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 jobs out of the community. 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. This result is consistent with the presented theoretical framework. Our investigation also suggests that differences between our results and those obtained in prior studies can largely 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 potential differential effects across various types of impact fees.

I. 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.[1] 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 (Nelson, 1988). 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 somewhat less frequently included are schools, libraries, police, fire and parks.[2] For each service there is a separate fee schedule, and developments pay fees only for services they directly consume (e.g., commercial developments would not be included under a school impact fee schedule).

Many controversial issues, including concerns over how impact fees will affect the availability and affordability of housing, surround the use of impact fees.[3] However, from the point of view of local governments considering implementing fees, the most

[1] Altshuler and Gomez-Ibanez (1993) discuss why property tax increases became increasingly unpopular over the 1970's. Based upon a nation-wide survey of local governments, Lawhon (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.

[2] 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 have expanded in both size (fee levels) and scope (services covered). See Burge and Ihlanfeldt (2007) for a discussion of these cases.

[3] Most existing 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 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).

important issue is the potential 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.[4] Proponents of fees make the case that they encourage *current* development by decreasing the uncertainly concerning the pattern of development over time (Nelson, et al., 1992; Nelson and Moody, 2003). This conclusion is based primarily on two arguments. 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 required and the tax revenues generated by the new development, existing property owners likely 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. Thus, in jurisdictions with impact fees the developer is more certain that his proposed project will be approved and that obtaining approval will not be unduly expensive, either in terms of money or time. 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.

[4] 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).

Only two studies (having much in common, as discussed more fully below) have empirically investigated the above issue, focusing on the affect that impact fees have on the local jurisdiction's private sector employment growth. Both studies conclude that 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 real possibility that impact fees are endogenous to employment growth. Even causal observation strongly 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 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 do not seem to matter. 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, 2006b). 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.

II. Literature Review

Only 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 have the advantage over 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 the data, time and area fixed effects were not included in any of their estimated models. They find the impact fee variable to be positive and statistically significant.[5]

Although pioneering, these studies suffer from two serious limitations. First, both studies suffer from the crude measurement of the impact fee variable. We argue the correct measure of commercial impact fees is what developers must pay per standardized area unit of commercial building space.[6] When combined with the fact that both studies lump residential and commercial impact fees together in their measure, it is difficult to argue either impact fee measure adequately captures the true nature of the impact fees faced by a typical commercial development project. Secondly, the failure to adequately deal with the endogeneity of fees (as well as many of the control variables), is a shortcoming of both studies. In the use of panel data, the strict exogeneity of the regressors is required to obtain consistent estimates (Wooldridge, 2000, 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: $E(X_{is}^{/}\varepsilon_{it}) = 0$, s, t=1,…, T. This assumption is much stronger than assuming zero contemporaneous correlation: $E(X_t^{/}\varepsilon_{it}) = 0$, t=1,…,T. Strict exogeneity is violated if current values of the dependent variable affect current or *future* values of the explanatory variables. Employment growth (the

[5] Neither of the two prior studies examines the possibility that different categories of impact fees may have differential effects on economic development. Both studies fail to account for water/sewer impact fees in constructing their impact fee measures and also make no distinction between residential and commercial fees. As discussed in Section III, this approach is incorrect because the relative costs and benefits to developers of commercial property are expected to vary greatly across different categories of fees.

[6] The dominant practice is to charge a set impact fee rate per 1,000 interior square feet of development. Our construction of the commercial impact fee variable is discussed in detail in Section IV.

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 find that commercial and school impact fees both have a positive effect on employment. However, we also demonstrate that these models clearly violate strict exogeneity.

