Valuing local collective goods: the case of business improvement districts

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It is hard to find situations with localized public goods which lend themselves well to empirical study in economics. Business improvement districts (BIDs), for this reason, make for an interesting case study. BIDs are regions which use tax revenue to increase local amenity provision, in hopes of stimulating extra economic activity in the area. This paper examines how these amenities are capitalized into residential properties which overlap with, or reside near, these BIDs. Several findings emerge. First, homes within BIDs appear to appreciate more than homes within the rest of the DC area, following a BID's anticipation or launch; this finding, however, is not robust to consideration of local price trends. Second, comparing homes within defined distances of BID borders suggests that homes within BIDs, following their anticipation or launch, do not appreciate more than their outside neighbors. Third, there is evidence of spillover effects. Homes closer to BIDs appreciate more than those further from such. Results suggest that positive externalities might not be solely confined to agents within a BID, but rather extend to its surrounding community.

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1 Introduction

It is well established from microeconomic theory that the optimal amount of local public good provision fails to be met when individuals determine their own contribution, producing a market failure lest such provision be determined at a higher level, namely a club or community. This is one argument used to justify collective action. How collective decisionmaking should be carried out, though, is not clear from such theory, nor are details concerning how costs and benefits should be allocated.

One application of local collective action which has started receiving attention in the literature is the formation of business improvement districts (BIDs). BIDs began in some parts of the United States as early as the 1960s, as a tool for revitalizing urban downtown areas, in response to the decline many have observed (see Glaeser and Kahn, 2004) with time, attributable in part to the movement of households, consumers, and firms away from central business districts beyond city boundaries, towards the surrounding suburban landscape - what is commonly referred to as urban sprawl. When a BID is established, typically with a vote by a predetermined metric among a group of commercial stakeholders within the geographic area of interest, an additional tax is imposed on this group which is used to supplement public goods which are already offered by their municipal government. Examples of supplemental goods may include street and sidewalk cleaning, area beautification, and the provision of security guards. Over the decades, BIDs have proliferated across the country at an accelerating pace, with over 1,000 currently in service by one estimate.

Recent economic research has started to further our understanding of BIDs and their implications. The question concerning where a BID is likely to be established has been modeled at the level of the state (Billings and Leland, 2012), city (Brooks, 2006), and district (Meltzer, 2012). Perhaps more economically interesting, though, is the question as to whether they are effective in revitalizing the inner city as their proponents claim, and how such regional gains can be quantified. One outcome of interest is crime. Indeed, findings suggest that BIDs help reduce criminal activity, and do not simply displace it towards other, surrounding regions. This effect has been identified with the use of dummy variables denoting a BID's presence in a neighborhood (Brooks, and Strange, 2008; Hoyt, 2005), as well as security expenditures of BIDs (Cook and MacDonald, 2011).

Recent findings support the additional claim that positive externalities generated by BIDs are evidenced by the rise in property values for areas affected by BIDs. Work by Ellen et al (2007) examine their effect on commercial and, to a lesser extent, residential properties using hedonic price modeling, in which the price of properties is regressed on observable property characteristics and its being "treated" by a BID, i.e. being located within the geographic area encompassed by such. Among other findings, Brooks and Strange (2011) provide further supportive evidence with their hedonic assessment using commercial properties. As the BID location decision is arguably endogenous for such properties, they select three different control groups to identify the treatment effect: those located near a BID, those in areas which took a vote on establishing a BID but ultimately decided against it, and all commercial properties in the area weighted in the regression via their propensity score (i.e. probability of being "treated" by a BID). Their results suggest that commercial properties in BIDs appreciate more - especially those with owners who supported starting the BID, and appear robust to the choice of control group.

While the studies of Ellen et al (2007) and Brooks and Strange (2011) are noteworthy for introducing the hedonic price methodology as a way of estimating the benefits of BIDs, and with the latter offering especially robust evidence in the case of commercial properties, omitted variable bias cannot be completely ruled out as a potential explanatory factor in their findings. If properties located inside of BIDs differ in unobserved ways from properties near the BIDs, or even from those properties in places unsuccessful in establishing a BID, and such factors correlate with both price appreciation and the likelihood of being treated by a BID, the treatment effect may be biased. Similarly, unobserved factors omitted from propensity score modelling can result in unreliable weights.

Using data collected for the District of Columbia (DC) area, this paper purposes to offer several extensions in quantifying the local externalities generated by BIDs, as reflected by property value appreciation, on several fronts. First, Brooks and Strange (2011) considers only the commercial property sector. It remains to be more rigorously shown, beyond the methods employed by Ellen et al (2007) - which gauges the treatment effect of being in a BID by using a simple, difference in difference (DID) methodology and, alternatively, by comparing the price trends of properties within BIDs with that of those outside of BIDs - if BIDs positively impact noncommercial properties, namely residential homes. One might expect positive externalities generated by BID activity to carry over to such markets. This paper tests this hypothesis.

Second, the paper offers two additional identification strategies. The first involves a border sample approach, in the spirit of geographic regression discontinuity design. It can be argued that positive BID externalities, like decreased crime, extend beyond the immediate BID area. And, indeed, Ellen et al (2007) attempt to identify this spillover effect in their estimates, using a DID methodology for homes within a defined buffer length around BIDs. However, some benefits offered by BIDs, like street cleaning and beautification, may be more localized in nature. It remains to be tested if such localized benefits can be sepearately identified. I do this by restricting the sample to properties located only within a small, fixed distance of the BID border. The second strategy more directly addresses the spillover effects in the neighboring community. Following methodologies which differs from that employed by Ellen et al (2007), and that has been used in the literature to assess the effect of proximity to such amenities as rail stations, I consider such a quantitative assessment here.

Third, this paper is to first to employ repeat-sale (RS) observations in the analysis of BID effects, in the analysis of spillover effects. The RS approach, often used in the hedonic price literature, is an additional method for mitigating omitted variable bias concerns. To the extent that unobserved, potentially confounding variables are time invariant, using RS observations “differences” them out of the regression equation altogether, thereby obviating concern of their biasing estimates.

Fourth, this paper uses such improved methods to test not only for a treatment effect, but also the possibility of an anticipation effect, i.e. appreciation which occurs shortly before the BID treatment occurs, as also previously considered by Ellen et al (2007) to some extent, who estimate time trends for BID properties both before and after a BID is launched. Such an effect is arguably plausible in theory, given the amount of community awareness that is raised during the BID formation process.

The results offer several findings. First, relative to residential homes in the DC area, homes within BIDs do appear to appreciate more than homes not located in BIDs, and that residents appear to anticipate them before they begin to provide services. However, these findings are not robust to the inclusion of regional time trends, suggesting that the effect initially observed may be attributable to improvements on a larger scale which correlates with the location of the BIDs. Second, using border samples, I am unable to detect statistically significant appreciation in homes located within fixed distances of up to 200m from a BID's border, following its anticipation or launch, relative to their neighbors outside of BIDs within the same fixed distances on the other side of the border. Third, estimates do not rule out the hypothesis that BIDs generate spillover effects: homes which are more proximal to a BID at the time of sale appear to sell at a higher premium than their further away neighbors, ceteris paribus. This third finding is supported by the use of RS observations, and also with such appears robust when subjected to a constructed falsification test.

The organization of the paper is as follows. Section 2 provides additional background information on BIDs in DC. Section 3 discusses the empirical methodology used for this study, and Section 4 its results. Section 5 concludes.

