Received: 1 June 2022 | Revised: 10 March 2023 | Accepted: 28 May 2023

DOI: 10.1111/jors.12655

RESEARCH ARTICLE

JOURNAL OF REGIONAL SCIENCE WILEY

The effect of open space maintenance spending on house prices

David M. Brasington

Economics Department, University of Cincinnati, Cincinnati, Ohio, USA

Correspondence
David M. Brasington, Economics Department, University of Cincinnati, Cincinnati, OH 45221-0371, USA.
Email: david.brasington@uc.edu

Funding information
Charles Phelps Taft Research Center; Lindner College of Business; University of Cincinnati

Abstract

We study the effect on housing values of cutting funding for the maintenance of local parks and recreational areas. It is the first study we find on house prices and park maintenance spending, and only the second open space study we find that uses regression discontinuity. We study tax votes with exogenous timing for renewing current expense spending on parks and recreation, adding to the vibrant literature on house price capitalization of environmental amenities. We find that otherwise similar communities that barely vote to cut taxes suffer an 11% drop in house prices, compared to communities that barely vote to renew tax funding. The capitalization discount grows to 13% and 16% in later periods. Voting against spending saves $70 a year for a typical house but cuts house values by over $30,000. We find stronger effects for large tax levies and more expensive houses.

KEYWORDS

capitalization, environmental valuation, house price hedonic, local government taxation, provision of local public goods

1 | INTRODUCTION

Communities in the United States spend about $40 billion on parks and recreation (US Census Bureau, 2018), a figure that has increased in real terms over time. Access to open space like parks has been linked to diverse types of psychological well-being (e.g., Fuller et al., 2007). People Tiebout (1956) sort to regions with attractive open space amenities (Brasington, 2017a), buying houses that match willingness to pay for open space with housing suppliers' offer functions at the efficient level of open space provided (Yinger, 2015).

Numerous studies have examined the effect of various types of open space on housing prices. These include scenic views, proximity to parks, size of open space, the preservation of open space, and the renovation of parks. We identify a gap in the literature: the effect on house prices of cutting funding for the maintenance of local parks and recreational areas.

We collect a unique data set of tax levies to renew current expense funding of parks and recreational areas in communities across the state of Ohio from 1991 to 2016. We argue that the timing of a new tax is an endogenous choice, so we restrict attention to the set of votes to renew an expiring tax, the timing of which is exogenous. We focus on levies for current expenses, which are for the maintenance, operation, and repair of parks and recreational areas—categories of expenditures like mulch whose effects have an estimated life of less than 5 years. We match the votes to actual house transaction prices and various characteristics of the community's park district to assess the effect of cutting park and recreation spending on house prices.

Hundreds of studies look at the value that open space provides to housing. They have used all manner of methodologies to identify their estimates. We use regression discontinuity to compare otherwise equivalent communities—some of which barely pass a tax levy, some of which barely fail it—to find that the difference in house prices between these communities is about 13%. The difference is not driven by differences in economic vitality between communities that pass and fail their tax levies. Yes, house prices and economic vitality are surely higher in communities that pass tax levies, but what matters is the characteristics of communities near the 0.50 vote cutoff. Within the effective bandwidth of the cutoff, economic indicators like income levels, the unemployment rate, and the labor force participation rate are the same between these groups. The only discernable difference is whether the community renews the funding or not. Ours is only the second house price and open space study we find that uses the causal inference technique of regression discontinuity (Lang, 2018).

This house price discount becomes evident 3 years after the tax levy and strengthens steadily in successive years to 16%. It is only a rational conjecture, but the increased discount is consistent with worsening park conditions over time. By voting to cut park funding, the typical house saves \$70 a year, at a cost of over \$30,000 in lost house value. Our results complement those of Livy and Klaiber (2016), which find a capitalized value of certain types of major park renovations that declines with time. In our case, decreased maintenance and repair seem not to be noticeable immediately, but the housing market reflects a discount 3 years after decreased maintenance, a discount that grows larger with time. We find this discount is more pronounced for larger tax levies and for more expensive houses.

2 | LITERATURE REVIEW

2.1 | Maintenance spending on parks as key factor

We examine the effect on house prices of cuts in park maintenance spending. Much of the literature focuses on the effect of the proximity of houses to open space amenities. The typical finding is that proximity to open space imparts a premium to house prices that decline with distance (Anderson & West, 2006; Franco & MacDonald, 2018; Lutzenheiser & Netusil, 2001; Parent & vom Hofe, 2013; Song & Knaap, 2004; vom Hofe et al., 2018; Wu & Dong, 2014). Distance away from disamenities is similarly reflected in house prices (Cohen & Coughlin, 2008; Jauregui et al., 2017; McMillen & Redfearn, 2010; vom Hofe et al., 2019).

