.014 (0.052)	0.015 (0.052)	-0.008 (0.037)	-0.010 (0.037)	-0.091 (0.057)	-0.092 (0.057)	-0.055 (0.0349)	-0.051 (0.033)
Neighboring sales tax rate, current	-0.051 (0.138)	-0.051 (0.139)	-0.039 (0.093)	-0.040 (0.093)	-0.003 (0.145)	-0.005 (0.144)	-0.029 (0.044)	-0.026 (0.045)
Neighboring sales tax rate, lagged	-0.064 (0.102)	-0.062 (0.100)	-0.028 (0.063)	-0.028 (0.063)	-0.009 (0.114)	-0.007 (0.114)	0.008 (0.056)	0.008 (0.056)
Enterprise Zone	-	0.048 (0.150)	-	-0.030 (0.034)	-	0.042 (0.036)	-	0.034** (0.014)
Effect of a unit increase in current plus lagged local sales tax rate	0.171* (0.104)	0.169 (0.104)	0.071 (0.066)	0.066 (0.064)	-	-	-	-

Notes: The columns without primes report specifications like those in Tables 4B and 4D, excluding the enterprise zone control, for the subperiod for which enterprise zone information is available, accounting for the inclusion of the lagged sales tax variable (1996-2006). The columns with primes include the enterprise zone control, which is equal to one if there was an enterprise zone in effect in the county and year. There are 737 observations. All specifications include county and year fixed effects, and county-time trend interactions. Standard errors are clustered at the county level. 1992 population levels are used as weights in the columns labeled "Wgt." Other notes to Table 4A apply here.

Table 5B: Regression Results Explaining Big Box and Anchor Store Retail Employment and Manufacturing Employment with Community Redevelopment Area Dummies, County-Level Analysis, 1992-2006

	Big box and anchor store retail		Manufacturing	
Explanatory variables	(1) Unwgt.	(2) Wgt.	(3) Unwgt.	(4) Wgt.
Sales tax rate, current	-0.011 (0.175)	0.072 (0.052)	-	-
Sales tax rate, lagged	0.306 (0.233)	0.080 (0.049)	-0.086* (0.051)	-0.072** (0.033)
Neighboring sales tax rate, current	0.287 (0.537)	0.021 (0.103)	-0.033 (0.098)	0.049 (0.037)
Neighboring sales tax rate, lagged	-0.287 (0.223)	0.032 (0.072)	0.074 (0.081)	0.066 (0.047)
Community Redevelopment Area	-0.078 (0.065)	-0.010 (0.010)	0.028 (0.024)	0.009 (0.007)
Effect of a unit increase in current plus lagged local sales tax rate	0.296** (0.130)	0.152** (0.065)	-	-

Notes: The specifications correspond to columns (3) and (3') from Table 4B and columns (4) and (4') from Table 4E. The only difference is the addition of a dummy variable for the presence of a Community Redevelopment Area in the county-year cell. Details from notes to those tables apply here.

Table 5C: Regression Results Exploring Pre-Treatment Trends in Determination of Changes in Sales Tax Rate, County-Level Analysis

Explanatory variables	All Retailing		BB/anchor store		Manufacturing	
	(1)	(1')	(2)	(2')	(3)	(3')
	Unwgt.	Wgt.	Unwgt.	Wgt.	Unwgt.	Wgt.
Δlog employment, lagged one year	0.003 (0.073)	0.157 (0.351)	0.009 (0.008)	0.039 (0.025)	0.007 (0.019)	0.080 (0.090)
Δlog employment, lagged two years	0.012 (0.093)	-0.224 (0.245)	0.004 (0.005)	-0.021 (0.020)	-0.017 (0.017)	-0.101 (0.067)
Δlog employment, lagged three years	-0.036 (0.073)	0.018 (0.209)	0.001 (0.003)	0.013 (0.022)	0.021 (0.022)	0.118 (0.092)
Sum of lagged changes in log employment	-0.021 (0.142)	-0.048 (0.407)	0.015 (0.011)	0.031 (0.040)	0.011 (0.042)	-0.062 (0.153)
R^2	0.07	0.10	0.07	0.10	0.07	0.10

Notes: The dependent variable is the change in the local sales tax rate. There are 737 county-year observations. All specifications include year fixed effects and county-time trend interactions. Standard errors are clustered at the county level. 1992 population levels are used as weights in columns (1'), (2'), and (3').