III. Theoretical Framework

In contrast to previous studies, our work investigates the relationship between employment growth and three distinct categories of impact fees: commercial fees (CF), water/sewer utility fees (UF), and school fees (SF). Clearly, an increase in either CF or UF decreases the supply of newly developed commercial space by adding a direct monetary cost to obtaining project approval. However, a higher CF or UF will also raise the "bounty" that counties receive from allowing new development, which may make it less costly for the developer to obtain regulatory approval for the project.[7] Ceteris pribus, a decline in project approval costs increases the supply of commercial space. Additionally, increases in CF or UF should lead to lower future property tax rates (for the CF case) and lower future user charges for utility service (for the UF case). This effect is straightforward; by creating revenue streams that pay for expansions to infrastructure in

[7] Communities with impact fees may zone more land for commercial development, saving developers both time and money by reducing the need for rezoning requests. Also, in these communities, the time it takes for planners' review of the project may be shorter and variances may be easier to obtain. Finally, there is typically wide latitude in the interpretation and application of land use regulations, allowing discretionary enforcement of the rules that may favor developers in impact fee communities. The idea that impact fees allow for more development by creating a less restrictive regulatory environment has been expressed by Altshuler and Gomez-Ibanez (1993), Gyourko (1991), and Ladd (1998).

the community, the general reliance on property taxes (or user fees for the case water/sewer services) is lessened. The downward pressure impact fees have on future property tax rates has been investigated in the theoretical (Yinger, 1998) and empirical (Ihlanfeldt and Shaughnessy, 2004) literature on impact fees, with both finding that impact fees will lower future millage rates. Thus, there are two indirect and offsetting positive effects on the supply curve for commercial development in addition to the direct negative effect. If the *net effect* of an increase in CF or UF is to expand (reduce) supply, the price of commercial space will decline (rise). Because labor and commercial space are complimentary inputs, the cheaper (more expensive) commercial space will expand (reduce) the county's equilibrium level of employment.

A higher CF or UF may also attract employers into a county if these increases improve, or provide with less delay, the public infrastructure needed by new development. Firm location studies have consistently shown that public infrastructure (especially roads) has an important influence on business location decisions (see, for example Eberts, 1991). Therefore, the potentially offsetting supply effects are not the only factors in play following CF or UF increases- increased demand for commercial space could lead to greater construction (and employment), even if the negative and positive supply effects are of similar magnitudes. And while a higher CF or UF may provide infrastructure benefits to employers in theory, neither the existence nor the magnitudes of these benefits has been documented empirically. Hence, we cannot sign the predicted effect that an increase in CF or UF is expected to have on a county's equilibrium employment level *a priory*.

In contrast, the expected effect of an increase in SF is expected to be positive. Because commercial developers are exempt from paying SF, there are no direct monetary costs that would act to decrease the supply of commercial development. And while we have no reason to suspect the increase in SF has any effect on the project approval costs faced by commercial developers, the negative effect on future millage rates described above still holds. Simply put, at least a portion of the funds that were previously coming from a common pool that commercial development was a part of, is now coming entirely from residential development. Thus, the rise in SF reduces the expected future property tax burden associated with ownership of commercial property, increasing the supply of commercial development. The increase in SF is also expected to alter the demand for commercial property. In theory, the construction of residential property may expand or contract in response to an increase in SF, depending upon the same factors that we have discussed in the context of commercial development and changes in CF or UF. The higher SF itself reduces the supply of residential space, while the bounty provided to the county from the higher SF may reduce project approval costs and thereby increase supply.

In prior work, we have investigated this issue directly. We found that increases in residential impact fees stimulate the future construction of both multifamily (Burge and Ihlanfeldt, 2006a) and single-family (Burge and Ihlanfeldt, 2006b) housing.[8] These results therefore suggest that an increase in SF reduces the project approval costs of

[8] 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 residential impact fees when focusing on categories that apply to residential properties but not to commercial. Our data also reveals that changes in school fees are uncorrelated with when changes in commercial impact fees occur, ensuring that our employed identification strategy can effectively isolate the effects of each type.

residential developers by more than the fees themselves.[9] When combined with the findings from the present analysis, the implication is that impact fees reduce project approval costs more for residential development than for commercial development.[10] More homes mean more people, which raises a county's equilibrium level of employment by increasing the demand for locally produced goods and services and by expanding the locally available labor supply, which reduces employers' search costs and offer wages. An increase in SF, therefore, first expands employment in the housing construction industry, and then, as temporary construction jobs are lost, there is an offsetting *permanent* increase in employment induced by a larger county population.

