Development impact fees and employment

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Development impact fees and employment

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ABSTRACT

Development impact fees have sparked considerable controversy as they have spread rapidly in usage throughout the United States. One contentious issue is the effect that these fees have on local economic development. While some scholars have argued that impact fees attract jobs by reducing developers' uncertainty, the development community maintains that they operate as an excise tax, reducing commercial development and driving away jobs. We use Florida county level panel data, from 1990–2005, to investigate the relationship between private employment and different types of impact fees. We find that commercial fees and school fees have countervailing effects, with the former repelling jobs and the latter attracting jobs. These results are consistent with our theory driven expectations. Our investigation also suggests that differences between our results and those obtained in prior studies can be attributed to two factors: the latter studies' violation of the condition of strict exogeneity required for consistent estimation and a failure to account for differential employment effects across various types of impact fees.

1. Introduction

Increasingly throughout the United States property tax revenues are insufficient to fund public infrastructure expansions necessitated by new development. Because raising property tax rates has politically become increasingly difficult, many local governments have chosen to address revenue shortfalls by adopting various types of development impact fees over the past few decades.[2] Impact fees are one-time levies, predetermined through a formula adopted by a local government unit, that are assessed on a property developer during the permit approval process. They are earmarked for specific public services, bridging the gap between the cost of infrastructure expansions and revenue streams that will help pay for them. The services covered vary from jurisdiction to jurisdiction, but routinely include road, water, and sewer. Other services less frequently included are schools, libraries, police, fire and parks.[3] For each service there is a separate fee schedule, and developments pay fees only for services they directly consume (e.g., commercial developments are not included under a school impact fee schedule).

Many controversial issues, including concerns over how impact fees affect the availability and affordability of housing, surround the use of impact fees.[4] However, from the point of view of local governments considering implementing fees, perhaps the most important issue is the effect that the fees have on local economic development. Critics, who come mainly from the development community, argue impact fees are an excise tax on development, driving investment and job growth to other jurisdictions where fees are lower or do not exist.[5] Proponents of fees make the case that they encourage development by decreasing developers' uncertainty surrounding two key elements affecting the profitability of their projects (Nelson et al., 1992; Nelson and Moody, 2003). First, impact fees may expedite project approval. The project approval process can be long and expensive to the developer. In the absence of development fees, funding for new public infrastructure typically comes from the property tax. Hence, depending on the magnitudes of the services

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[2] Altshuler and Gomez-Ibanez (1993) discuss why property tax increases have become increasingly unpopular. Based upon a nation-wide survey of local governments, Lawhorn (2003) concludes that roughly a quarter of local governments levied impact fees in 2002. Usage rates are considerably higher in areas that have experienced intense population growth, such as Florida and California.

[3] In Florida, the typical county first adopted water/sewer fees during the late 1970s or 1980s. Road fees, usually small in size, were typically added in the late 1980s. Once the legality of impact fees was established through a number of court cases, most impact fee programs expanded in both size (fee levels) and scope (services covered). See Burge and Ihlanfeldt (2007) for a discussion of these court cases.

[4] Most studies on the effects of impact fees have focused on the issue of incidence (i.e., to what extent are fees shifted forward to home buyers in the form of higher housing prices). For a review of this literature see Ihlanfeldt and Shaughnessy (2004). There is also a smaller literature that relates impact fees to housing construction. These studies are reviewed in Burge and Ihlanfeldt (2006b).

[5] See, for example, the internet pages of the Urban Land Institute (http://www.uli.org), the National Association of Realtors (http://www.realtor.org), and the National Association of Home Builders (http://www.nahb.org).

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required and the tax revenues generated by the new development, existing property owners may face higher property tax burdens when growth occurs. Impact fees reduce or eliminate this risk, presumably making local government more willing to approve new development, and to do so expeditiously. Second, because impact fees are earmarked, they may reduce the uncertainty that developers/employers have over whether the infrastructure they need will be provided by local government or provided in a timely fashion. Developers may view impact fees as a contractual agreement with local government that gives them some assurance that the infrastructure services they need will be provided.

Only two studies (having much in common, as discussed more fully below) have empirically investigated the above issue, focusing on the effect that impact fees have on the local jurisdiction's private sector employment growth (Nelson and Moody, 2003; Jeong and Feiock, 2006). Both studies conclude that impact fees increase the number of jobs within the jurisdiction and attribute the employment growth to a reduction in developer uncertainty. However, neither study adequately deals with the concern that impact fees are endogenous to employment growth. Even casual observation suggests that fees are more likely within those jurisdictions where strong growth has created a deficit in their stock of public capital. The positive relationship observed by these studies between fees and employment growth may therefore be the result of reverse causation.

The purpose of this paper is to present the results obtained from estimating panel data models that relate private sector job growth to three types of impact fees: commercial, school, and water/sewer. We exploit the panel nature of our data to control for potential endogeneity and multiple sources of unobserved heterogeneity. In making these improvements, we find that higher commercial fees reduce employment, while the opposite is true for school fees. Water/sewer fees are not found to have a significant effect in either direction. The negative effect that commercial impact fees have on employment suggests that these fees impose costs on developers that exceed any benefits that they may accrue from reduced uncertainty. Our finding that school fees increase employment is consistent with our earlier work showing that residential impact fees stimulate the construction of both single-family and multi-family housing construction (Burge and Ihlanfeldt, 2006a,b). More homes mean more people, which bring benefits to commercial developers/employers in the form of greater customer demand and labor supply. In addition, commercial developers bear no costs from school fees, because they are exempt from paying them.

2. Literature review

Two previous studies have empirically examined the relationship between impact fees and employment levels. Both investigations, as well as the current study, use panel data at the county level from the state of Florida. Nelson and Moody (2003) explain the Florida advantage:

> Florida is also an appropriate state to examine since it has arguably the most extensive history of applying rational nexus-style development impact fees and therefore the most likely to reveal an observable cause and effect relationship between impact fees and tangible economic benefits.

Nelson and Moody's key data item is annual impact fee collections for each of Florida's 67 counties covering the years 1993–1999. They regress the two-year change in jobs (E_t-E_{t-2}) on impact fees collected by each county between the base year ($t-2$) and the previous year ($t-3$) divided by the total number of building permits issued over the same time period. Their control variables include base year employment change ($E_{t-2}-E_{t-3}$), prior decade employment change ($E_{1990}-E_{1980}$), per capita property taxes collected between the base year and the previous year ($T_{t-2}-T_{t-3}$), along with year and region fixed effects. The impact fee variable is positive and statistically significant.