2 Background

A BID, by construction, comprises properties which are geographically contiguous to one another, but otherwise is under no constraint regarding its shape or size. The introduction of BIDs into DC began officially in 1996, following years of policy discussion, with the enactment of the Business Improvement District Act. This law defines the process necessary for establishing a BID, each of which must undergo a process to produce its own particular BID enabling legislation. This process involves several major steps, the main ones of which are briefly summarized here.[1] To start, a committee of tenants, property owners, or other interested groups must be established to lay the groundwork, including determining the BID boundaries and the tax, gauging interest within the community, establishing a nonprofit entity to independently oversee the operations of the BID, and all the while raising funds for these activities.[2] An application must then be submitted to the Mayor, which among other types of information, including a proposed business plan and tax requirements, requires the signatures of stakeholders within the area under consideration. Aside from exceptions made by the DC Council, the lawmaking body of the city, in the cases of particular BIDs, a request to formulate a BID requires the approval of, among those within the area, 1) business owners with at least 51 percent of the assessed value of commercial properties, 2) 51 percent of commercial tenants, and 3) owners of at least 51 percent of the commercial properties.[3] Starting a BID additionally requires working with a DC Councilmember, in order to have BID enabling legislation introduced and passed. Following additional procedures, including a mandatory public hearing on the matter, the Mayor may choose to authorize the BID's establishment. Revenue for BIDs, once authorized and launched, is acquired through tax bills - at rates according to the BID enabling legislation, which the city collects on their behalf. The tax rate can be a function of a property's assessed value, geographic size and type of property (e.g. retail, hotel, etc). Residential homes are typically exempt from such taxation, with exceptions of residential units in three BIDs - Capitol Riverfront, Noma, and Mount Vernon Triangle - which employ per unit taxes between $96-$120 annually. Law requires that BID enabling legislation be renewed every five years. To date, no BID has

[1] For a more detailed description in BID starting process, see the DC BID Council (http://www.dcbidcouncil.org/storage/Starting-a-BID.pdf).

[2] Once a BID has been established, arrangements can be made to use its revenue to reimburse for these initial expenses.

[3] Such requirement are *de jure*. *De facto*, in all cases to date, the DC Council has waived the second requirement regarding commercial tenants, and amended the third to 25 percent.

failed to obtain renewal.

As Figure 1 illustrates, to date nine BIDs have been established within DC. About 64 percent of office space, 54 percent of all jobs, and half of all hotel rooms are estimated to be located within a BID. Over $1.5 billion in tax revenue originates from activity within BIDs. Their provided amenities include cleaning, beautification, hospitality, marketing, planning, safety, event organizing and programming. Concerning cleaning and beautification, in 2011 it is estimated that over 3,000 tons of trash were removed from sidewalks and public spaces. Furthermore, 324 trees and 36,000 bulbs are estimated to have been planted, in addition to existing tree boxes and planters being maintained. With hospitality, in the same year 773,000 visitor questions are estimated to have been answered by staff. BIDs in DC have additionally organized hundreds of special events to draw visitors into the commercial areas. BIDs also extend outreach efforts to the homeless. They estimate having assisted 78 persons obtain permanent housing, and over 1,000 homeless persons to obtain other basic needs, including meal provision, psychiatric counseling, training for employment, and public benefits. Some BIDs have promoted small scale sustainability efforts, including recycling campaigns, biodiversity initiatives, and park maintenance (DC BID Council, 2012).

3 Empirical Methodology

Following a common practice of economic literature stemming as far back as Rosen (1974), I begin with an empirical framework for hedonic price analysis. This approach models the price of a residential property as a function of observable characteristics which comprise its value, i.e. buyers' willingness to pay for such a property. Such a model here takes the following form of

$$P_{it} = X_{it}\beta_1 + T_{it}\beta_2 + Treated_{it}\beta_3 + Treated{*}After_{it}\beta_4 + Treated{*}Antic_{it}\beta_5 + \varepsilon_{it} \tag{1}$$

The dependent variable P_{it} denotes the logarithm of the price of property i sold at time t. Covariates of the regression comprise characteristics of the home, denoted by the vector X_{it}, include the land area, age of the structure, number of rooms and bathrooms, number of fireplaces, style of the structure, distance to the nearest Washington Metro rail station, etc.[4] Time trends are controlled for using year fixed effects, T_{it}, to differentiate between periods during which appreciation and depreciation is more heavily observed among properties in general, e.g. the rise and subsequent fall of home prices during the 2000s. As most BIDs began operating during periods that home prices were on the rise, time fixed effects are indispensable in this type of assessment.

The fundamental identification strategy underlying the first set of models in this paper is DID. Treated observations, defined here as all properties which are geographically located within the confines of a BID, are denoted by $Treated_{it}$. Its coefficient β_3 captures the otherwise unobserved, time invariant premium which may be particular to properties located in an area which adopts a BID from properties outside of such. Identification of the treatment effect comes from β_4, the coefficient of interest, which interacts the $Treated_{it}$ term with a dummy variable indicating that a property was sold after its BID began providing services. This coefficient thus estimates the marginal effect of a property's being treated by a BID, i.e. being directly exposed to its provided amenities.

If property owners within a BID's borders are prescient of a BID's arrival before its operations begin, one might expect property values to adjust before the BID even launches. As previously discussed in Section 2, a BID in DC must first obtain a majority vote of

[4] A full listing of covariates used in the model can be located in Appendix 1.

approval, via signature collection, before being lawfully enacted by the DC Council and authorized by the Mayor. The campaign process to render such a majority possible can be time consuming.[5] For this reason, I additionally incorporate an anticipation effect, denoted by $Treated * Antic_{it}$, which interacts the treatment term with a binary variable indicating that the property in a BID region was sold during the period of anticipation. One problem with this approach is that it is difficult to precisely define a time window as to how far in advance property owners could predict a BID's arrival. Interviews with executive members of BIDs in DC indicate there being much heterogeneity in time and effort needed to establish a BID in each region. In general, however, these interviews suggest that campaign efforts precede the legislative process by at least 12 months - since law requires signatures to be collected before the BID can be established. Using legislative records, I can identify all dates associated with the momentum of BID legislation, and use it to estimate - albeit crudely - how far back the campaigning period extended. I thus define the anticipation time window as the period preceding the launch of the BID, as far back as the 6 months prior to the month that the first piece of legislation for a particular BID was introduced in the DC Council.[6]

The model of interest up to now, Equation (1), compares the appreciation observed by homes in BIDs with those outside of BIDs. The story, however, becomes more complicated if treatment is not so discrete in nature. The premium effect of BIDs may to some extent be captured by homes proximal to BIDs, as well. If such amenities as increased neighborhood safety and a cleaner atmosphere benefit homes close to, but not necessarily in, BIDs it is reasonable to expect such properties to capitalize in response as well, and we might expect to find perhaps little difference in appreciation between homes on either side of a BID's border. It is possible, for example, that home buyers generally do not make a major distinction

[5] In the case of one BID, Capitol Hill, campaigning began three years prior to its eventual launch in spring, 2003.

[6] Documentation of campaign efforts recognized by the media, and evidence of a BID's actual incorporation suggest that, in some cases, these made assumptions underlying the duration of the anticipation period are conservative.

between home residing within the confines of a BID and being within walking distance of one. For this reason, it is worth considering spillover effects. To do this, I substitute the previous specification with instead the following function, for residential homes located outside of a BID:

$$P_{it} = X_{it}\beta_1 + T_{it}\beta_2 + D_{it}\beta_3 + \varepsilon_{it}. \tag{2}$$

Here, the term D_{it} replaces the previous variable of interest, $Treated_{it}$, denoting a home's distance to the most proximal BID at time t. The coefficient of interest,β_3, can be interpreted as the marginal effect of an incremental change in distance to a BID which has launched on one's property value.[7] This model is analogous to that often considered, for example, in the urban literature for assessing the effect of a home's distance to such nearby amenities or disamenities as a nearby rail transit station or a polluted site. Note that this specification requires a distance be assigned to each observation, meaning that all observations included in the model must have sold subsequent to at least one BID being already in place. Identification then relies on the variation observed not only between observations relative to the first BID, but relative subsequent differences induced by the launch of later BIDs. For this reason, it is not as straightforward to perform a before/after type of assessment.

For this reason, I additionally employ the use of RS estimates. These comprise observations which sell more than once over the time windows considered.[8] Consider a property i which sells at time periods t and, later, t+j. Equation (2) can then be remodeled using the following alternative:

[7] Alternatively, the choice can be made to gauge distance to a BID for homes sold anytime following the anticipation period. Such a modification was considered within the model; results, though not included in this paper, appear very similar to those applying this definition of distance.