IV. Panel Data Set

Our panel data come from 66 Florida counties and cover the years 1990 – 2005.[11] Thus, there are potentially 1,056 observations (16 years times 66 counties).[12] However, after testing for and excluding extreme outliers and dropping several observations with missing data, our final count equaled 1,043. Given our selected identification strategy that is described in Section V, it is important 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.

[9] We note in Burge and Ihlanfeldt (2006b) that part of the increase in single-family home construction may be attributed to an increase in demand. The argument follows the same reasoning presented above. Namely, that residential impact fees (as argued in that paper, but the argument extends to any type of impact fee covering services otherwise financed through property taxes) provide an insurance policy against future property tax rate increases caused by the need to finance additional public infrastructure. These expected future property tax savings lower the costs of ownership, attracting residents to the community.

[10] We are not surprised by this implication. Costs associated with regulatory barriers as a percentage of overall project costs are expected to be much higher for residential property (in particular, small homes and multifamily housing), than they are for commercial development.

[11] There are 67 counties in Florida. Unfortunately, extreme data problems coming from the employment variable cause us to lose all observations from one small county (LaFayette County).

[12] 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.

Bureau of Labor Statistics (ES-202) and the U. S. Bureau of the Census (County Business Patterns), the BEA data provides the most complete coverage of employees.[13]

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.[14] 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.[15] 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).[16] 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

[13] 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.

[14] 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.

[15] 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.

[16] Most water/sewer impact fees in Florida are collected through county utility departments, while all other categories are typically collected by the planning department. Also, water/sewer impact fees are distinct from tap/connection fees that developers must pay to connect to the system. The onsite vs. 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.

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 fees per 1000 square feet of commercial space. In addition to water/sewer and commercial fees, school fees were added to our panel. These fees are, by far, the largest and most common category of impact fee that is paid by residential development but not by commercial development.[17] 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 fees levied on a standardized, medium-sized single-family home having 1800 square feet of living space and three bedrooms.[18]

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. 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*.[19]

[17] Two other categories that are typically paid by residential development, but not commercial, are parks and libraries. Library fees are not common and are extremely small were they are found. Parks fees can be somewhat larger (though nowhere near the size of typical school impact fees) but unfortunately 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 changes in each category are found to occur without correlation to one another.

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

[19] 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.

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. 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%. Changes in school fees are even more 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%.[20] Because our estimation strategy involves first differencing the data, an important feature of the changes in commercial and school fees is that they generally were not changed within counties in the same year. Hence, while *levels* of commercial and school fees are correlated within counties (average correlation coefficient is above .4), this is not the case for changes (average correlation coefficient is below .07).

V. 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 (X) and those that do change over time. In the latter category are impact fees (F) and other variables (Y). A reduced form model explaining equilibrium employment in county i at time t can then be expressed as:

$$E_{it} = aX_{it} + bF_{it} + cY_{it} + e_{it} \quad (1)$$

By first differencing both sides of (1), the time invariant vector (X) drops out:

$$\Delta E_{it} = b\Delta F_{it} + c\Delta Y_{it} + \Delta e_{it} \quad (2)$$

[20] 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 was met is also reported in Table 1.

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 an already first-differenced model. Hence, our preferred specification becomes equation (2), after having included both time (t) and county (i) dummies.

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. 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 model (2) 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 the property tax rate, the crime rate, and population. The second set includes all of the control variables (i.e., adds the sales tax rate and real per capita income to the first set).

Two important econometric issues arise in estimating (2). 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 equation (2) 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.[21] 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}. Adding together the estimated coefficients on these lagged variables yields the long-fun propensity (LRP). The LRP represents the long-run change in employment that can be attributed to an annual change in impact fees.[22] Second, both heteroskedasticity and serial correlation were detected in the residuals.[23] We therefore report standard errors that are robust to both arbitrary serial correlation and heteroskedasticity obtained from computing the robust variance matrix for the FD estimator (Wooldridge 2000, p. 282).