Table 6: Regression Results Explaining Changes in Big Box and Anchor Store Retail Employment and Manufacturing Employment, First Difference and Variable Lag Specifications

	Retail big box and anchor stores				Manufacturing			
	One-year first differences		Two-year first differences		One-year first differences		Two-year first differences	
	(1)	(1')	(2)	(2')	(3)	(3')	(4)	(4')
Explanatory variables, first differences	Unwgt.	Wgt.	Unwgt.	Wgt.	Unwgt.	Wgt.	Unwgt.	Wgt.
A. Specification with current and lagged sales tax rates								
Sales tax rate, current	0.053 (0.081)	0.061 (0.043)	0.082 (0.115)	0.130** (0.050)	-	-	-	-
Sales tax rate, lagged one year	0.242 (0.246)	0.030 (0.040)	0.073 (0.178)	-0.030 (0.037)	-0.057* (0.030)	-0.048** (0.023)	-0.056 (0.037)	-0.022 (0.025)
Sales tax rate, lagged two years	0.011 (0.060)	0.025 (0.031)	0.176 (0.109)	0.109** (0.044)	-0.034 (0.027)	-0.001 (0.015)	-0.052 (0.033)	-0.036* (0.022)
Sales tax rate, lagged three years	-	-	-	-	-0.047* (0.026)	-0.027* (0.017)	-0.051* (0.031)	-0.001 (0.027)
Sum of sales tax coefficients (cumulative effect)	0.306 (0.269)	0.116* (0.067)	0.331* (0.169)	0.209** (0.068)	-0.139** (0.065)	-0.077** (0.035)	-0.159** (0.071)	-0.059* (0.034)
R^2	0.076	0.102	0.127	0.167	0.088	0.143	0.164	0.232
N	804	804	737	737	737	737	670	670
B. Subsample with leads								
Sales tax rate, one- or two- year lead	0.121** (0.060)	0.079** (0.031)	0.174 (0.119)	0.059 (0.043)	-0.025 (0.027)	-0.040* (0.024)	-0.036 (0.031)	-0.034 (0.028)
Sum of current and lagged sales tax coefficients, specification including lead	0.369 (0.295)	0.176** (0.068)	0.304* (0.167)	0.239** (0.074)	-0.142** (0.063)	-0.094** (0.037)	-0.188** (0.074)	-0.105** (0.044)
Sum of lead, current, and lagged sales tax coefficients, specification including lead	0.489 (0.306)	0.255** (0.079)	0.478* (0.249)	0.298** (0.090)	-0.166** (0.070)	-0.134** (0.051)	-0.224** (0.086)	-0.139** (0.062)
Sum of current and lagged sales tax coefficients, specification excluding lead	0.335 (0.294)	0.146** (0.071)	0.351* (0.185)	0.254** (0.076)	-0.136** (0.063)	-0.078** (0.034)	-0.173** (0.074)	-0.083** (0.039)
N	737	737	670	670	670	670	603	603

Notes: Aside from the additional lags and using short first differences rather than within-group estimation, notes from Tables, 4A, 4B, and 4D apply here. The first differences of the explanatory variables are computed over one year or two years, as indicated in the column heading. All specifications include county and year fixed effects; the county-trend interactions drop out because of the differencing. In Panel B, the one-year or two-year first-differenced lead is included, corresponding to the specification. Standard errors are clustered at the county level. 1992 population levels are used as weights in the columns labeled "Wgt."