Other factors besides proximity to housing have been studied, like acreage of parks and recreational areas (Clark & Cosgrove, 1990), sudden changes in the availability of open space (Ohler & Blanco, 2017) and the official preservation of undeveloped open space (Black, 2018; Fernandez et al., 2018; Lang, 2018). Like our study, we find two other studies that consider the effect of parks and recreation spending on house prices: Schroeder (1982) and

Stadelmann (2010). They do not, however, distinguish between capital expenditures and current expenditures. Carlino and Saiz (2019) look separately at capital and operating expenditures on recreational areas, but its outcome variable is the number of visits, not housing prices.

Livy and Klaiber (2016) look at major renovations of existing recreational facilities. This study is probably the most closely related to ours, although the capital renovations it studies are the opposite of the decreased maintenance that we study. Its house-specific fixed effects model employs 83,246 sales in Baltimore from 2000 to 2007, finding no link between house prices and proximity to parks overall. On the other hand, it finds increased house prices as a result of playground replacement, trail renovation, and fence renovation. Consistent with a depreciation story, and important for our study, it finds that the capitalized value of these major renovations declines with time, just like we find an increased discount over time from reduced maintenance spending.

2.2 | Estimation approach in open space literature

Ordinary least squares (OLS) is the traditional estimation approach, often with spatial fixed effects added to help with omitted variable bias. Spatial econometric approaches have also been applied (Cohen & Coughlin, 2008; Ho & Hite, 2008; Ohler & Blanco, 2017; Parent & vom Hofe, 2013; vom Hofe et al., 2018). Another set of studies relies on instrumental variables to achieve identification (Irwin & Bockstael, 2001; Song & Knaap, 2004).

Other studies use more idiosyncratic approaches. Abbott and Allen Klaiber (2013) use propensity score matching on observables and nearest neighbor covariate matching. Black (2018) uses a difference in differences estimator. Fernandez et al. (2018) combine repeat sales data with a matching estimator. Methodologically, Lang (2018) is the most similar to our study, as it also uses voting data with regression discontinuity. Its voting data are 1145 referenda for local governments to purchase development rights to undeveloped land or to purchase the land itself. Housing values are based on Zillow zip code-level house values. It finds positive capitalization into house prices, indicating that communities are inefficiently under-providing open space.

Regression discontinuity has drawbacks. Unlike sorting models, for example, regression discontinuity cannot yield welfare changes for marginal changes in an attribute. The hedonic approach using OLS can be used to analyze a wide range of geographies and attributes, but it is rare to find a situation that has a fixed, known cutoff that determines treatment status, which is a requirement of regression discontinuity. The results of a regression discontinuity study only apply to agents with characteristics within a narrow bandwidth of the cutoff, so the results may not be generalizable to a larger population.

On the other hand, we believe regression discontinuity has the potential to better identify estimates than other approaches. Even with a careful consideration of functional form and the inclusion of myriad important controls, OLS still may suffer from omitted variable bias. Spatial econometric approaches incorporate spatial dependence either as a spatial lag of the dependent variable, a spatial lag of the error term, or both. Doing so can improve hypothesis testing and/or bias, depending on how the spatial dependence is modeled, but it still leaves a model subject to bias from omitted variables that do not vary by space and from omitted variables that vary by space at a different level of aggregation than the researcher models. Difference in differences, fixed effects, and first differencing panel data methods control for the influence of omitted variables that do not vary over time, but time-varying omitted variables may still bias estimates. A natural experiment can be a great way to identify estimates, but treatment may be inadvertently correlated with an omitted variable that predicts outcomes. The estimates from instrumental variable techniques can be highly credible, but it is hard to be sure. The exogeneity of the instrument can only be assumed but not tested; a constant-coefficient interpretation of instrumental variable estimation is implausible in the presence of causal effect heterogeneity; and the "only through" assumption is difficult to satisfy and is itself rarely testable. Matching estimators can potentially mimic the estimates of a randomized experiment, but it is unclear how to handle the sampling weights, it is difficult to match on uncommon characteristics, and it cannot handle matching on potentially important unobservable characteristics. Shadish et al. (2008) find that the median reduction in bias from various matching methods is 70%, compared to an 89% reduction by OLS.