VI. Results

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

[21] 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 five years.
[22] 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.
[23] The preferred test for serial correlation involves regressing Δe_{it} on $\Delta e_{i,t-1}$, for various time periods, as suggested by Wooldridge (2000, p. 283).

Akaike's AIC Criterion. We therefore concluded that water/sewer impact fees do not affect employment and dropped these variables from our models.[24]

Before presenting the results obtained from estimating (2), we report results from several models that are structurally similar to those estimated in prior studies. Found in the top panel of Table 2, these regressions 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 our preferred specification of equation (2), we also report in the bottom panel of Table 2 the results obtained from estimations similar to prior models, but using our dependent variable, for the purposes on 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 continuous measure 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 bases. Columns 1 and 2 use only our commercial impact fee variable while columns 3 and 4 include both commercial

[24] 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 how real water/sewer fee levels have remained relatively constant between 1990 and 2005.

and school fees. While all 6 columns use time fixed effects, the final two columns report the results obtained from using both impact fee variables along with county fixed effects.

With a great deal of consistency across specifications, the first four columns 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 a one or two year change in employment is measured. These results parallel those reported in prior studies. None of the models, however, passes the strict exogeneity test.[25] The failure to pass this test suggests that in all of these models, the estimated effects of impact fees on employment growth are significantly biased upward since greater employment growth in the present pushes impact fees higher in the future.[26]

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 are more than 50% smaller. And importantly, strict exogeneity is now satisfied, suggesting that allowing county-specific growth trends effectively controls for the endogeneity problem generated by feedback from employment growth to future levels of impact fees.

[25] 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 two years on $(E_{t-1} - E_{t-2})$ if the change in employment is a one year change. For our model (2) we regress $(F_t - F_{t-1})$ on $(E_{t-1} - E_{t-2})$.With county fixed effects, the change in employment measured over two 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 (2) $(E_t - E_{t-2})$ 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.

[26] 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.

While strict exogeneity can not 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, not their levels. Hence, regressing changes on levels (i.e., the positions of the curves rather than their movement) 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 equation (2) 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 coefficients on the CF variables are negative but insignificant, while in model (2) they are negative, much larger in absolute magnitude, and significant.[27] 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.

[27] 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.

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.[28] 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.

The magnitude of the estimated LRPs suggests that a $1000 increase in real commercial impact fees reduces the equilibrium level of private sector employment by just over 2,000 jobs, while the same increase in real school fees expands employment by somewhat more than 2,000 jobs. A reasonable approach to interpreting the magnitude of these overall effects is to undertake a simple "what if" thought experiment using a typical county. Lee County, which contains the cities of Ft. Myers and Cape Coral, contained 258,571 private sector jobs in 2005. Lee's employment is close to the mean level among counties having impact fees in 2005. Lee County's impact fee levels are also close to 2005 mean levels ($\overline{CF}$ = $2718, $\overline{SF}$ = $2459). Our estimates suggest that over time, Lee's

[28] 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 changes 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 control variables.

adoption of commercial impact fees has reduced its equilibrium employment level by about 2%, but that their adoption of school fees has increased employment by almost exactly the same amount, resulting in little net change. However, the most important information uncovered from this example, 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 two 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 this may seem like a relatively short initial delay, we argue this makes sense because as 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 held in equilibrium. 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 CFs reduces employment suggests that these fees exceed the dollar value of any benefits that they provide. Hence, our results fail to support the hypothesis that CFs stimulate economic development by reducing developers' uncertainty. On the other hand, our theoretical discussion suggested that an increase in SF should raise the county's equilibrium level of employment, and our results confirm this

hypothesis. An interesting result was that the magnitudes of the LRP were both plausible in magnitude and of similar size across the two categories.

VII. 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 may 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 prior studies to have investigated the effect that 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 results reported in prior studies. We first demonstrate that these results may have been biased towards a positive effect by 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, and potential benefits from these fees, while theorized to be present, are not likely to be all that large. While we recognize that commercial impact fees are likely to reduce the level of uncertainly over future development patterns, potentially lower future millage rates, and act to increase the likelihood of project approval (or simply reduce regulatory costs), we argue that our finding provide strong evidence that these offsetting benefits are smaller than the direct monetary costs. School fees, on the other hand, impose no direct costs on commercial developers and carry the benefits of reducing future uncertainly over development patterns and 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 this yields tangible benefits to commercial developers.