Jeong and Feiock's (2006) panel covers the years 1991 to 2000. Their dependent variable is the two-year change in employment per 1000 population ($(E/P)_t-(E/P)_{t-2}$) in each county. Their impact fee variable is a dummy variable indicating whether the county had a fee in year ($t-2$). Their control variables are more extensive than those employed by Nelson and Moody, but only four are statistically significant: form of government (council–manager cities generate more jobs than mayor–council cities), population change (P_t-P_{t-2}), per capital state job growth (J_t-J_{t-2}) and lagged county employment (E_{t-3}). Although it would have been feasible given the panel nature of their data, neither time nor area fixed effects are included in any of their estimated models. They also find the impact fee variable to be positive and statistically significant.

Although pioneering, these studies suffer from two serious limitations. First, both fee collections per building permit and a fee existence indicator variable crudely measure impact fees. The correct measure of commercial impact fees is what developers must pay per standardized area unit of commercial building space.[6] Prior studies also mismeasure impact fees by lumping together residential and commercial impact fees into a single variable.[7] As we argue below, employment is expected to respond differently to each type of fee. Secondly, both studies fail to adequately deal with the endogeneity of fees (as well as many of the control variables). In the use of panel data, the strict exogeneity of the regressors is required to obtain consistent estimates (Wooldridge, 2002, p. 254). Strict exogeneity implies that explanatory variables in each time period (X_{it}) are uncorrelated with the idiosyncratic error (ε_{it}) in each time period: $E(X_{is}^{l}\varepsilon_{it})=0$, s, $t=1,\ldots, T$. This assumption is much stronger than assuming zero contemporaneous correlation: $E(X_{it}^{l}\varepsilon_{it})=0$, $t=1,\ldots, T$. Strict exogeneity is violated if current values of the dependent variable affect current or *future* values of the explanatory variables. Employment growth (the dependent variable in prior studies) experienced in previous periods is likely to influence both whether an impact fee program exists in future years (Jeong and Feiock) and the level of future impact fee collections (Nelson and Moody). Hence, strict exogeneity may have been violated in prior studies, potentially accounting for the positive correlation found between employment change and impact fees. We later use our panel data to demonstrate that models akin to those estimated in prior studies do yield the finding that both commercial and school impact fees have a positive effect on employment. However, we also demonstrate that these models clearly violate strict exogeneity. To improve upon prior studies, we estimate first differenced models that are well grounded theoretically, allow for employment to respond to changes in fees using appropriate lag structures, and satisfy the strict exogeneity condition required for consistent estimation.

3. Theoretical framework

We investigate the relationship between county employment and three distinct categories of impact fees: commercial fees (CF), water/sewer utility fees (UF), and school fees (SF). Our theoretical framework is built upon two common characteristics of these fees: 1) they provide a "bounty" that counties receive from allowing new development, which on the margin may increase the likelihood that

[6] The dominant practice is to charge a set impact fee rate per 1000 interior square feet of development. Our construction of the commercial impact fee variable is discussed in detail in Section 4.

[7] Both studies also fail to account for water/sewer impact fees. No explanation is given in either study for why these fees are ignored in the analysis. The importance of this oversight is magnified by the fact that in Florida water/sewer impact fees are the most heavily used type of impact fees. See Table 1.

proposed projects will be approved, and 2) they are earmarked for specific types of public infrastructure.[8] CF are for transportation improvements and other infrastructure supporting services (like police and fire) that are consumed by businesses; UF are for off-site water/sewer infrastructure improvements; and SF are for new schools.

Consider first CF. Assume that developers construct commercial space which they then rent to employers. In the absence of CF, property taxes fund the infrastructure required to provide public services to new development. Since a portion of the costs of this infrastructure is funded by CF, expected future property tax payments decline.[9] As a result, the switch from property tax finance to commercial impact fees may change the fiscal burden (F_{CF}) on commercial developers associated with CF:

$$\Delta F_{CF} = CF - (PVT_{NCF} - PVT_{CF}) \tag{1}$$

where PVT_{NCF} and PVT_{CF} are the present values of expected future property tax payments without and with CF in place, respectively. If the CF are greater (less) than the present value of the savings in property taxes, F_{CF} rises (falls) and repels (stimulates) commercial development, ceteris paribus.[10]

However, in addition to the fiscal effect of CF, there is also the uncertainty effect. As discussed above, CF may reduce developers' uncertainty over whether the local government, who has local monopoly control over land use decisions, will approve their projects and provide needed infrastructure in a timely fashion. If we assume ΔF_{CF} is positive, we have the following proposition: if the dollar benefit equivalent to the reduction in uncertainty is greater (less) than ΔF_{CF}, CF would be expected to stimulate (reduce) commercial development. More commercial development increases the supply of commercial space, which reduces its price. Because labor and commercial space are complementary inputs, the cheaper commercial space expands the county's equilibrium level of employment. Conversely, less commercial space reduces employment.

Next consider UF. The theory for UF is similar to that presented above for CF. In the absence of UF, the costs of off-site water/sewer system infrastructure improvements necessitated by new development are embodied in higher base rates. Hence, UF engenders expected savings in future utility payments, analogous to the property tax savings created by CF. The fiscal effect of UF is therefore:

$$\Delta F_{UF} = UF - (PVW_{NUF} - PVW_{UF}) \tag{2}$$

where PVW_{NUF} and PVW_{UF} are the present values of expected future utility payments without and with UF, respectively. UF, like CF, may decrease developers' uncertainty over project approval and the provision of needed infrastructure (in this case, water and sewer related infrastructure). Again, if the dollar benefit equivalent to this reduction in uncertainty is greater (less) than ΔF_{UF}, UF would be expected to stimulate (reduce) commercial development, with the corresponding changes to the equilibrium level of county employment, as outlined above.

Finally consider SF. Here, causal pathways to the equilibrium level of employment are different than those identified for CF and UF. First, there is no direct monetary cost of the fee to commercial developers and therefore, no initial reduction in the supply of commercial space coming from the fees themselves. Second, we have no reason to believe that SF has any effect on the uncertainty facing commercial developers. Hence, the only direct effect that SF has on the commercial market is that, like CF and UF, it generates expectations of future property tax savings. Following the same logic described above, this effect would tend to increase the equilibrium level of employment.