[8] RS observations were not previously considered in the context of Equation (1), due to there being too few RS observations in the sample which were directly treated by a BID.

$$P_{it+j} - P_{it} = (X_{it+j} - X_{it})\beta_1 + (T_{it+j} - T_{it})\beta_2 + (D_{it+j} - D_{it})\beta_3 + (\varepsilon_{it+j} - \varepsilon_{it}). \quad (3)$$

Assuming that property covariates remain time invariant,[9] this simplifies to:

$$\triangle P_i = \triangle T_i \beta_2 + \triangle D_i \beta_3 + \triangle \varepsilon_i. \quad (4)$$

The RS approach strengthens the identification strategy for estimating the coefficients of interest, β_3, by comparing the within variation in the values of properties which observe a change in distance to the nearest BID to a control group of homes which observe no change over similar time periods. Identification using RS data thus stems not simply from variation in distance per se, but variation in distance *changes*. The salience of this subtle innovation is demonstrated, among others, by Baum-Snow and Kahn (2005) and Gibbons and Machin (2002), who illustrate this in the case of promixity to rail transit stations. It exploits the structure enabled by a panel dataset under the fixed effects assumption, further mitigating concerns about unobserved heterogeneity, and which could otherwise potentially bias estimates due to their correlation with the distance term. The tradeoffs associated with using RS observations are well recognized in the literature. First, as already noted, is the assumption that covariates do not change with time. This can be especially a problem if homes are remodeled or structurally altered between sales, something which I attempt to address in this data by omitting homes identified as such. Second, there is reduced efficiency due to the loss of information resulting from omitting homes which do not sell more than once.

[9] In the actual empirical model, this is a mild oversimplification. It is known, for example, that additional rail stations were added since 1997, causing the covariate for the distance to the nearest rail station to change for some homes between sales. In the RS specifications I run, these changes are controlled for.

Third, introducing RS can introduce new omitted variable concerns: properties which sell more than once may systematically differ from their single sale counterparts, in which case estimates would not generalize to the entire population. Using RS estimates thus requires the assumption that none of these three problems are notably relevant to the case at hand.

Data for this exercise originates from several sources. Information on property characteristics, transactions, and geographic location was provided by DC Office of Tax and Revenue. Additional mapping for the location of BIDs, which enables me to identify the location of properties relative to BIDs using geographic information systems (GIS) software, was provided by the GIS Department of the DC Office of the Chief Technology Officer. For dates associated with when anticipation and services for each BID began, I consulted several sources, including the DC BID Council, executive members of BID staff, and legislative records from the DC BID Council. Homes sales for the first part of the analysis, using total and border samples, extend as far back as 1992. Sales for the second part, analyzing spillover effects, extend as far back as 1998, since the first two BIDs began operations in November, 1997.

Observations for the RS sample are restricted to only arms length transactions, which are conveniently denoted already in the provided data as "qualified" sales. Given the heterogeneity in what is defined as a residential property, I limit consideration to single family dwellings only. RS observations are further filtered to omit those which 1) do not sell twice between 1 and 10 years of one another, 2) are identified in the data as having been remodeled or potentially had structural improvements between both sales, and 3) to mitigate concerns of "flipped" homes and other outliers, exceed a 25% rate of annual appreciation. To the extent that BIDs are believed to induce appreciation large enough to exceed this threshold, estimates obtained with the third filter in place may be interpreted as conservative, lower bounds of the true treatment effect.

4 Results

Following an inspection of sample characteristics, this section begins by using several different specifications and samples to identify the direct effect of business improvement districts on home property values. It follows with an assessment of spillovers.

4.1 Large sample estimates

Table 1 provides several descriptive statistics of characteristics for homes sold within the sample, provided alongside of difference in mean estimates which compare homes within a BID (treated) with homes outside of a BID (untreated). Treated homes do appear to sell, on average, at amounts higher than their counterparts outside of BIDs. It is possible that several other factors, however, left uncontrolled for, may account for this, as evidenced by a number of covariate characteristics which significantly differ between the two groups. For example, homes with BIDs, are on average more often remodeled, are closer to rail stations, younger in age, and have sold more recently than non-BID homes. This is not to say that all characteristics appear more valuable within BIDs, however. As evidenced by decreased land and building area, and the increased share of row homes (in contrast to detached or semi-detached), these homes tend also to be smaller and more compact, though this may be offset to some extent by larger number of stories. Regression analysis will be important for more thoroughly controlling for these characteristics.

Table 2 begins with specifications modeled in Equation (1) in which all, or a substantial subset, of the sample is included in the estimation. Given the large number of controls used in the hedonic regression, Table 2 and all subsequent tables display results for only the variables of interest. In this case, those variables are $Treated_{it}$, $Treated * Antic_{it}$, and $Treated * After_{it}$.[10] Column (1) displays the "naive" baseline case, wherein all single family home sales considered within the sample. It predicts a positive, statistically significant effect of 5.75 percent premium for homes located in a BID after its launch. Although barely

[10]Appendix 1 provides additional estimates for remaining covariates, particularly for the specification corresponding to Table 2, column (1).

insignificant at the 10 percent level, it also predicts a premium during the anticipation period preceding the BID's launch which, interestingly, in the amount of 10.6 percent exceeds the predicted magnitude associated with the launch itself. However, there is reason to be suspicious about the validity of these estimates, as all of the BIDs are located in or near the downtown region of the city, near the National Mall. Starting in the late 1990s, this area began to undergo additional revitalization efforts other than the establishment of BIDs. Interviews with BID associates suggest, for example, that the launch of the MCI Center was one event which was poised to attract additional consumer activity in the region. Thus, column (1) estimates may not be reliable, due to the presence of unobserved, potentially confounding factors. Interestingly, in this case no preexisting premium is detected for homes residing in BID territory, irrespective of its sale relative to the timing of a BID.

In column (2), I relax the assumption that all residential homes in DC are subject to the same price trends, by allowing year fixed effects to vary geographically by ward, a smaller geographic entity of which there are eight in DC.[11] Two observations are noteworthy. First, controlling for these local trends, there appear to be preexisting differences between BID and non-BID homes, which is accounted for by the 10.5 percent predicted premium for $Treated_{it}$. Second, the positive effects previous predicted for both of the coefficients of interest disappear. In the case of $Treated * After_{it}$, we observe instead a statistically significant coefficient which is counterintuitively negative, suggesting that homes within BIDs lose the preexisting premium within their wards following the launch of such BIDs. In column (3), I refine the specification further, by reducing the sample to homes within the two wards containing the two BIDs - Capitol Hill (CH) and Adams Morgan (AM) - which comprise the bulk of treated residential properties. Here, the story is similar to that told by column (2), with a preexisting positive premium being predicted for homes within BIDs, which is offset by relatively higher appreciation of homes surrounding such following BID establishment.

[11]To mitigate concerns about the geographic delineation of the wards being correlated with those of the BIDs, I use borders for wards as defined originally in 1990, years before serious discussion about BIDs began to take place.

4.2 Border sample estimates

In Table 3, I refine my identification strategy by reducing the sample to only observations located within a fixed distance of any BID border. The border discontinuity approach purposes to limit the sample to properties for which the treatment effect is less likely to be biased by other confounding factors and trends, by comparing treated homes to those which are plausibly most likely to be similar in terms of their physical characteristics, and primarily varying by what side of the road they are on. This approach has the advantage of mitigating concern about geographic differences between the control and treatment group. Given the large sample restriction such an exercise entails, tradeoffs between efficiency and unbiasedness must be considered. The more inclusive of observations one's sample becomes, by extending the length from the border considered, the more arguable this alikeness assumption becomes, and the more susceptible one's estimates potentially are to bias. Homes located 200m from a BID, for example, may differ more generally from homes the same distance from the border on the opposite side than homes located 25m from either side of the border. Conversely, the smaller one's sample, the more susceptible to noise and inefficiency point estimates become.[12] For this reason, I consider multiple fixed buffer lengths around the BID borders.