The gold standard in empirical work today is the randomized experiment. Regression discontinuity has been shown to produce estimates that are statistically indistinguishable from randomized experiments (Berk et al., 2010; Buddelmeyer & Skoufias, 2004; Shadish et al., 2011). Regression discontinuity creates a condition in which observations are as good as randomized within a narrow range of a cutoff value so that the only systematic difference between the control and treatment groups is treatment status. It follows that any difference in outcome can be attributed to treatment status, free from the influence of omitted variables, so that one can find a causal link between treatment and outcome, and not just an association. Unlike instrumental variables, for example, the assumptions behind regression discontinuity are empirically testable.

3 | INSTITUTIONAL BACKGROUND

We study Ohio, a US state with vast swaths of rural regions, three urban areas of about two million residents each, four more urban areas with 500,000–1,000,000 residents, and numerous small industrial cities isolated from large urban areas. Ohio consists of 88 counties, each of which was originally subdivided into about 15 townships of about 36 square miles each. Townships are communities that rely exclusively on the property tax for revenue. Villages may form when residents petition for incorporation, and when a village exceeds 5000 persons it becomes a city. Cities and villages generate revenue from both property and income taxes. Communities can cooperate in the provision of parks, called a joint recreation district, but typically a community provides its own local parks. Cities and villages are headed by a mayor and a council; townships are run by a board of trustees. Joint recreation districts are also headed by a board of trustees.

There are 128 cities, 131 townships, and 146 villages in the data set, many of which have multiple votes between 1995 and 2016. Throughout the paper, we use the term "communities" to refer to local park and recreation taxing authorities, encompassing cities, villages, townships, and joint recreation districts.

The relevant city council or board of trustees may petition voters for funding for parks and other forms of open space. A simple majority vote is required to pass the tax levy. These tax levies are typically set for a specific time period, the most common of which is 5 years. At the end of 5 years, the taxing authority will approach voters to renew the tax. If the tax levy garners ≤50% of votes in favor, the tax levy fails, the tax is no longer collected, and funding must be cut. Not all park funding comes from tax levies specifically designated for that purpose. Communities may fund parks with municipal current expense tax levies or a limited source of funds called inside millage that does not require voter authorization, but the tax levies in the current study are special purpose tax levies designated for parks and recreation, and by law the funding they generate may not be used for other purposes. Communities that vote against renewing a park tax levy may still have a functional parks department if the department relies partly on funding from current expense tax levies, inside millage, or other park tax levies that are still in effect; but if voters reject the tax, funding will be cut. In fact, we match our voting data set to City County Data Book information about park spending by communities over time. This source of data excludes certain observations, including most townships, but the remaining observations that match our voting data show an average 16% drop in current operating expenditures the year after a parks and recreation tax levy fails.

Section 57.05.01 of the Ohio Revised Code defines two types of tax levies. Those for permanent improvements are for expenditures to purchase property, assets, or improvements with an estimated life or usefulness of 5 years or more. This includes the acquisition of new land and the construction or major renovation of buildings and equipment on the land. We focus instead on current expenditures, which state law defines as expenditures that are not permanent improvements. Current expenditures of a park or recreation district that have an estimated life of less than 5 years include things like wages of park employees, supplies of gravel and mulch, the repair and maintenance of existing structures, and the maintenance of equipment and trails.

To vote in Ohio, one must be a resident aged 18 or older. Residents must have registered to vote, lived in Ohio for at least 30 days before the vote, and be US citizens. Residents may not vote if they have been declared incompetent by a court, been incarcerated for a felony conviction, or have violated election laws.

The State of Ohio maintains the largest nature preserves. The types of open space that form the basis of our voting data are tax-funded local parks and small recreational areas. They sometimes contain trails through fields and wooded areas. They sometimes abut bodies of water like rivers or ponds. They generally feature a playground area, picnic shelters, baseball fields, soccer fields, and basketball and tennis courts.

4 | METHODOLOGY

4.1 | Regression discontinuity

The regression discontinuity approach was invented by Thistlethwaite and Campbell (1960) but seldom used until recent decades and, as far as we can tell, only used once before in the literature on open spaces (Lang, 2018). The idea is simple: find a situation where treatment depends on whether a subject exceeds a critical threshold for some continuous variable. The groups near the threshold should be otherwise similar, except that one group receives treatment and the other does not. If a few conditions are met, any change in an outcome is solely caused by receiving treatment.