However, SF may also indirectly affect commercial development and county employment by first affecting residential development. The effects of SF on housing development are analogous to those that CF and UF have on commercial development.[11] The fiscal effect is:

$$\Delta F_{SF} = SF - (PVT_{NSF} - PVT_{SF}) \tag{3}$$

where PVT_{NSF} and PVT_{SF} are the present values of expected future property tax payments without and with SF, respectively. Also, SF, like CF and UF, in the commercial market, may lower developers' uncertainty in the housing market by increasing the likelihood of project approval and the timely provision of needed infrastructure (in this case, schools). Furthermore, the adoption of (or increase in) SF may reduce other regulatory costs faced by the developer. Again, if the dollar benefit equivalent to the reduction in uncertainty and/or savings in non-SF related regulatory costs is greater (less) than ΔF_{SF}, SF will expand (reduce) housing development. More residential development means more people, which is expected to raise a county's equilibrium level of employment by 1) increasing the local demand for consumer goods and services and thereby the demand for labor, and 2) expanding the locally available labor supply, which reduces employers' search costs and equilibrium wages.[12]

In prior work, we found that residential impact fees stimulate the construction of both multi-family (Burge and Ihlanfeldt, 2006a) and single-family (Burge and Ihlanfeldt, 2006b) housing.[13] These results suggest that the benefits of SF to developers coming from reduced uncertainty and non-impact fee related costs are enough to fully offset the size of the monetary fee itself for residential development in suburban areas (where a majority of Florida's population lives and an overwhelming majority of new development occurs). These results, combined with the prediction that SF reduces the fiscal burden of commercial property, lead us to expect that SF will raise the equilibrium level of county employment.

In summary, the impact of CF and UF on county employment will depend upon whether the difference between the uncertainty reduction benefit and the fiscal effect is positive or negative, which

[8] Besides increasing the likelihood of project approval, the bounty provided by impact fees may reduce developers' regulatory costs. For example, communities with CF may zone more land for commercial development, saving developers both time and money by reducing the need for rezoning requests. Also, regardless of the type of fee, the time it takes for planners' review of the project may be shorter. Finally, other explicit fees, compliance costs, and exactions may well be lower where there are impact fees. The idea that impact fees allow for more development by creating a less restrictive regulatory environment (and thereby both increasing the likelihood of project approval and reducing the costs of obtaining approval) has been expressed by Altshuler and Gomez-Ibanez (1993), Gyourko (1991), and Ladd (1998).

[9] Empirical evidence that residential impact fees provide future property tax savings is provided by Ihlanfeldt and Shaughnessy (2004). The relationship between impact fees and future property tax rates is treated theoretically by Yinger (1998).

[10] This issue has been addressed in the literature by Brueckner (1997) who argues that the sign of ΔF_{CF} depends on whether or not the city implementing (increasing) the new CF is operating on the rising or falling portion of U-shaped per capita cost curve for public services. Communities that are below (above) the optimal city size who implement (or increase) CF should tend to have positive (negative) values for ΔF_{CF}. Impact fees in Florida are levied by counties typically containing heterogeneous areas (i.e., potentially having a central city beyond the optimal size but also with several smaller suburbs) within their borders. However, observation suggests that most new commercial development occurs in rapidly expanding suburban communities that are still small relative to central cities. Because these communities are likely to be on the upward sloping part of the public services cost curve, the expectation stemming from this part of the analysis is that increases in CF would tend to lower the equilibrium level of employment.

[11] For a more complete discussion of the relationship between development impact fees and residential development see Burge and Ihlanfeldt (2006a,b).

[12] An anonymous referee noted that more residential development may not cause more employment if the supply of vacant, developable land is limited. In this case, increased residential development could crowd out commercial development. As we note below, all of Florida's counties have an ample supply of available land available for development. We also provide evidence in footnote 37 that the effect of SF on employment does not depend on the amount of vacant land within the county.

[13] See these papers for a more comprehensive discussion of the relationship between residential impact fees and housing construction. School impact fees are, by far, the largest component of fees that apply to residential but not to commercial properties. Our data also reveal that changes in school fees and commercial fees are uncorrelated, ensuring that the identification strategy we employ can effectively isolate the effects of each type of fees.

cannot be determined *a priori*. However, in the case of SF, the expectation is that they will have a positive effect on the equilibrium level of county employment.

4. Panel data set

Our panel data come from Florida's 67 counties and cover the years 1990–2005. Thus, there are potentially 1072 observations (16 years times 67 counties).[14] We lose 22 observations due to missing data, including all observations from LaFayette County. We additionally tested for and excluded extreme outliers, leaving our final count at 1043.[15]

Because our identification strategy involves first differencing the data, it is advantageous that our panel is both wide and long. Annual employment estimates at the county level are those published by the Bureau of Economic Analysis (BEA). While other estimates are available (e.g., those published by the U.S. Bureau of Labor Statistics (ES-202) and the U. S. Bureau of the Census (County Business Patterns), the BEA data provide the most complete coverage of employees.[16]

The other key data items are our impact fee variables. A complete history of impact fees was obtained for each of Florida's counties by contacting county planning and building offices.[17] Through these contacts we were able to obtain all current and past impact fee schedules, each containing the actual monetary levels of all impact fees used in the community. Impact fees can be categorized into those that pay for part of the infrastructure costs of services funded by user fees and those that partially cover the infrastructure costs for those services funded by property taxes.[18] In the first category are water and sewer impact fees, while the second category includes all other impact fees (henceforth labeled non-water/sewer fees).[19] Non-water/sewer fees are used to help fund a wide variety of local public services. Because businesses do not directly benefit from certain services (e.g., schools, libraries, and parks), commercial developers only pay impact fees for a limited number of services. Non-water/sewer impact fees paid by commercial developers typically include those for road, police, fire, and emergency medical services (henceforth, summed to form a total amount that we label commercial fees). Each county has separate fee amounts for retail, office, and industrial land uses. Our commercial fee variable equals the average across these three categories of the real value of fees per 1000 ft^2 of commercial space. In addition to water/sewer and commercial fees, real school fees were added to our panel. These fees are, by far, the largest and most common of the fees paid by residential but not commercial developers.[20] In many counties, school fees depend on the square feet of living area and/or the number of bedrooms in the house. Our school fee variable equals the real fees levied on a standardized, medium-sized single-family home having 1800 ft^2 of living space and three bedrooms.[21]