In columns (1) through (4), using the hedonic price specification for all considered lengths around the BID border, the model fails to detect any statistically significant findings associated with either the anticipation or arrival of a BID on property values. Curiously, what is frequently instead observed is a positive and significant value for the $Treated_{it}$ coefficient, suggesting once again that a preexisting premium for homes located in territory treated by a BID appears to exist, but that the treatment of a BID in itself does not affect that premium.

[12] In Appendix 2, I offer a visual demonstration of this tradeoff by performing difference in means comparisons for properties in the smallest and largest border samples considered. It shows that extending the sample causes more variables to significantly differ from one another.

4.3 Spillover effect estimates

The results thus far from Tables 2 and 3 suggest what appears to be some appreciation associated with homes affected by BIDs. However, the lack of robustness associated with this premium specifically applying to homes within the confine of BIDs, as indicated by generally insignificant findings shown by 1) the incorporation of ward-year fixed effects, and 2) the comparison of homes on opposites sides of a BID's border, leaves open the possibility that capitalization indeed extends beyond the immediate area, amounting to a spillover effect.

One decision that must be made in testing for this effect is how much of the sample to be used. As in previous specifications, there is a tradeoff between bias and efficiency to be mindful of. The more of the sample that is included, the more plausible the concern that a property's distance from a BID correlates with unobservables which are also correlated with property values. As done previously with the border samples, I employ multiple sample sizes for consideration, using homes within one quarter mile, one half mile, one mile, and two miles of the border of any of the nine BIDs.

Table 4 provides results for each of the four samples considered, including both hedonic price and, additionally here, RS estimates. To account for possible nonlinearity, I include a squared term for the distance measure in all specifications. Figures 2a and 2b are included to supplement the table by visually demonstrating the trajectories implied by the regression coefficients. Before beginning, it is important to note that, given the difference in specification structure between hedonic and RS models, the figures convey similar types of information but are not directly comparable to one another. The x-axis in the hedonic model (Figure 2a) refers to the actual distance from the closest BID, whereas for the RS model (Figure 2b) it indicates the *change* in distance from the closest BID that occurred between sales.

A few observations are noteworthy. First, in nearly all eight specifications, the distance term and its squared counterpart are predicted to be statistically significant, implying a non-linear effect. One exception is column (1), which predicts a strictly linear, negative effect.

For the hedonic specification, extending one mile from a BID's border is associated with about a seven to ten percent depreciation in property values, and an additional three to ten percent decrease as one moves to two miles from a BID. For RS, the predicted pattern is an approximate 12 to 15 percent decrease in property values for the first mile change of distance, with a wider range predicted for the second mile change, from an actual seven percent appreciation to another six percent depreciation. The more parabolic pattern observed with RS estimates is interesting, although it is worth bearing in mind that the U-shaped appreciation is observed more for specifications of smaller samples, namely those with observations one quarter and one half mile from BIDs. Second, although there is more variation in the range of predicted depreciation in the RS case, the two specifications are generally unanimous in the general trend of price decreases predicted by a home's change in distance from the nearest BID. These models support the claim that changes in distance become less relevant the more distant from BIDs a residency becomes, and are consistent with the hypothesis that homes proximal to BIDs appreciate more in value as a result of the increased exposure to amenities which they provide.

It is important to keep in mind, especially in the case of hedonic price estimates, that observed capitalization may be a result of bias, if homes more proximal to BIDs have other characteristics in common which cause them to appreciate more than homes further away, over the time periods considered in the data. It could be argued, for example, that what is being captured is rather a preexisting trend, whereby despite the structure of DC being less amenable to the label of monocentric typically assumed in urban economic models, homes more proximal to the downtown region experience higher property value appreciation in general, independent of BIDs. The RS model is one attempt to mitigate this concern. As an additional precautionary measure, however, I consider a falsification exercise, whereby I perform the same type of assessment, but under the counterfactual assumption that each BID instead was launched precisely 4 years prior to the month and year recorded in the data. If there are preexisting price trends which correlate with home proximity to BIDs, then we should observe a similarly negative effect in this scenario.

In performing this assessment, several adjustments to the sample are made. First, observations for the four years preceding the time following the first BID launch, 1998, are included, so that observations now include homes selling as early as 1994. Second, in the hedonic price specification, observations are omitted which observe the same value in the falsification exercise as they do in the true case. This is done in an effort to minimize "contamination," by having actual effects of distance from a BID mixing with the counterfactual effects. Likewise, I omit observations in the RS case if the counterfactual, nonzero change in distance between sale periods is equal to the actual change observed.

Results of the counterfactual estimation are listed in Table 5, and illustrated in Figures 3a and 3b. For hedonic estimates, the effect of proximity initially appears to be somewhat less relevant - as suggested by the one quarter mile estimates, for which the results are statistically insignificant, and the half mile estimates, which observe a weaker negative effect initially that gradually becomes positive at around 1.25 miles from a BID. However, remaining estimates using samples of observations within one mile and two miles of any BID do not experience these complications, and predict monotonic depreciation of up to 15 to 18 percent for homes two miles from a BID. In both cases, though, there is a parabolic pattern, albeit more pronounced in the case of the one mile sample, which was not unobserved in the factual case. Taken as a whole, though, the falsification estimates for the hedonic price case do not seem to rule out the possibility of preexisting trends confounding with previously observed estimates.

The RS results in columns (5) through (8) of Table 5, shown by Figure 3b, however, tell a different story. In all four samples considered, we observe instead a nonlinear, predominantly positive effect as a residency's change in distance to a nearest BID increases. Expanding the sample to a one or two mile radius of the BIDs results in a strictly positive effect, as predicted by the insignificant, negative coefficients in columns (7) and (8). The depreciation predicted in the case of the quarter and half mile samples also appears to be much smaller in absolute and relative terms. These results, in general, contrary to both the original findings and the falsification test under a hedonic price specification, predict that homes more distant from locations where BIDs counterfactually develop four years sooner appreciate in value more than homes closer to such BIDs. In other words, few, if any, positive spillovers are predicted in this scenario. Given the advantages associated with the use of RS observations in mitigating omitted variable concerns, as well as the mitigated concern of omitted variables bias in the case of this particular identification strategy, I attribute more credence to this set of falsification test results than the set obtained from the hedonic price estimates.

5 Conclusion

This paper aims to examine the effect of localized public good provision on the greater community located within, and surrounding areas directly affected. It focuses particularly on BIDs, entities created by the vote of commercial property owners and tenants comprising such, which use additional taxes from such stakeholders to provide additional amenities to make the region more economically marketable. It follows from a previous line of literature, which posits evidence that the benefit of amenities provided by BIDs are reflected by lowered crime incidence and higher appreciation for commercial properties within BIDs.

This work contributes to this literature by concentrating on the residential property market, using several identification strategies to assess how amenities provided by BIDs are capitalized into such. Several noteworthy findings emerge from this exercise. First, homes located within BIDs appear to appreciate more relative to homes within the rest of the DC area; much of this appreciation, however, appears to be the result of more local-level price trends. Once accounted for, such appreciation appears less evident. Second, in reducing the sample to homes within defined proximities of the borders of BIDs, homes within BIDs do not appear to appreciate significantly more than their untreated neighbors, following the anticipation or launch period of a BID. Third, evidence does not rule out the possibility of spillover effects. Homes proximal to BIDs, following their establishment, observe more appreciation in home values than homes which are further distant from such. This last finding is supported by the use of RS observations, and with such is unable to be replicated by a falsification exercise.

It would make sense, as these findings suggest, that BIDs offer communal benefits to residential homes in the greater community. As is also the case with previous papers assessing the effect of BID amenities, however, it is important to be cautious in drawing causal inferences from these findings. Although several measures are taken in this paper to mitigate the concern of confounding factors, the fact remains that the BID movement was not an isolated venture, but one of several measures taken to revitalize the greater downtown DC area. To the extent that the launch of BIDs in particular geographic regions correlates closely with the timing and location of other revitalization efforts, the observed spillover effects may perhaps be additionally accounting for such activity.