Table 7: Regression Results Explaining Changes in Retail Employment at New Big Box and Anchor Stores, County-Level Analysis

	Retail big box and anchor stores			
	Change in employment at new stores, one-year window		Change in employment at new stores, two-year window	
Explanatory variables, first differences	(1)	(1')	(2)	(2')
	Unwgt.	Wgt.	Unwgt.	Wgt.
Sales tax rate, current	68.16** (34.10)	99.79 (69.21)	48.15* (27.90)	67.159 (66.07)
Sales tax rate, lagged one year	64.25** (29.00)	95.00 (57.11)	18.59 (20.88)	45.00 (73.46)
Sales tax rate, lagged two years	23.68 (20.51)	-34.32 (133.14)	41.63** (18.17)	57.87 (64.18)
Sum of sales tax coefficients (cumulative effect)	156.09*** (54.84)	160.47 (134.64)	108.38*** (38.86)	170.04** (82.78)
R^2	0.527	0.591	0.510	0.544
N	804	804	737	737

Notes: The dependent variable is employment created by births or move-ins of stores. For the one-year window, births or move-ins are identified as stores that had zero employment in the county in period $t-1$, but positive employment in the county in period t. The change in employment is then employment in period t, which measures the employment created by the birth or move-in. (Almost all of the variation is from births.) For the two-year window, new establishments are identified between periods $t-2$ and t. The unweighted means are 90.92 for the one-year window, and 87.35 for the two-year window. The first differences of the explanatory variables are computed over one year in columns (1) and (1') and over two years in columns (2) and (2'). All specifications include county and year fixed effects; the county-trend interactions drop out because of the differencing. Standard errors are clustered at the county level. 1992 population levels are used as weights in the columns labeled "Wgt." Notes from Tables 4A, 4B, 4D, and 6 apply here.

Table 8A: Regression Results Explaining Total Retail Employment, Border-Interior Analysis

Explanatory variables	(1)	(2)	(3)	(4)	(5)	(6)
Sales tax rate, current	-0.033 (0.049)	-0.046 (0.047)	-0.037 (0.056)	0.020 (0.078)	-0.042 (0.061)	-0.013 (0.093)
Sales tax rate, lagged	-	0.024 (0.047)	0.035 (0.057)	-	0.034 (0.064)	0.061 (0.094)
Sales tax rate, current, x border dummy	-	-	-	-0.069 (0.093)	-0.005 (0.078)	-0.029 (0.089)
Sales tax rate, lagged, x border dummy	-	-	-	-	-0.012 (0.082)	-0.031 (0.094)
Neighboring sales tax rate, current	-	-	-0.026 (0.060)	-	-	-0.030 (0.066)
Neighboring sales tax rate, lagged	-	-	-0.027 (0.060)	-	-	-0.032 (0.066)
Effect of a unit increase in current plus lagged sales tax rate (in columns (2') and (3'), results are for interior regions)		-0.021 (0.069)	0.002 (0.084)	-	-0.009 (0.096)	0.048 (0.133)
Effect of a unit increase in current sales tax rate on employment in border regions (main effect plus interaction)				-0.049 (0.057)	-	-
Effect of a unit increase in current plus lagged sales tax rate on employment in border regions (main effects plus interactions)					-0.025 (0.080)	-0.012 (0.086)
Difference in effect of current plus lagged sales tax rate between border and interior regions (sum of border interactions)					-0.017 (0.111)	-0.060 (0.122)
R^2	0.99	0.99	0.99	0.99	0.99	0.99

The dependent variable is the log of total retail employment. There are 4,155 observations for the contemporaneous specifications, and 3,878 observations for the specifications with lags. The sales tax rate variable is the sum of the local sales tax plus the 6% general Florida sales tax, measured in units of 0-100. The classification of border and interior regions is based on 1-mile border zones. All regressions include fixed effects for each sub-county area (each unique border area and county interior), year fixed effects, and county-time trend interactions. Standard errors are clustered at the sub-county region level. Estimates are not weighted.