The other variables included in our panel are the county's crime rate, real per capita income, population, property tax rate, and sales tax rate. The crime rate is the number of index offenses per 100,000 persons, where index offenses include murder, forcible sex, robbery, aggravated assault, burglary, larceny, and auto theft. Real per capita income is per capita personal income divided by the Urban South Consumer Price Index (base year=2005). The property tax rate is the county millage rate, which is the full tax rate within the unincorporated area of the county, covering all locally provided public services, including schools.[22] The millage rate equals the effective rate, because the state requires that the taxable value of commercial real estate equal fair market value. Sales taxes are levied at the discretion of the county and are earmarked for different services, depending on the county. All of these variables change over time within counties and for each it is reasonable to hypothesize that a change in the variable alters the equilibrium level of county employment. Except for the sales tax rate, all variables are reported annually in the *Florida Statistical Abstract*. Sales tax rates come from annual editions of the *Local Government Financial Information Handbook*.[23]

The means of all variables are reported in Table 1 for all 16 years of the panel and for the first (1990) and last (2005) years of the panel.[24] While the number of counties charging commercial fees increased by only 2 (from 30 to 32) over our panel, the average fee in real terms increased by 44%. The number of counties with water/sewer fees rose from 30 in 1990 to 44 in 2005, but there is little change in the average real value of these fees over the panel. Changes in school fees are the most dramatic. The number of counties with school fees increased from 7 in 1990 to 21 in 2005, and the real average fee increased by 248%. Because our estimation strategy involves first differencing the data, an important feature of impact fees in Florida is that different types were not frequently changed within counties in the same year. For example, while *levels* of commercial and school fees are correlated within counties (average correlation coefficient is above .4), this is not the case for their changes (average correlation coefficient is below .07). The same tendency holds for the correlations between water/sewer fees and school or commercial fees.

5. Estimated models

The equilibrium level of employment (E) in a county depends on a wide range of factors affecting labor supply and labor demand. These factors can be split into those that do not change (or change very little) over time ($\boldsymbol{X}$) and those that do change over time. In the latter category are impact fees (F) and other variables (Y). A reduced form

[14] Our data actually precede 1990 so that, although we first difference the data and include multiple lagged values in several of our estimations, we do not lose any observations.

[15] In preliminary runs, we tested for extreme outliers using the dfbeta command in Stata. Observations influencing the estimated effects of the test variables, in either direction, by more than 0.2 (roughly 10% of the eventual estimated long run effect of both school and commercial impact fees) were dropped.

[16] BEA estimates at the county level include both full-time and part-time jobs. The annual estimates are obtained from averaging twelve monthly observations for the year. Hence, the estimate provided best measures the number of jobs that existed within the county on May 15 of each year. For a complete description of BEA's methodology see http://www.bea.gov/regional/pdf/lapi2005/employment.pdf.

[17] Impact fees in Florida are imposed by county governments and are countywide in their application. While cities may charge impact fees for services not covered by the county (or charge different rates for a particular service), this practice is rare and city fees are in all cases small relative to those at county levels.

[18] Impact fee ordinances in Florida must satisfy the "rational nexus" test, which requires 1) a clear connection between new growth and the need for new capital facilities, 2) fees that are proportional to the costs of providing the facility, and 3) the payer of the fee benefit from the new public facilities.

[19] Most water/sewer impact fees in Florida are collected through county utility departments, while all other categories are typically collected by planning departments. Also, water/sewer impact fees are distinct from tap/connection fees that developers must pay to connect to the system. The onsite versus offsite cost distinction is critical. Water/sewer impact fees cover offsite costs that stem from the fact that new development eventually necessitates improvements/additions to the system that allow for more capacity. Water/sewer fees in Florida are always based on the number of equivalent residential units (ERUs) associated with a project, where the ERU is based on the average consumption of a single family home. Hence, it is the county's ERU fee that is used as the water/sewer impact fee variable in our panel.

[20] Two other fees that are paid by residential, but not commercial, developers are earmarked for parks and libraries. Library fees are not common and are small where they are found. Parks fees on average are somewhat larger (though still small in comparison to average school fees). Park fees tend to change at similar points in time to road impact fees—a category of fees that comprises a large portion of all commercial impact fees. The use of school impact fees alone to capture the desired property of applying to residential development, but not commercial, is ideal because our data reveal school and commercial fee changes to be uncorrelated.

[21] We selected this definition to remain consistent with Burge and Ihlanfeldt (2006b). Approximately half the existing housing stock in Florida lies above/below this cutoff.

[22] In incorporated areas the full tax equals this rate plus the city tax. The latter is typically 15 to 20% of the total tax and covers additional services that the city chooses to provide.

[23] The Florida Statistical Abstract is published by the Bureau of Economic and Business Research at the University of Florida. The Local Government Financial Information Handbook is published by the Florida Legislature Committee on Intergovernmental Relations.

[24] For all impact fee variables, the overall panel average and the year specific averages for 1990 and 2005 are computed using only observations where positive levels were present. The number of observations for which this condition is met is reported in Table 1.

Table 1
Variable means

	All years		1990		2005	
	Mean (standard deviation)	# Cases with fee[a]	Mean (standard deviation)	# Cases with fee	Mean (standard deviation)	# Cases with fee
Real commercial fees[b,c]	2029 (1127)	485	1888 (887)	30	2718 (2008)	32
Real school fees	1420 (1022)	221	706 (473)	7	2459 (1966)	21
Real water/sewer fees	3115 (1308)	638	3525 (1212)	30	3194 (1335)	44
Property tax rate	17.29 (2.58)		16.88 (2.38)		15.71 (2.76)	
Sales tax rate	.64 (.50)		.28 (.44)		.82 (.43)	
Crime rate	4649 (2280)		5404 (2983)		3519 (1261)	
Population	226,001 (372,079)		195,974 (331,207)		271,368 (433,787)	
Real per capita income	25,241 (7392)		24,148 (6856)		27,370 (7745)	
Total employment	104,647 (193,770)		86,585 (164,351)		134,462 (239,552)	

[a] The total number of cases is 1043 overall and 66 in 1990 and 2005.
[b] 2005 is base year for all real dollar values.
[c] Impact fees means are reported for those cases where fees are greater than zero.