Although several previously unused techniques are employed for assessing BIDs in this paper, many questions remain unanswered concerning BIDs, and their effect on surrounding communities. For example, BIDs are arguably heterogeneous, both within and between cities. It remains to be shown how some of these differences affect measured outcomes of interest. Another unanswered question concerns how BIDs, in themselves, change the composition of neighborhoods, and to what extent this change may help to explain the positive externalities BIDs are believed to generate. Such changes may be social or demographic in nature, or economic to the extent that they affect what types of businesses operate within or near the area. Such topics I leave for consideration in future research.

Appendix 1. Additional information about data

Hedonic price regression

Appendix Table 1A provides the remaining results for the hedonic regression for the specification displayed in Table 2, column (1), in which all sample observations available are used.

Differences in mean characteristics across the BID border

Appendix Table 2A lists difference in means results for the dependent variable and additional covariates for two different samples: one for homes identified by GIS as being within 25m of a BID's border on either side, and one for homes identified as being within 200m if such.

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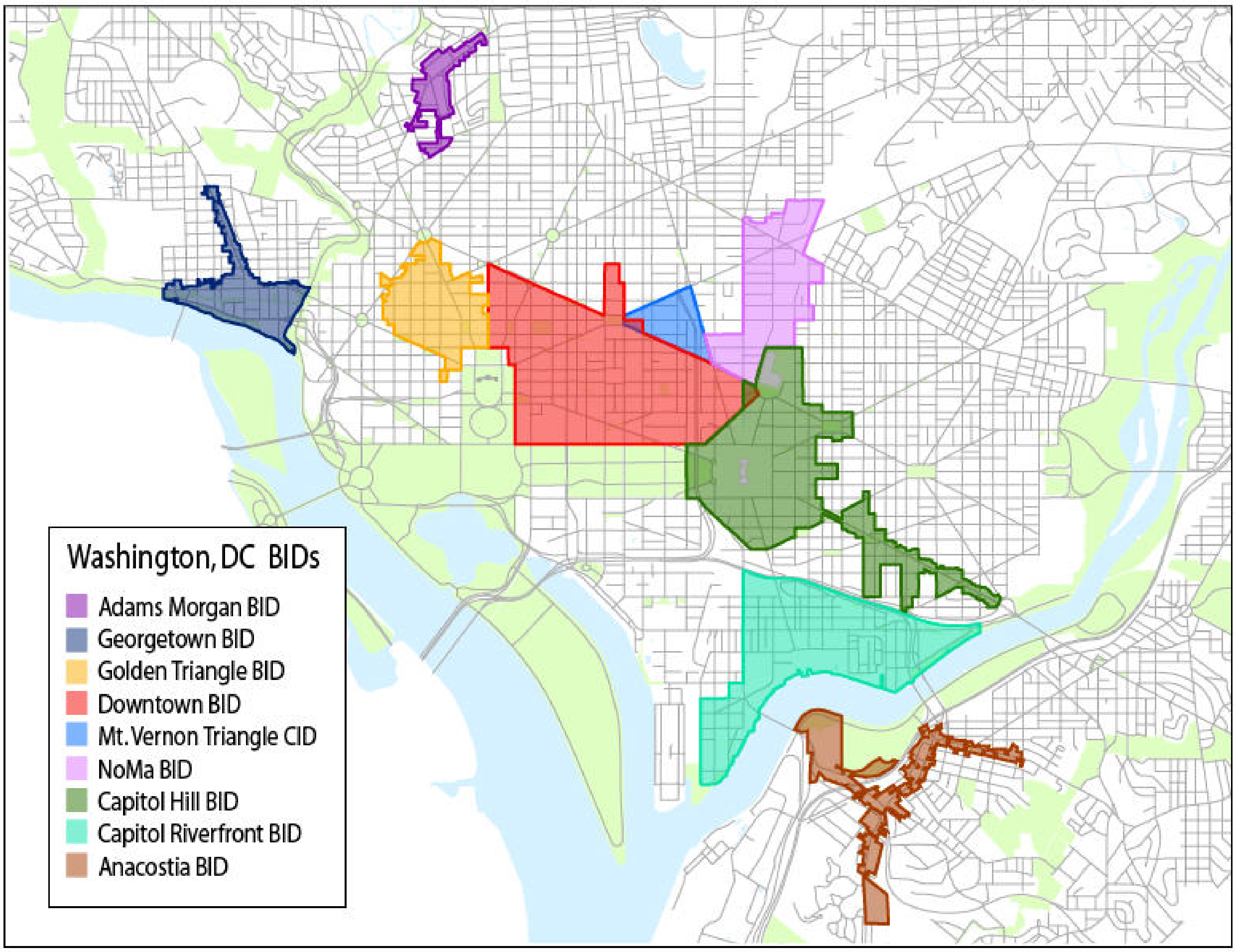
Figure 1. Map of the Business Improvement Districts in District of Columbia



Source: DC BID Council, 2012. Accessed online May 30, 2013: http://www.dcbidcouncil.org/map/.

Figure 2a. Predicted spillover effect of distance to the nearest BID (miles) on residential property values, using hedonic price estimates

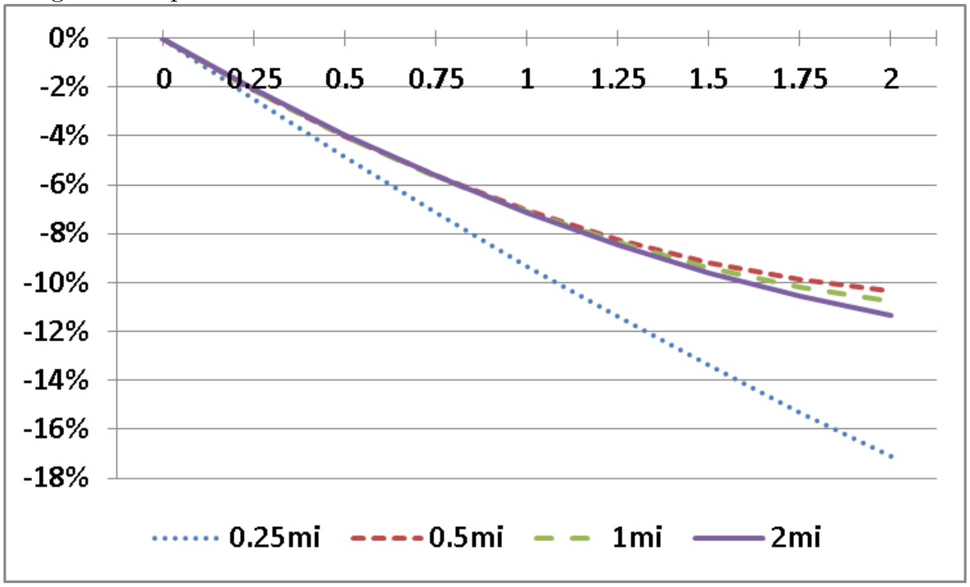


Figure 2b. Predicted spillover effect of a change in distance to the nearest BID (miles) on residential property values, using repeat sale estimates

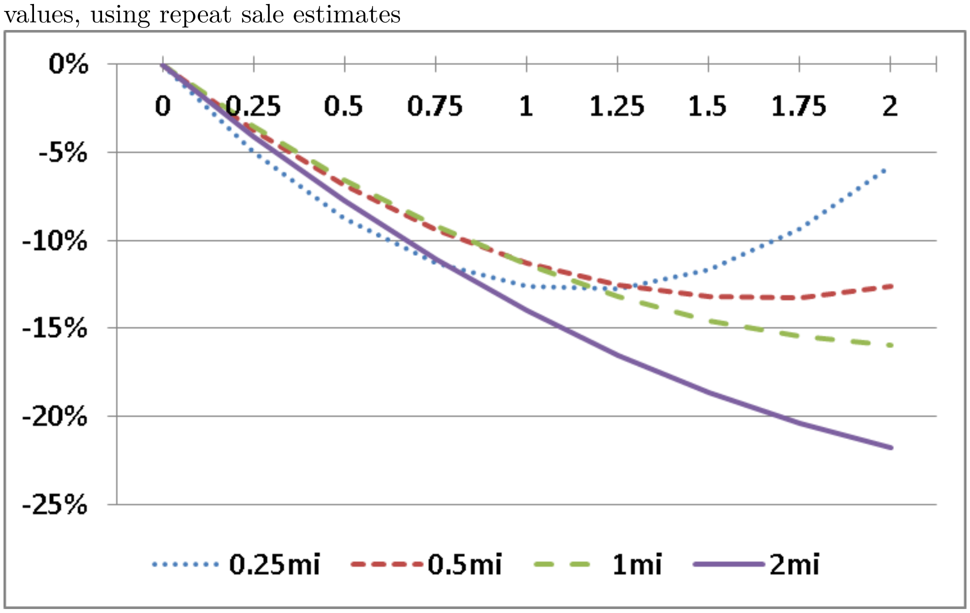


Figure 3a. Predicted spillover effect, from falsification exercise, of distance to the nearest BID (miles) on residential property values, using hedonic price estimates

