This paper uses data on sales for multifamily homes to determine whether TIF designation leads to higher appreciation rates. The study differs from previous work in its focus on individual sales rather than averages across municipalities. The analysis is carried out using what appears to be a straightforward hedonic approach applied to data from 1992-2000. The main finding is that “appreciation rates within TIF districts exceeded those of properties outside TIF boundaries, and the designation of TIF districts stimulates market value increases in areas that are ultimately designated TIF districts.” This result is impressive because neighborhoods may be more likely to be designated as TIFs when they are faring poorly so that they would have low appreciation rates in the absence of the TIF designation.

The paper is currently difficult to follow. The problem starts with the specification of the hedonic model and becomes serious with the presentation of the results. Some of the problem will end when the paper is eventually published: it is hard to switch back and forth between the text, footnotes, and long tables. But some of the problem is due to poor presentation. There is no need for a full set of results for 77 community areas, 35 quarterly dummies, and interactions between the quarterly dummies and the TIF designation variable. Full descriptive statistics are presented for all of these variables, and another full table presents the estimated indexes transformed as $\beta^{-1}$. Ironically, I usually complain that authors do not present enough information, whereas here so much information is presented that the paper becomes difficult to read. Fortunately, this overload can be eliminated by simply presenting the estimated indexes in diagrams, perhaps with confidence intervals. The descriptive statistics and model results do not need to be presented for the community area and time dummy variables.

The specification of the hedonic price function is also somewhat awkward. The author has three categories: (1) areas never receiving TIF designation, (2) a sale that takes place in an area that will be designated as a TIF in the future, and (3) a sale in an area that has already been designated as a TIF. Let $D_1$, $D_2$, and $D_3$ represent the three groups, and let $Q$ represent the series of quarterly dummy variables. The natural hedonic specification is:

$$Y = \alpha + X\beta + Q\lambda_1 + [QD_2\lambda_2 + QD_3\lambda_3 + \delta_2D_2 + \delta_3D_3] + u$$  \hspace{1cm} (1)$$

The main question is whether prices appreciate more (or less) rapidly in areas that are designated as TIFs. This can be done by comparing the price indexes for the base group ($Q\lambda_1$) against the path implied by the terms in brackets. A related question is whether for the subset of TIF properties prices appreciate more rapidly after TIF designation, i.e, is the path implied by $QD_2\lambda_2$ different from the path implied by $QD_3\lambda_3$? These questions can be addressed using F-tests and graphs showing the trends.
Instead, the author estimates two separate models. In the first, categories 2 and 3 are combined so that the model becomes:

\[ Y = \alpha + X\beta + Q\lambda_1 + [QD\lambda + \delta D] + u \]  
(2)

However, if the pre-TIF and post-TIF paths are different, these estimates are biased. The second model is estimated for the subset of properties that are eventually in TIF areas. This model allows the author to test whether the path implied by \( QD\lambda_2 \) to the path implied by \( QD\lambda_3 \). It would be better to combine the models into one equation using (1).

The author never addresses the issue of sample selection bias, which other authors have worried about in this context. I would expect that TIFs tend to be designated for areas that are expected to have slower rates of appreciation. Thus, the finding that appreciation rates are higher in TIF zones is a particularly striking finding. However, I think the author should discuss the issue. The complete set of community area dummies might control for most of the missing variable problems that lead to selection bias. Still, these community areas are large, much larger than census tracts. Since they include disparate neighborhoods, they may not control completely for selection bias problems. I am not advocating a full statistical analysis of selection bias; rather, I think the problems should be discussed along the lines here.

Minor points:

1. The author refers to Chicago’s “community areas” as being “census defined.” The community areas were defined around 1930 by a group of University of Chicago sociologists (I think) and have since been adopted as official designations by the city. Census tract boundaries do not cross community area lines. But I don’t think it is correct to give them official census status; they are a local term that the Census Bureau appears to respect.
2. The tables would be much easier to read if the variables were spelled out rather than using acronyms.
3. Should the property tax be included as an explanatory variable? Taken at face value, the results imply that higher property taxes are a good thing in that they increase sales prices. This result suggests that the variable is endogenous and should probably be dropped.