model explaining equilibrium employment in county i at time t can then be expressed as:

$$E_{it} = a\boldsymbol{X}_i + bF_{it} + cY_{it} + e_{it}. \quad (4)$$

By first differencing both sides of Eq. (4), the time invariant vector ($\boldsymbol{X}$) drops out:

$$\Delta E_{it} = b\Delta F_{it} + c\Delta Y_{it} + \Delta e_{it}. \quad (5)$$

The Y variables fall into two distinct groups: 1) those that change uniformly over time across counties, and 2) those changing non-uniformly across counties. The first group of variables can be controlled for by including time fixed effects (i.e., dummy variables for each year of the panel). The second group of variables includes those that tend to follow a trend over time *within* counties. These variables can be controlled for by allowing each county to have its own specific employment growth trend. This is accomplished by introducing a set of county dummy variables into our first-differenced model. After adding both time (γ) and county (α) fixed effects to Eq. (5), our estimating equation becomes:

$$\Delta E_{it} = \alpha_i + \gamma_t + b\Delta F_{it} + c\Delta Y_{it} + \Delta e_{it}. \quad (6)$$

Under this specification, first differencing and county fixed effects control for unobserved heterogeneity in E levels and E changes, respectively. Including additional regressors other than impact fees may therefore be unnecessary.[25] Omitted variable bias will only result if changes in an excluded variable affecting employment are somehow commonly correlated with changes in impact fees within counties. Nevertheless, to thoroughly investigate the robustness of our impact fee results, we ran Eq. (6) with and without our control variables. Two sets of control variables are alternatively used. The first set is based on Akaike's AIC Criterion (Akaike, 1973). The set of variables that minimized the criterion (and thereby maximized "goodness-of-fit") included changes in the property tax rate, the crime rate, and population. The second set includes all of the control variables (i.e., adds changes in the sales tax rate and real per capita income to the first set).

Two important econometric issues arise in estimating Eq. (6). First, a change in impact fees is expected to have an impact on employment that is both delayed and potentially distributed over multiple years. To determine the appropriate lag structure to use for the estimation of Eq. (6) we iterated over combinations of $\Delta F_t \ldots \Delta F_{t-5}$, allowing the commercial fee (CF) and the school fee (SF) to have potentially different lag structures.[26] To identify the preferred specification we again used Akaike's AIC Criterion. The criterion is minimized by including the following impact fee variables in the model: ΔCF_{t-1}, ΔCF_{t-2}, ΔSF_{t-1}, and ΔSF_{t-2}.[27] Adding together the estimated coefficients on the lagged variables for each type of fee yields its long-run propensity (LRP). The LRP represents the long-run change in employment that can be attributed to a one-time change in the impact fee.[28] Second, both heteroskedasticity and serial correlation were detected in the residuals.[29] We therefore report standard errors that are robust to both arbitrary serial correlation and arbitrary heteroskedasticity obtained from computing the robust variance matrix for the FD estimator (Wooldridge 2002, p. 282).[30,31]

6. Results

All of our models were initially estimated including all three types of impact fees—water/sewer, school, and commercial. In none of the

[25] In our earlier work where we focused on the number of houses completed in an area per year (2006b) as the dependent variable, we included both the change in impact fees and the level of impact fees as explanatory variables. Changes were included based on the idea that they create a difference between the demand and supply price of housing, resulting in a change in housing construction. The level of fees was included because they may alter the percentage of residential projects annually proposed by developers that receive approval from the local government. That is, higher fees may speed up the adjustment to the new equilibrium level of the housing stock and thereby increase housing construction. Here we seek to explain the change in the equilibrium level of county employment that results from a change in each of our categories of impact fees. Hence, we include only the change in fees and not their levels. However, we do allow a change in fees to affect employment with a distributed lag over multiple years. We investigated whether the pattern in lagged effects is affected by the level of fees, expecting that shorter lags would have relatively stronger effects where levels of fees are higher. The results only weakly support this expectation and show that the level of fees does not affect the long-run change in employment that can be attributed to annual changes in impact fees.

[26] The current levels (t) of employment and impact fees are measured at May 15 and January 1, respectively. Hence, there is already a significant intra-year lag allowed for just by including ΔF_t. Therefore, by extending the grid search to ΔF_{t-5} we are allowing a maximum delay of more than 5 years.

[27] Note that normally ΔCF_{t-1} and ΔSF_{t-1} would require instrumentation in order to satisfy strict exogeneity. However, because of the intra-year lag identified in the previous note, instrumentation is not necessary. This is confirmed by the results from strict exogeneity tests discussed below.

[28] In the models including the control variables, the same two lags are included on these variables as on the impact fee variables. Experimentation with fewer or more lags on the control variables had little effect on their estimated LRPs or on the estimated LRPs of impact fees.

[29] The preferred test for serial correlation involves regressing Δe_{it} on $\Delta e_{i,t-1}$, for various time periods, as suggested by Wooldridge (2002, p. 283).

[30] The fully robust standard errors are obtained by using the "cluster" option in Stata, specifying that the standard errors be clustered at the county level.

[31] Another econometric issue that might be listed is suggested by Wolfers (2006), who argues that the effect of a policy shock may be absorbed by an area-specific time trend, if the lag structure is misspecified. Wolfers' concern is with a single policy change, in his case the passage of a no fault divorce law on a state's divorce rate, registered by a dummy variable in a model that includes a linear time trend that is interacted with a set of state dummy variables. He finds that this model yields biased estimates and offers a more appropriate model for capturing the dynamic effects; namely, instead of just using a dummy variable that equals one for the years that the state has the law, which is what prior studies had done, he uses a series of dummy variables indicating that the state had the law for 1 year, had the law for 2 years, etc. This model can be viewed as an event study that allows for lagged effects: one event per state—the passage of the No Fault Divorce Act. Wolfers' criticism of existing studies and his improved model specification are not applicable in our case, because impact fees are not an "event". Over the 15 years of our panel, fees change within counties from year to year. Higher fees can result from the adoption by counties not possessing fees, increases in existing fees, and adding new fees to cover additional public services. Also, the time trend effect that Woofers specifically attacks is a linear trend for each state in a fixed effects model. We estimate a first differences model that includes county dummy variables so that we can control for unobserved heterogeneity in employment levels (from first differencing) and in employment changes (from including the county dummy variables).