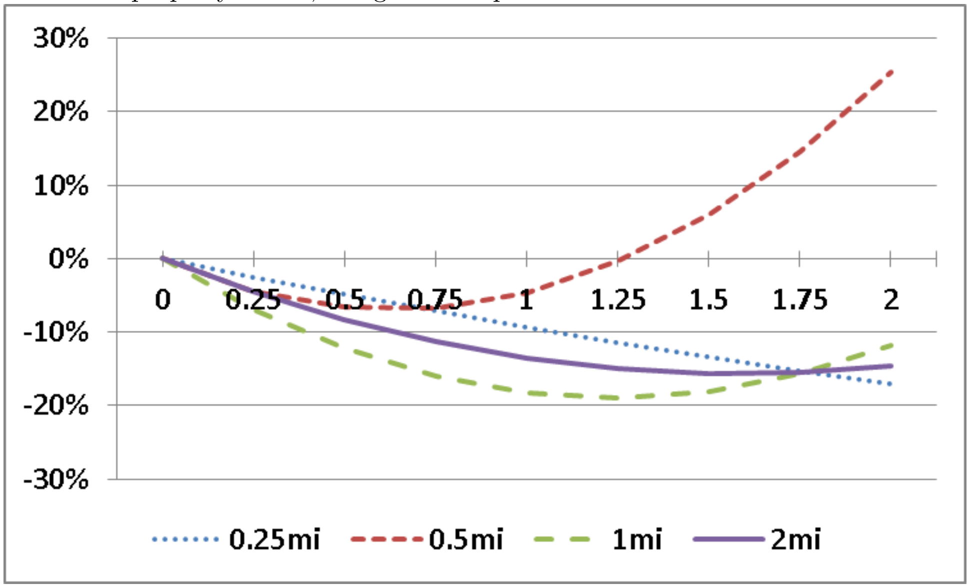
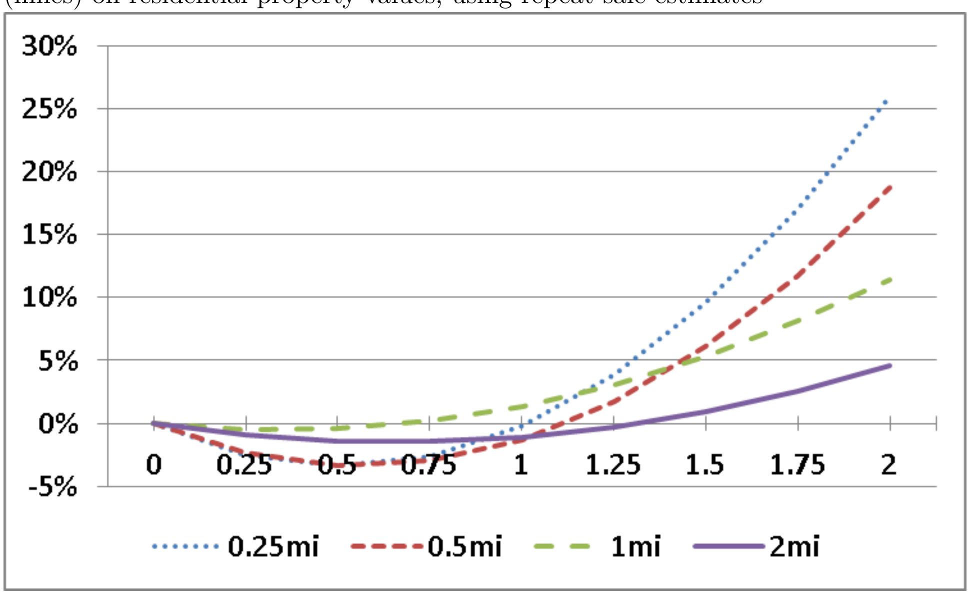


Figure 3b. Predicted spillover effect, from falsification exercise, of a change in distance to the nearest BID (miles) on residential property values, using repeat sale estimates



Note: the falsification exercise operates by assuming that BIDs are launched 4 years prior to the recorded month and year of the actual launch. For hedonic price estimates, observations are omitted for which the distance to the nearest BID is identical to the distance when the dates of BID launches are not altered. For repeat sale estimates, observations are omitted for which the difference in distance to the nearest BID between sales remains equal to the difference observed when the dates of the BID launches are not altered.

Table 2. Descriptive statistics

	Full sample (n=42176)				Difference in means					
	mean	standard error	min	max	Untreated (n=41553)		Treated (n=623)		Difference	
Log price	12.639	(0.004)	5.521	17.469	12.635	(0.004)	12.942	(0.028)	0.308***	(0.034)
Distance to rail station	0.388	(0.001)	0.005	1.246	0.392	(0.001)	0.147	(0.003)	-0.244***	(0.01)
Log land area	7.846	(0.003)	5.375	11.957	7.859	(0.003)	7.015	(0.018)	-0.844***	(0.028)
Log gross building area	7.911	(0.002)	5.529	10.076	7.914	(0.002)	7.707	(0.018)	-0.207***	(0.019)
Number of stories	2.081	(0.002)	0	9	2.077	(0.002)	2.359	(0.02)	0.282***	(0.016)
Number of bathrooms	1.981	(0.005)	0	11	1.981	(0.005)	2.006	(0.035)	0.026	(0.04)
Number of half bathrooms	0.667	(0.003)	0	6	0.666	(0.003)	0.706	(0.02)	0.04	(0.025)
Number of fireplaces	0.747	(0.004)	0	13	0.747	(0.004)	0.78	(0.04)	0.033	(0.037)
Number of rooms	7.042	(0.009)	0	30	7.044	(0.009)	6.923	(0.085)	-0.121	(0.075)
Number of kitchens	1.045	(0.001)	0	5	1.045	(0.001)	1.035	(0.008)	-0.009	(0.009)
Estimated year built	1963.526	(0.082)	1900	2012	1963.393	(0.082)	1972.387	(0.957)	8.994***	(0.677)
Remodeled	0.356	(0.002)	0	1	0.354	(0.002)	0.467	(0.02)	0.113***	(0.019)
Sale year	2002.312	(0.028)	1992	2012	2002.289	(0.028)	2003.876	(0.24)	1.588***	(0.229)
Row house	0.52	(0.002)	0	1	0.514	(0.002)	0.939	(0.01)	0.425***	(0.02)
Condition: average	0.474	(0.002)	0	1	0.476	(0.002)	0.294	(0.018)	-0.183***	(0.02)
Grade: average	0.588	(0.002)	0	1	0.59	(0.002)	0.47	(0.02)	-0.12***	(0.02)
Brick exterior wall	0.834	(0.002)	0	1	0.833	(0.002)	0.9	(0.012)	0.068***	(0.015)
Hardwood Interior wall	0.886	(0.002)	0	1	0.887	(0.002)	0.817	(0.016)	-0.07***	(0.013)
Shingle Roof	0.296	(0.002)	0	1	0.3	(0.002)	0.01	(0.004)	-0.29***	(0.018)
Hot water rad heating	0.381	(0.002)	0	1	0.381	(0.002)	0.395	(0.02)	0.014	(0.02)

Notes: standard errors are reported in parentheses; the symbols *, **, and *** respectively denote statistical significance at the 10%, 5%, and 1% levels; with difference in means, the term "treated" is used to denote homes which are located in an area encompassed by a BID; the tests for statistical significance are performed using the assumption of unknown, but equal variance between the treated and untreated groups.