Table 8B: Regression Results Explaining Big Box and Anchor Store Retail Employment, Border-Interior Analysis

Explanatory variables	(1)	(2)	(3)	(4)	(5)	(6)
Sales tax rate, current	-0.022 (0.058)	-0.030 (0.042)	-0.024 (0.037)	0.406** (0.179)	-0.011 (0.150)	0.017 (0.153)
Sales tax rate, lagged	-	-0.008 (0.070)	-0.020 (0.064)	-	0.410 (0.261)	0.463* (0.270)
Sales tax rate, current, x border dummy	-	-	-	-0.557*** (0.189)	-0.023 (0.165)	-0.045 (0.169)
Sales tax rate, lagged, x border dummy	-	-	-	-	-0.545** (0.269)	-0.585** (0.275)
Neighboring sales tax rate, current	-	-	-0.027 (0.053)	-	-	-0.029 (0.047)
Neighboring sales tax rate, lagged	-	-	0.040 (0.062)	-	-	-0.060 (0.054)
Effect of a unit increase in current plus lagged sales tax rate (interior in columns (2') and (3'))		-0.039 (0.080)	-0.044 (0.078)	-	0.400* (0.226)	0.480** (0.240)
Effect of a unit increase in current sales tax rate on employment in border regions (main effect plus interaction)				-0.151*** (0.056)	-	-
Effect of a unit increase in current plus lagged sales tax rate on employment in border regions (main effects plus interactions)					-0.169** (0.070)	-0.149** (0.069)
Difference in effect of current plus lagged sales tax rate between border and interior regions (sum of border interactions)					-0.568** (0.226)	-0.629*** (0.237)
R^2	0.96	0.96	0.96	0.96	0.96	0.96

The dependent variable is the log of the sum of big box and anchor store retail employment. All regressions include fixed effects for each sub-county area (each unique border area and county interior), year fixed effects, and county-time trend interactions. Standard errors are clustered at the sub-county region level. Estimates are not weighted. Other details from notes to Table 8A apply here.

Table 8C: Regression Results Explaining Manufacturing Employment, Border-Interior Analysis

Explanatory variables	(1)	(2)	(3)	(4)	(5)	(6)
Sales tax rate, current	0.073 (0.078)	0.080 (0.075)	0.066 (0.080)	-0.185 (0.123)	-0.154 (0.099)	-0.270** (0.134)
Sales tax rate, lagged	-	-0.030 (0.063)	-0.032 (0.071)	-	-0.103 (0.106)	-0.104 (0.144)
Sales tax rate, current, x border dummy	-	-	-	0.335** (0.157)	0.302** (0.134)	0.407** (0.163)
Sales tax rate, lagged, x border dummy	-	-	-	-	0.095 (0.136)	0.081 (0.163)
Neighboring sales tax rate, current	-	-	0.052 (0.105)	-	-	0.119 (0.117)
Neighboring sales tax rate, lagged	-	-	-0.005 (0.065)	-	-	0.007 (0.074)
Effect of a unit increase in current plus lagged sales tax rate (interior in columns (2') and (3'))		0.050 (0.107)	0.033 (0.118)	-	-0.257* (0.154)	-0.374** (0.174)
Effect of a unit increase in current sales tax rate on employment in border regions (main effect plus interaction)				0.150 (0.095)	-	-
Effect of a unit increase in current plus lagged sales tax rate on employment in border regions (main effects plus interactions)					0.140 (0.127)	0.114 (0.131)
Difference in effect of current plus lagged sales tax rate between border and interior regions (sum of border interactions)					0.397** (0.189)	0.489** (0.196)
R^2	0.97	0.97	0.97	0.97	0.97	0.97

The dependent variable is the log of manufacturing employment. All regressions include fixed effects for each sub-county area (each unique border area and county interior), year fixed effects, and county-time trend interactions. Standard errors are clustered at the sub-county region level. Estimates are not weighted. Other details from notes to Table 8A apply here.