Table 2
Results from estimating models akin to those estimated in prior studies

Independent variables	(1)[a]	(2)	(3)	(4)	(5)	(6)
Dependent variable = $E_t - E_{t-2}$						
Commercial	4.107***		2.142**		.177	
fee level	(1.144)[b]		(1.083)		(.461)	
School			7.550***		4.267***	
fee level			(2.855)		(1.139)	
Commercial		8352***		3726**		1715
fee (yes = 1)		(2124)		(1489)		(2106)
School				11,410***		7989***
fee (yes = 1)				(4284)		(2513)
R^2	.209	.165	.322	.264	.740	.737
Observations	1043	1043	1043	1043	1043	1043
Dependent variable = $E_t - E_{t-1}$						
Commercial	1.957***		1.123**		−.071	
fee level	(.570)		(.568)		(.210)	
School			3.064**		1.718**	
fee level			(1.517)		(.706)	
Commercial		4426***		1795**		2049
fee (yes = 1)		(1116)		(718)		(2164)
School fee				6056***		4281***
(yes = 1)				(2180)		(1516)
R^2	.191	.164	.276	.258	.677	.677
Observations	1043	1043	1043	1043	1043	1043

***,**,* Significant at 1%, 5%, and 10% levels, respectively.
[a] Models (1)–(4) include time but not county fixed effects. Models (5) and (6) include time and county fixed effects.
[b] Standard errors robust to heteroskedasticity and serial correlation in parentheses.

models did the water/sewer variables come close to approaching statistical significance either individually, jointly, or when their estimated coefficients are summed to obtain the LRP. Moreover, excluding the water/sewer variables from the estimated models increased goodness-of-fit based on Akaike's AIC Criterion. We therefore concluded that water/sewer impact fees do not affect employment and dropped these variables from our models.[32]

Before presenting the results obtained from estimating Eq. (6), we report results from regressing the change in employment on levels of impact fees without including county fixed effects. These models mirror those estimated in prior studies. The results reported in the top panel of Table 2 are from estimating models that follow Nelson and Moody (2003) and Jeong and Feiock (2006) by using the two-year change in employment as the dependent variable. Because we use a one-year change in employment in estimating Eq. (6), we also report in the bottom panel of Table 2 the results obtained from regressions similar to prior models, but using our dependent variable for the purposes of comparison. Following Jeong and Feiock, impact fees enter the models in columns 2, 4, and 6 as a dummy variable indicating whether the county had an impact fee in year t. Our real monetary fee levels should be more closely correlated with the impact fee variable used by Nelson and Moody (fee collections per building permit), so the results of models estimated using our impact fee variables are presented in columns 1, 3, and 5. Since both previous investigations use impact fee variables that do not distinguish between commercial and school fees, we present the results of several related regressions that cover all possible cases. Columns 1 and 2 report the results obtained from using only our commercial impact fee variable while the models estimated for columns 3 and 4 include both commercial and school fees. The final two columns report the results obtained from using both impact fee variables and adding county fixed effects to the models.[33]

Displaying a great deal of consistency across specifications, the results reported in the first four columns of Table 2 suggest CF and SF have strong, positive, and significant effects on employment growth, regardless of whether fees enter as dummy variables or in levels, or whether the change in employment is measured over 1 or 2 years. These results parallel those reported in previous studies. None of the models, however, passes the strict exogeneity test.[34] The failure to pass this test suggests that in all of these models the estimated effects of impact fees on employment growth are biased upward, because greater employment growth in the present pushes impact fees higher in the future.[35]

Adding the county fixed effects to the series of models dramatically changes the results. In all cases, CF is now insignificant. Estimated SF effects remain positive and significant, but their magnitudes decline by more than 50%. And importantly, strict exogeneity is now satisfied, suggesting that allowing county-specific growth trends effectively mitigates the endogeneity bias problem generated by feedback from employment growth to future levels of impact fees.

While strict exogeneity cannot be rejected if county fixed effects are added to the models estimated in prior studies, these augmented models still have a number of significant drawbacks. First, they lack a theoretical underpinning. Changes in a county's equilibrium level of employment are caused by shifts in labor demand and/or labor supply curves. Hence, regressing changes on levels (i.e., the positions of the curves rather than their movements) makes little sense. Second, because of the high multicollinearity between CF and SF in levels, it may not be possible to isolate their individual effects on employment growth.

The results from estimating Eq. (6) are presented in Table 3. Columns 1 and 2 report the results obtained from estimating models including only impact fees and no control variables. The difference between the models is that county fixed effects are included in model (2) but not in model (1) (both models include the time variables). As is true for the estimated models whose results are reported in Table 2, allowing each county to have its own employment growth trend has important effects on the results. First, in model (1) the estimated

[32] Several explanations for the insignificance of the water/sewer fees arise. First, developers may simply be unaware of changes in these fees. The typical county in Florida has had water/sewer fees prior to the beginning of our panel and has increased these fees slowly (and perhaps even predictably) over time, with little public fanfare. In contrast, increases in commercial fees tend to be large and come from an additional public service being added to the mix, (e.g., a county may go from having just a road fee to having both a road fee and an EMS fee). Each new fee or existing fee increase requires the passage of a local ordinance that is preceded by public debate. Alternatively, developers' perceived benefits from improved water/sewer system infrastructure may be larger than the perceived benefit from having improved non-water/sewer public services if commercial development relies heavily upon good water/sewer service. Finally, we recognize the possibility that there may not be sufficient volatility in water/sewer impact fee levels during our panel. Table 1 shows that real water/sewer fee levels have remained relatively constant between 1990 and 2005.

[33] All six of the estimated models whose results are reported in Table 2 include time fixed effects.

[34] The strict exogeneity tests we use are those suggested by Wooldridge (2002). In the absence of county fixed effects, the preferred test is simply to check for feedback between the dependent variable and the independent variable (Wooldridge 2002, p. 146). So for each estimated model meant to mirror previous estimations we regress both CF_t and SF_t on $(E_{t-1}-E_{t-3})$ if the change in employment is measured over 2 years and on $(E_{t-1}-E_{t-2})$ if the change in employment is a one-year change. For our model (3) we regress (F_t-F_{t-1}) on $(E_{t-1}-E_{t-2})$.With county fixed effects, the change in employment measured over 2 years, and the fees in levels (as measured in prior studies), the strict exogeneity test involves regressing (E_t-E_{t-2}) on F_t and leading values of impact fees (Wooldridge, 2002. p. 285). We use F_{t+1} and F_{t+2}. The null hypothesis of strict exogeneity is rejected if the leading variables are jointly statistically significant. For our model (3), (E_t-E_{t-2}) is regressed on $(F_{t-1}-F_{t-2})$, $(F_{t-2}-F_{t-3})$, F_{t+1} and F_{t+2}. Again, strict exogeneity is rejected if the latter two variables are jointly significant. In all cases our joint significant tests are based on an F-statistic robust to both heteroskedasticity and serial correlation.