Table 2. Regression results, using full and subsets of the total sample

	(1)	(2)	(3)
Treated	0.0132	0.105***	0.107***
	(0.0243)	(0.0248)	(0.0256)
Treated x Antic	0.106	-0.0349	-0.0187
	(0.065)	(0.0656)	(0.0689)
Treated x After	0.0575*	-0.118***	-0.106***
	(0.0294)	(0.0298)	(0.0305)
Ward-year fixed effects?	no	yes	yes
Sample reduced to homes within wards including Capitol Hill and Adamas Morgan BIDs?	no	no	yes
Census tract fixed effects?	yes	yes	yes
Observations	42,176	42,176	10,889
R^2	0.9	0.91	0.877

Notes: robust standard errors in parentheses; the symbols *, **, and *** respectively denote statistical significance at the 10%, 5%, and 1% levels; the dependent variable is the log sale price of the observations.

Table 3. Regression results, with sample reduced to homes near BID borders

	(1)	(2)	(3)	(4)
Treated	0.0602*	0.0569*	0.0497*	0.0572**
	(0.0361)	(0.0296)	(0.0295)	(0.0256)
Treated x Antic	0.0565	0.0424	0.0953	0.0412
	(0.116)	(0.0956)	(0.0696)	(0.0595)
Treated x After	-0.0176	0.00353	-0.00417	-0.0241
	(0.0444)	(0.035)	(0.0333)	(0.029)
Ward-year fixed effects?	no	no	no	no
Considered distance around BID border	25m	50m	100m	200m
Census tract fixed effects?	no	no	no	no
Observations	731	1,412	2,572	4,376
R^2	0.909	0.898	0.885	0.889

Notes: robust standard errors in parentheses; the symbols *, **, and *** respectively denote statistical significance at the 10%, 5%, and 1% levels; the dependent variable is the log sale price of the observation.

Table 4. Regression results for assessing the spillover effect of BIDs on residential properties within considered distances of BIDs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Hedonic Price Estimates				Repeat Sale Estimates			
Distance to nearest BID (miles)	-0.101**	-0.0894***	-0.0883***	-0.0864***	-0.224**	-0.163***	-0.148***	-0.171***
	(0.0463)	(0.0346)	(0.0271)	(0.0191)	(0.0675)	(0.0504)	(0.0391)	(0.0425)
Distance^2	0.00775	0.0188*	0.0172**	0.0149***	0.975***	0.0499*	0.0340***	0.0310***
	(0.0179)	(0.0114)	(0.00812)	(0.00391)	(0.0392)	(0.0256)	(0.00937)	(0.00967)
Ward-year fixed effects?	no	no	no	no	no	no	no	no
Maximum distance of observations from any BID	0.25 miles	0.5 miles	1 mile	2 miles	0.25 miles	0.5 miles	1 mile	2 miles
Census tract fixed effects?	yes	yes	yes	yes	-	-	-	-
Observations	5.818	9,552	16,506	23,896	94	158	262	380
R^2	0.586	0.586	0.585	0.633	0.951	0.948	0.941	0.915

Notes: robust standard errors in parentheses; the symbols *, **, and *** respectively denote statistical significance at the 10%, 5%, and 1% levels; the dependent variable is the log sale price of the observation.

Table 5. Regression results for falsification exercise in assessing the spillover effect of BIDs on residential properties within considered distances of BIDs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Hedonic Price Estimates				Repeat Sale Estimates			
Distance to nearest BID (miles)	0.0719	-0.219*	-0.305***	-0.197***	-0.134**	-0.120**	-0.0298	-0.0449
	(0.237)	(0.131)	(0.0874)	(0.0622)	(0.0613)	(0.0580)	(0.0400)	(0.0334)
Distance^2	-0.0482	0.173**	0.123***	0.0619***	0.132***	0.107***	0.0435***	0.0339***
	(0.198)	(0.0760)	(0.0321)	(0.0144)	(0.0238)	(0.0237)	(0.0123)	(0.00738)
Ward-year fixed effects?	no	no	no	no	no	no	no	no
Maximum distance of observations from any BID	0.25 miles	0.5 miles	1 mile	2 miles	0.25 miles	0.5 miles	1 mile	2 miles
Census tract fixed effects?	yes	yes	yes	yes	-	-	-	-
Observations	1,460	2,563	4,575	6,997	162	269	426	614
R^2	0.629	0.630	0.618	0.664	0.943	0.938	0.926	0.900

Notes: robust standard errors in parentheses; the symbols *, **, and *** respectively denote statistical significance at the 10%, 5%, and 1% levels; the dependent variable is the log sale price of the home in the hedonic price specification, whereas for repeat sales it is the difference in log sale prices between the two sales; the falsification exercise operates by assuming that BIDs are launched 4 years prior to the recorded month and year of the actual launch. For hedonic price estimates, observations are omitted for which the distance to the nearest BID is identical to the distance when the dates of BID launches are not altered. For repeat sale estimates, observations are omitted for which the difference in distance to the nearest BID between sales remains equal to the difference observed when the dates of the BID launches are not altered.

Appendix Table 1A. Additional regression results for baseline model, using complete sample of residential, single family home observations

Log land area	0.156***	(0.00444)	Roof: composite shingle	0.0652**	(0.0327)
Log gross building area	0.237***	(0.00615)	Roof: built up	0.0624*	(0.0327)
Number of stories	0.0561***	(0.00438)	Roof: shingle	0.0473	(0.0373)
Number of bathrooms	0.0375***	(0.00226)	Roof: shake	0.0975***	(0.0356)
Number of half bathrooms	0.0325***	(0.00249)	Roof: metal-pre	0.100**	(0.0495)
Number of fireplaces	0.0425***	(0.00241)	Roof: metal-sms	0.0528	(0.0327)
Number of rooms	0.00643***	(0.00109)	Roof: metal-cpr	0.108	(0.103)
Number of kitchens	-0.00495	(0.00609)	Roof: composition ro	0.0237	(0.0657)
Age of original structure	-0.000510***	(9.62e-05)	Roof: concrete tile	0.0376	(0.0558)
Age squared	2.16e-07***	(4.82e-08)	Roof: clay tile	0.116***	(0.0359)
Remodeled	0.0861***	(0.00308)	Roof: slate	0.0874***	(0.0328)
Distance to rail station	-0.187***	(0.0357)	Roof: concrete	0.0196	(0.0630)
Distance squared	0.0250	(0.0290)	Roof: neoprene	0.0950***	(0.0349)
Condition: poor	0.289***	(0.0959)	Roof: water proof	-0.375	(0.321)
Condition: fair	0.330***	(0.0550)	Roof: wood-FS	-0.0528	(0.160)
Condition: average	0.435***	(0.0521)	Heat: forced air	0.0559	(0.151)
Condition: good	0.513***	(0.0522)	Heat: air-oil	0.00343	(0.159)
Condition: very good	0.610***	(0.0525)	Heat: wall furnace	0.0486	(0.155)
Condition: excellent	0.689***	(0.0543)	Heat: electric red	0.0152	(0.161)
Grade: fair quality	0.159	(0.277)	Heat: elec base brd	0.0377	(0.153)
Grade: average	0.207	(0.271)	Heat: water base brd	0.0415	(0.153)
Grade: above average	0.244	(0.271)	Heat: warm cool	0.0795	(0.151)
Grade: good quality	0.289	(0.271)	Heat: ht pump	0.0647	(0.151)
Grade: very good	0.349	(0.271)	Heat: evp cool	0.0636	(0.170)
Grade: excellent	0.451*	(0.271)	Heat : air exchng	-0.0384	(0.203)
Grade: superior	0.554**	(0.271)	Heat: gravity furnace	0.114	(0.157)
Grade: exceptional - A	0.689**	(0.272)	Heat: ind unit	0.141	(0.173)
Grade: exceptional - B	0.768***	(0.273)	Heat: hot water red	0.0356	(0.151)
Grade: exceptional - C	0.898***	(0.280)	Int wall: resilient	0.119	(0.104)
Grade: exceptional - D	0.994***	(0.281)	Int wall: carpet	0.00573	(0.0794)
Structure: single	-1.609***	(0.0784)	Int wall: wood floor	0.0323	(0.0791)
Structure: multi	-1.665***	(0.0906)	Int wall: ceramic tile	0.0143	(0.0906)
Structure: town End	-1.537***	(0.0901)	Int wall: terrazo	0.0557	(0.0849)
Structure: town Inside	-1.498***	(0.0878)	Int wall: hardwood	0.0381	(0.0790)
Structure: row End	-1.623***	(0.0863)	Int wall: parquet	-0.0767	(0.171)
Structure: row Inside	-1.612***	(0.0862)	Int wall: vinyl comp	-0.341***	(0.0820)
Structure: semi-detached	-1.647***	(0.0899)	Int wall: vinyl sheet	-0.0868	(0.0868)
Use code: detached	-0.0110	(0.0363)	Int wall: lt concrete	0.0833	(0.0897)
Use code: semi-detached	0.00472	(0.0269)	Int wall: hardwood/carp	0.0249	(0.0790)