From a policy perspective, our results clearly favor the use of school fees over commercial fees. Commercial fees not only repel jobs, but it can also be argued that they are inequitable. Impact fees are justified where development fails to generate sufficient property tax revenue to cover the full cost of providing the additional public infrastructure it necessitates. Numerous fiscal impact analyses have concluded that commercial development 'pays its own way', while residential development has a negative fiscal impact on the local community.[29] At the same time, casual observation reveals that communities who use school impact fees also levy commercial impact fees. Impact fee programs are politically determined policies, and there seems to be

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

overwhelming evidence to support the notion that communities adopt 'balanced' approaches to paying for new infrastructure through impact fees. Although we document that *changes* in commercial and school fees are uncorrelated in our data, their *levels* are highly correlated- counties with high school fees tend to have high commercial fees, in nearly all cases. Hence, while the reasons this balanced approach tends to be universally followed by communities has not been a focus of this investigation, we extend our comments to point out that if school and commercial fees are in fact an 'all or nothing' proposition for local governments, then the 'all' seems to have no adverse effects on the long run equilibrium level of jobs.

Our current research can be extended in a number of ways. One important avenue for future inquiry would be to investigate whether the effects of impacts fees on employment vary by industry group or between large and small communities. Industry 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. For example, employers may find it more difficult to avoid paying the commercial impact fees of a central city in comparison to those of a small suburban town.

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Table 1
Variable Means

	All Years		1990		2005	
	Mean (st. dev.)	# Cases With Fee[c]	Mean (st. dev.)	# Cases With Fee	Mean (st. dev.)	# Cases With Fee
Real commercial fees[a,b]	2029	485	1888	30	2718	32
	(1127)		(887)		(2008)	
Real school fees	1420	221	706	7	2459	21
	(1022)		(473)		(1966)	
Real water/sewer fees	3115	638	3525	30	3194	44
	(1308)		(1212)		(1335)	
Property tax rate	17.29		16.88		15.71	
	(2.58)		(2.38)		(2.76)	
Sales tax rate	.64		.28		.82	
	(.50)		(.44)		(.43)	
Crime rate	4649		5404		3519	
	(2280)		(2983)		(1261)	
Population	226001		195974		271368	
	(372079)		(331207)		(433787)	
Real per capita income	25241		24148		27370	
	(7392)		(6856)		(7745)	
Total employment	104647		86585		134462	
	(193770)		(164351)		(239552)	

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

Table 2
Results from Estimating Models Akin to Those Estimated in Prior Studies

	Dependent Variable = $E_t - E_{t-2}$					
Independent Variables	(1)[a]	(2)	(3)	(4)	(5)	(6)
Commercial fee level	4.107***		2.142**		.177	
	(1.144)[b]		(1.083)		(.461)	
School fee level			7.550***		4.267***	
			(2.855)		(1.139)	
Commercial fee (yes=1)		8352***		3726**		1715
		(2124)		(1489)		(2106)
School fee (yes=1)				11410***		7989***
				(4284)		(2513)
R^2	.209	.165	.322	.264	.740	.737
Observations	1043	1043	1043	1043	1043	1043
	Dependent Variable = $E_t - E_{t-1}$					
	(1)	(2)	(3)	(4)	(5)	(6)
Independent Variables						
Commercial fee level	1.957***		1.123**		-.071	
	(.570)		(.568)		(.210)	
School fee level			3.064**		1.718**	
			(1.517)		(.706)	
Commercial fee (yes=1)		4426***		1795**		2049
		(1116)		(718)		(2164)
School fee (yes=1)				6056***		4281***
				(2180)		(1516)
R^2	.191	.164	.276	.258	.677	.677
Observations	1043	1043	1043	1043	1043	1043

[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.

***,**,* significant at 1%, 5%, and 10% levels, respectively.

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)

[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.
[***,**,*] significant at 1%, 5%, and 10% levels, respectively.