[35] As noted above, higher fees can result from fee adoption by counties not possessing fees, increases in existing fees, and adding new fees to cover additional public services.

Table 3
Results from estimating first-differenced models

	(1)[a]	(2)[b]	(3)	(4)
Δ commercial fee ($t-1$)	−.747	−1.511*	−1.344*	−1.361*
	(.701)[c]	(.867)	(.767)	(.768)
Δ commercial fee ($t-2$)	−.044	−.862*	−.821*	−.873*
	(.417)	(.472)	(.473)	(.484)
Δ school fee ($t-1$)	2.481***	1.204**	1.195*	1.194*
	(.898)	(.611)	(.655)	(.660)
Δ school fee ($t-2$)	2.217**	1.061***	.937**	.906**
	(1.059)	(.401)	(.381)	(.379)
Δ property tax rate ($t-1$)			−.537	−.530
			(.419)	(.422)
Δ property tax rate ($t-2$)			.110	.116
			(.150)	(.151)
Δ crime rate ($t-1$)			−.220*	−.203
			(.136)	(.135)
Δ crime rate ($t-2$)			−.209	−.217
			(.161)	(.170)
Δ population ($t-1$)			.038	.037
			(.115)	(.113)
Δ population ($t-2$)			−.128	−.130
			(.080)	(.080)
Δ sales tax ($t-1$)				.522
				(.391)
Δ sales tax ($t-2$)				.321
				(.433)
Δ real per capita income ($t-1$)				−.227
				(.232)
Δ real per capita income ($t-2$)				−.025
				(.125)
R^2	.069	.671	.678	.679
Observations	1043	1043	1043	1043
Long-run propensities				
Commercial fee	−.791	−2.373*	−2.165*	−2.234**
	(.974)	(1.230)	(1.130)	(1.134)
School fee	4.698***	2.265**	2.132**	2.100**
	(1.730)	(.898)	(.947)	(.955)
Property tax rate			−.427	−.414
			(.522)	(.526)
Crime rate			−.429	−.420
			(.273)	(.278)
Population			−.090	−.093
			(.112)	(.110)
Sales tax				.843
				(.754)
Real income				−.252
				(.249)

***,**,* Significant at 1%, 5%, and 10% levels, respectively.
[a] Column (1) model includes time but not county fixed effects.
[b] Columns (2)–(4) models include time and county fixed effects.
[c] Standard errors robust to heteroskedasticity and serial correlation in parentheses.

coefficients on the CF variables are negative but insignificant, while in model (2) they are negative, much larger in absolute magnitude, and significant.[36] Second, while the estimated coefficients on the SF variables are positive and significant in both models (1) and (2), their magnitudes are about half as large in (2) as in (1). Finally, and most importantly, model (2) passes the strict exogeneity test, while model (1) does not. In combination, these results again demonstrate the importance of including a county-specific employment growth trend to obtain consistent estimates.

Column 3 reports the results from adding the Akaike selected variables to the model, while the results reported in column 4 are obtained from including the entire set of control variables.[37] As expected, the addition of these variables to the model has little effect on the estimated CF and SF coefficients. The estimated coefficients on the CF and SF variables are highly similar among columns 2, 3, and 4. In addition, in all cases the variables are statistically significant. The bottom of Table 3 reports the estimated LRPs along with their estimated robust standard errors. The CF LRP ranges from −2.16 to −2.37, while the LRP range for SF is 2.10 to 2.26. All LRPs are significantly different from zero.[38]

The magnitudes of the estimated LRPs suggest that a $1000 increase in real commercial impact fees reduces the equilibrium level of private sector employment by just over 2000 jobs, while the same increase in real school fees expands employment by somewhat more than 2000 jobs. A reasonable approach to interpreting the magnitude of these overall effects is to undertake a simple “what if” thought experiment using a typical county. Lee County, which contains the cities of Ft. Myers and Cape Coral, contained 258,571 private sector jobs in 2005. Lee's employment is close to the mean level among counties having impact fees in 2005. Lee County's impact fee levels are also close to 2005 mean levels $\left(\overline{\text{CF}} = \$2718,\ \overline{\text{SF}} = \$2459\right)$. Our estimates suggest that over time, Lee's adoption of commercial impact fees has reduced its equilibrium employment level by about 2%, but that its adoption of school fees has increased employment by almost the same amount, resulting in little net change. However, the most important information gleaned from this experiment is that a 2% reduction (increase) in the equilibrium level of employment from having the average CF (SF) seems plausible in magnitude.

Our results suggest that it takes somewhere between 18 months and 2 years before a change in impact fees (CF or SF) starts to affect employment levels and that changes in employment continue thereafter for another 18 months. Although the initial response in employment may seem to occur too rapidly, the timing is plausible given that

[36] Because we are controlling for both space and time-fixed effects, it is reasonable to be somewhat more liberal in what is considered to be significant p values in determining statistical significance. Hence, an estimated parameter is considered to be statistically significant if the p value is 0.10 or lower, by a two-tailed test.

[37] None of the LRPs on the control variables is ever statistically significant. This result was expected and makes sense. To the extent that these variables trend smoothly over time, their influence is captured by the county fixed effects, which allow each county to have its own employment growth trend. This appears to be happening for all five control variables. If we drop the county fixed effects and re-estimate the model with the controls, the estimated LRPs show that population and per capita income have positive and significant effects, while the property tax rate and the crime rate have negative and significant effects. Fortunately for our investigation, the pattern of changes in impact fees differs greatly from that in the control variables. Impact fee changes are anything but smooth over time. Rather, changes are infrequent, and when a change does occur, it tends to be large (on average about $1000). Hence, while we are able to consistently identify the employment effects of impact fees in our preferred county fixed effects models, we are not able to use these results to comment on the effects of any of the control variables.