Appendix Table 1A (continued)

Ext wall: plywood	-0.425	(0.328)	Sale year: 1993	0.00209	(0.00917)
Ext wall: hardboard	-0.200**	(0.0954)	Sale year: 1994	-0.0101	(0.00902)
Ext wall: metal siding	-0.180**	(0.0822)	Sale year: 1995	-0.0571***	(0.00956)
Ext wall: vinyl siding	-0.202***	(0.0494)	Sale year: 1996	-0.0559***	(0.00962)
Ext wall: stucco	-0.151***	(0.0492)	Sale year: 1997	-0.0595***	(0.00955)
Ext wall: wood siding	-0.163***	(0.0490)	Sale year: 1998	-0.0292***	(0.00967)
Ext wall: shingle	-0.168***	(0.0502)	Sale year: 1999	0.103***	(0.0129)
Ext wall: brick veneer	-0.0474	(0.0503)	Sale year: 2000	0.214***	(0.00837)
Ext wall: stone veneer	-0.249***	(0.0579)	Sale year: 2001	0.316***	(0.00916)
Ext wall: concrete block	-0.146**	(0.0604)	Sale year: 2002	0.537***	(0.00878)
Ext wall: stucco block	-0.0750	(0.0671)	Sale year: 2003	0.711***	(0.00796)
Ext wall: common brick	-0.153***	(0.0487)	Sale year: 2004	0.899***	(0.00795)
Ext wall: face brick	-0.128**	(0.0616)	Sale year: 2005	1.106***	(0.00773)
Ext wall: adobe	0.0387	(0.0556)	Sale year: 2006	1.160***	(0.00777)
Ext wall: stone	-0.128**	(0.0509)	Sale year: 2007	1.165***	(0.00774)
Ext wall: concrete	-0.126	(0.0798)	Sale year: 2008	1.113***	(0.00784)
Ext wall: aluminum	-0.204***	(0.0510)	Sale year: 2009	1.061***	(0.00749)
Ext wall: brick/stone	-0.174***	(0.0515)	Sale year: 2010	1.051***	(0.00755)
Ext wall: brick/stucco	-0.173***	(0.0501)	Sale year: 2011	1.041***	(0.00813)
Ext wall: brick/siding	-0.184***	(0.0490)	Sale year: 2012	0.933***	(0.0198)
Ext wall: stone/stucco	-0.147***	(0.0516)	Constant	10.27***	(0.319)
Ext wall: stone/siding	-0.186***	(0.0539)			
Observations		42,176			
R^2		0.900			

Notes: robust standard errors in parentheses; the symbols *, **, and *** respectively denote statistical significance at the 10%, 5%, and 1% levels; the dependent variable is the log sale price of the observation; these results correspond to the baseline specification provided in Table 2, column (1).

Appendix Table 2A. Differences in means for 25m and 200m border samples

	25m sample						200m sample					
	Untreated (n=540)		Treated (n=191)		Difference		Untreated (n=3788)		Treated (n=588)		Difference	
Log price	12.824	(0.036)	12.758	(0.048)	-0.066	(0.066)	12.79	(0.014)	12.931	(0.029)	0.142***	(0.037)
Distance to rail station	837.41	(22.411)	566.358	(23.669)	-271.052***	(40.236)	790.383	(8.175)	477.356	(10.757)	-313.027***	(21.179)
Log land area	7.344	(0.021)	7.132	(0.03)	-0.212***	(0.039)	7.3	(0.008)	7.033	(0.019)	-0.267***	(0.021)
Log gross building area	7.664	(0.02)	7.6	(0.035)	-0.064	(0.04)	7.675	(0.008)	7.701	(0.019)	0.026	(0.021)
Number of stories	2.209	(0.022)	2.151	(0.031)	-0.058	(0.041)	2.202	(0.008)	2.343	(0.021)	0.141***	(0.022)
Number of bathrooms	1.789	(0.038)	1.639	(0.053)	-0.15**	(0.072)	1.83	(0.015)	1.978	(0.036)	0.148***	(0.041)
Number of half bathrooms	0.637	(0.025)	0.602	(0.038)	-0.035	(0.047)	0.663	(0.009)	0.699	(0.021)	0.036	(0.025)
Number of fireplaces	0.996	(0.044)	1.084	(0.089)	0.087	(0.09)	0.956	(0.017)	0.823	(0.042)	-0.133***	(0.047)
Number of rooms	6.487	(0.077)	6.246	(0.132)	-0.241	(0.152)	6.492	(0.029)	6.828	(0.086)	0.337***	(0.081)
Number of kitchens	1.07	(0.013)	1.052	(0.019)	-0.018	(0.024)	1.061	(0.004)	1.037	(0.009)	-0.024**	(0.011)
Estimated year built	1963.443	(0.577)	1960.948	(0.864)	-2.495**	(1.098)	1963.144	(0.215)	1971.063	(0.953)	7.919***	(0.662)
Remodeled	0.52	(0.022)	0.592	(0.036)	0.071*	(0.042)	0.519	(0.008)	0.485	(0.021)	-0.034	(0.022)
Sale year	2001.902	(0.248)	2002.073	(0.415)	0.171	(0.484)	2002.11	(0.095)	2003.629	(0.249)	1.519***	(0.26)
Row house	0.859	(0.015)	0.906	(0.021)	0.046*	(0.028)	0.875	(0.005)	0.935	(0.01)	0.06***	(0.014)
Condition: average	0.415	(0.021)	0.393	(0.035)	-0.022	(0.041)	0.425	(0.008)	0.304	(0.019)	-0.121***	(0.022)
Grade: average	0.472	(0.022)	0.586	(0.036)	0.114***	(0.042)	0.561	(0.008)	0.488	(0.021)	-0.073***	(0.022)
Brick exterior wall	0.881	(0.014)	0.859	(0.025)	-0.023	(0.028)	0.884	(0.005)	0.895	(0.013)	0.011	(0.014)
Hardwood Interior wall	0.798	(0.017)	0.728	(0.032)	-0.07	(0.035)	0.798	(0.007)	0.811	(0.016)	0.013	(0.018)
Shingle Roof	0.041	(0.009)	0.021	(0.01)	-0.02	(0.016)	0.048	(0.003)	0.01	(0.004)	-0.038***	(0.009)
Hot water rad heating	0.415	(0.021)	0.482	(0.036)	0.067	(0.042)	0.42	(0.008)	0.41	(0.02)	-0.01	(0.022)

Notes: standard errors are reported in parentheses; the symbols *, **, and *** respectively denote statistical significance at the 10%, 5%, and 1% levels; with difference in means, the term “treated” is used to denote homes which are located in an area encompassed by a BID; the tests for statistical significance are performed using the assumption of unknown, but equal variance between the treated and untreated groups.