A formal model of regression discontinuity follows, drawing from Calonico et al. (2017, 2019). Let P_i represent the natural log of house prices in community *i*, where $P_i(0)$ is house prices for the control group: the group of communities that fails to renew its park spending tax levies. Let $P_i(1)$ be the natural log of house prices for the treated group, the group that successfully renews its tax levies. A vector called the running variable plays a critical role in regression discontinuity. It determines whether treatment is received, and at each value of the running variable agents should have similar characteristics like income levels and education levels. In regression discontinuity studies that use voting data, the running variable X_i is the proportion of votes in favor of the tax levy, called the vote share. Let $T_i \in \{0,1\}$ represent communities that receive treatment (1) or not (0), so that the observed outcome is represented by the equation:

$$P_i = P_i(0) \times (1 - T_i) + P_i(1) \times T_i. \tag{1}$$

Local voting in Ohio follows a simple majority rule, so let $\bar{x} = 0.50$ represent the cutoff value of X_i that determines whether the tax levy successfully renews or not. The difference between the outcomes for the treatment and control groups at the cutoff is the treatment effect τ, as in the below equation:

$$\tau = \tau(\bar{x}) = \mathrm{E}\,\{P_i(1) - P_i(0) \mid X_i = \bar{x}\}. \tag{2}$$

Theoretically, one would only use observations at the cutoff, but no statistically significant treatment effects would result because there are essentially no observations exactly at the cutoff. Researchers must balance the precision of using just the estimates at the cutoff with the need to include observations some distance away from the cutoff—but not so far away that the groups become different in observable characteristics. The proper bandwidth *h* around the cutoff was historically a matter of trial and error and robustness checking, but formal procedures based on objective criteria are now available (Calonico et al., 2019) and used in the current study.

Five different ways of calculating the optimal bandwidth illustrate the robustness of the treatment effect estimates to these choices. The "RD" option is the traditional way. It imposes the same bandwidth on either side of the cutoff. The "TWO" option allows different bandwidths on either side of the cutoff. The "SUM" option adds the bandwidths of the "RD" and "TWO" options, and the "COMB1" option selects the minimum of "RD" and "SUM." The

"COMB2" option uses the bandwidth that is the median of the estimates provided by the "RD," "TWO," and "SUM" options, while also allowing the bandwidth to differ on either side of the cutoff.

Once a proper bandwidth h around $\overline{x}$ is found, the treatment effect from Equation (2) can be estimated as in the below equation:

$$\tilde{\tau}(h) = e_0'\tilde{\beta}_{P+,D}(h) - e_0'\tilde{\beta}_{P-,D}(h). \tag{3}$$

In Equation (3), D is the polynomial order, the plus and minus symbols represent parameter estimates $\tilde{\beta}$ to the left and right of the cutoff, and e_o is a $(D+1)$ single-entry vector with 1 in the first position and zeroes in the rest. The estimates of β come from the minimization of $\tilde{\theta}$ in the below equation:

$$\tilde{\theta}_{P,D}(h) = \underset{\beta_-,\beta_+,\Lambda}{\operatorname{argmin}} \sum_{i=1}^{n} \left\{P_i - r_{-,D}(X_i - \overline{x})'\beta_- - r_{+,D}(X_i - \overline{x})'\beta_+ - W_i'\Lambda\right\}^2 K_h(X_i - \overline{x}). \tag{4}$$

In the above equation, $K(\cdot)$ is a kernel function with bandwidth h. Because we find that the data best fits a nonlinear function (Section 5.5), we adopt a triangular kernel to assign more weight to observations closest to the cutoff. The triangular kernel is the most commonly used in empirical work and is the MSE-optimal choice for point estimation (Cattaneo et al., 2019). Continuing the discussion of Equation (4), W is a vector of covariates included to reduce the standard error of the estimates, n is the number of observations, and the r terms are defined in the below equations:

$$r_{-,D}(x) = \mathbb{1}(x < 0)(1, x, \ldots, x^D)', \tag{5}$$

$$r_{+,D}(x) = \mathbb{1}(x \geq 0)(1, x, \ldots, x^D)'. \tag{6}$$

Calonico et al. (2019) find that the traditional way of estimating the treatment effect in Equation (3) suffers from bias when covariates are used, so the current study adopts its bias-corrected estimator, $\tilde{\tau}^{bc}(h, b) := \tilde{\tau}(h) - h^{D+1}\tilde{\beta}(b)$, where the bias-correction term $h^{D+1}\tilde{\beta}(b)$ requires a separate bandwidth sequence b. The bias-corrected estimator depends on the curvature of the conditional expectations of the covariates and the curvature of $E\{P_i(t)|X_i = x\}$. Because communities may experience more than one renewal vote during the timeframe of the sample, we cluster standard errors at the community level.