[38] As checks on the robustness of our results, we ran three additional sets of regressions: 1) we reestimated our models after dropping the 18 counties that had no impact fees during the years covered by our panel, 2) we estimated models that added a dummy variable to Eq. (6) indicating whether the county adopted a new impact fee in year t, and 3) we estimated Eq. (6) adding interactions that allowed the effects of CF and SF to vary across counties with different amounts of vacant developable land. Regarding 1), there is little change in the estimated LRPs or their levels of statistical significance from dropping those counties without impact fees; hence, our conclusions are unaltered by paring down the sample. Regarding 2), the adoption dummy variables are never statistically significant and their inclusion has little effect on the estimated CF and SF coefficients. These results, which are consistent with our theoretical analysis, suggest that what matters to employment growth are changes in the magnitudes of fees, regardless of whether the change comes from an initial adoption or raising already existing fees. Finally, in addressing 3) we were particularly interested in whether SF would cause less employment growth in counties with less developable land area. If the supply of land is constrained, the commercial development that we have hypothesized follows the construction of new housing may not occur to the same degree. Using GIS maps for each county, we measured the amount (in square miles) of undeveloped land. The estimated coefficients on the latter variable interacted with CF and SF are not statistically significant and including these interactions had little effect on our estimated LRPs. These results are not surprising, because considerable amounts of vacant land remain in all of Florida's counties. Even Broward County, which is commonly perceived as the county closest to being “built-out”, still has nearly 150 miles^2 of land available for future development.

an initial wave of employment changes is expected to come from construction related jobs. But if only the number of construction related jobs was affected, there would be no long-run change in equilibrium employment. The permanent decrease in employment from an increase in CF comes from a smaller equilibrium stock of commercial space, a factor of production which compliments labor, and thereby shrinks the number of jobs in the long-run. The permanent increase in employment from an increase in SF comes from the growth in population accompanying the expansion in housing supply induced by the higher SF.

Our finding that CF reduces employment suggests that these fees exceed the dollar value of any benefits that they provide to developers. Hence, our results fail to support the hypothesis that CF stimulates economic development by reducing developers' uncertainty. On the other hand, our theoretical discussion suggested that SF should raise the county's equilibrium level of employment, and our results confirm this hypothesis. On the surface, the results may appear contradictory, because the fiscal effect appears to dominate the uncertainty effect in the case of CF, but the opposite appears to be true for SF. However, fiscal impact analyses suggest that, in general, commercial development largely pays its own way, while residential development imposes a fiscal deficit on the community.[39] Hence, proposed housing projects are more likely to be near a yes/no cutoff margin with respect to project approval. Impact fees would therefore raise the likelihood of approval more for housing than for commercial proposals. This suggests that the benefit equivalent of the reduction in uncertainty may be quite different between CF and SF, which may help account for their differential effects on employment. This, combined with the fact that commercial development is exempt from SF, makes the opposing employment effects of CF and SF found in this paper quite plausible. In addition, it is worth reiterating that each impact fee has a LRP of reasonable size and the absolute value of the two LRPs is similar in magnitude.

7. Conclusion

Development impact fees are a relatively new source of revenue for local governments. While the number of cities and counties with fees is growing, this growth has been stunted by the real estate development community's opposition to fees. In part, this opposition is based on the belief that impact fees repel commercial and residential development by acting as a development tax, resulting in a lower property tax base and fewer jobs.

Our review of the literature found two studies that had investigated the effect impact fees have on private sector employment. Both studies conclude that the development community's prognostication is wrong—impact fees do not reduce the number of jobs, but in fact actually cause an expansion in employment by reducing developer uncertainty. Based upon these findings, impact fees would seem to be the ideal funding source for local public infrastructure. Impact fees eliminate the negative externality from new development imposed on existing property owners from higher property taxes and they expand the community's economic base.

However, our results cast suspicion on the conclusions of these studies. We first demonstrate that previous results may have been biased towards finding a positive relationship between fees and employment due to feedback from employment growth to higher future impact fees. We then report the results obtained from a first differenced model that is well grounded theoretically, allows for employment to respond to changes in fees using the appropriate lag structure, and satisfies the strict exogeneity condition required for consistent estimation. We find that the equilibrium level of employment is unaffected by water/sewer fees, grows with increases in school fees, and declines with increases in commercial fees. The contrasting results obtained for school and commercial impact fees are easy to rationalize. Direct monetary costs are imposed on commercial developers by commercial fees. While these fees may reduce the level of uncertainly over future development patterns, lower expected future property taxes, and increase the likelihood of project approval (or simply reduce regulatory costs), our findings suggest that these employment-enhancing effects are dominated by the negative effect that the fees themselves have on employment by raising the cost of commercial development. School fees, on the other hand, impose no direct costs on commercial developers and also carry the possible benefits of property tax savings and/or improved levels of public service provision. Additionally, we outlined how our previous work found school fees to stimulate housing construction. More homes bring more people, and more people bring more jobs.

From a policy perspective, our results suggest that the impacts CF and SF have on a community's job base be considered before adopting or raising these fees. How these impacts are viewed by the community will depend on its objectives. For example, CF may not be as desirable within communities that seek to maintain or expand employment. On the other hand, they become more favorable if the community wishes to repel commercial activity, perhaps out of a desire to maintain its residential character. SF are consistent with a goal to increase the number of jobs, but communities may or may not desire the accompanying population growth.

Politically, it may be difficult to adopt just one of the two types of fees. Some support for this conjecture is provided by our data. Counties with CF also tend to have SF and, although we find *changes* in CF and SF are uncorrelated, we also find their *levels* are highly correlated. If SF and CF are in fact an 'all or nothing' proposition for local governments, our results suggest that the 'all' seems to have no adverse effect on the long-run equilibrium level of jobs. However, while commercial and school fees may have offsetting effects on the level of employment, again it may matter that school fees increase the number of jobs, in part, by expanding the county's population.

Our research can be extended in a number of ways. One important avenue for future inquiry is to investigate whether the effects of impact fees on employment vary by industry group or between large and small communities. Industry type may matter because the dependence on local public infrastructure varies, for example, between manufacturing and services. If impact fees do reduce developer uncertainty, they may have less of a negative effect on manufacturing jobs than upon other types of jobs. Community size may matter because as the size of the jurisdiction increases relative to the size of the overall metropolitan area, it possesses greater monopoly power. Hence, the demand for commercial space and the demand for labor may be less elastic within larger cities, resulting in smaller job loses from an increase in commercial impact fees.

[39] For a review of these studies see Burchell et al. (1998).

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