4.2 | Independence of votes

Economists who use voting data have recognized that votes may not be independent, violating a fundamental assumption of the classical linear regression model and invalidating results that rely on the central limit theorem. Lang (2018) and Hong and Zimmer (2016) are two examples of studies that recognize that communities might hold multiple votes, each of which might be related to prior votes or influence future votes. Each of these studies addresses the issue in a similar manner: by conditioning the treatment effect on a community's referenda history. The current study takes a different approach. The timing of any new tax is endogenously chosen by the taxing authority, so it is not only possible that a vote depends on prior votes, but that even an initial vote may be proposed at such a time as to maximize the probability of passage. These may be times of economic prosperity, after an influx of residents from a large new business relocating to the community, or after a social shock to the community. The endogeneity of the timing may bias treatment effect estimates.

We retain only the tax levies whose timing is exogenously set. For example, most park tax levies in Ohio are passed with a duration of 5 years. A new tax levy with this duration that is passed in 2003 is up for renewal in 2008, and is classified as a renewal tax levy by the Secretary of State. The timing of the tax is not chosen in 2008; it is exogenously set by decisions that were made in 2003. If the tax passes, it will be up for renewal at the date specified in the tax levy. If it fails, funding is cut, and the taxing authority may introduce a tax to replace it, but this will be classified as a new tax by the Secretary of State and not included in our sample. This is also the approach to independence taken by Brasington (2017b), which also uses Ohio data. The use of park tax levies with exogenous timing does not rule out the possibility that communities put new tax levies for nonpark purposes on the ballot with the set timing of park renewal levies in mind. Further research is warranted. Data on new tax levies and renewal tax levies cannot be combined in a single sample because the consequences of passage and failure are different: if a renewal levy passes, funding continues as normal; if a new tax levy passes, funding is increased. If a renewal levy fails, funding is cut; if a new tax levy fails, funding continues as normal.

5 | DATA

5.1 | Overview of data

The data comes from Ohio primarily because Ohio is one of the few states that makes historical local voting data available in a centralized location. Even so, Ohio is a reasonable geography to study. Its 2020 population is 11.8 million, which makes it the seventh most populous US state and more populous than all but 78 nations. Its 2020 gross domestic product is \$700 billion, which would rank 21st in the world, between that of Switzerland and Poland. Politically, economically, and geographically, it is hard to think of a state more representative of the United States.

Every tax levy for parks and recreation in Ohio from 1991 to 2016 is collected. Of the 1745 levies, 909 are initial tax levies with endogenously chosen timing; these are discarded from the sample to address the independence of votes, leaving 836 renewal levies with exogenous timing. For this reason, the sample only includes existing parks and recreational areas, and does not study the formation of new open space like Lang (2018) and Fernandez et al. (2018).

The tax levies are fairly evenly distributed between cities, townships, and villages, and across years. The typical duration of renewal is 5 years (71% of the sample). The votes include, for example, a 2007 vote in Burbank village, a 2013 vote in Attica village, and a 1991 vote in Bristol township, each of which might be a first-round renewal or a renewal of a tax initially passed decades ago. The data do not distinguish between types of open space. Funds from the 2007 Burbank tax will be distributed among Burbank's park and recreational areas in unspecified ways determined by the village's director of parks. This is consistent with other studies that look at the effect of spending on environmental facilities and improvements (Carlino & Saiz, 2019; Schroeder, 1982; Stadelmann, 2010).

5.2 | Running variable

The running variable "Percent in Favor," measuring the proportion of votes in favor of the tax, is collected from the voting data. It has a mean of 0.50 and a standard deviation of 0.02. Values of the running variable come from the parks and recreation tax levy votes by the 128 cities, 131 townships, and 146 villages during the sample period. A histogram of "Percent in Favor" is shown in Figure 1, showing a fairly normal distribution.

A concern in regression discontinuity is that some agent may be manipulating votes so that a lot of tax levies barely pass while significantly fewer barely fail, but a density test (Cattaneo et al., 2018) fails to reject the null of similar density, with a p-value of 0.63.

5.3 | Community-level covariates

The voting data are matched to US Census data that provides characteristics of the community. Readers familiar with least squares regression may be confused by the role of covariates in regression discontinuity. In OLS,

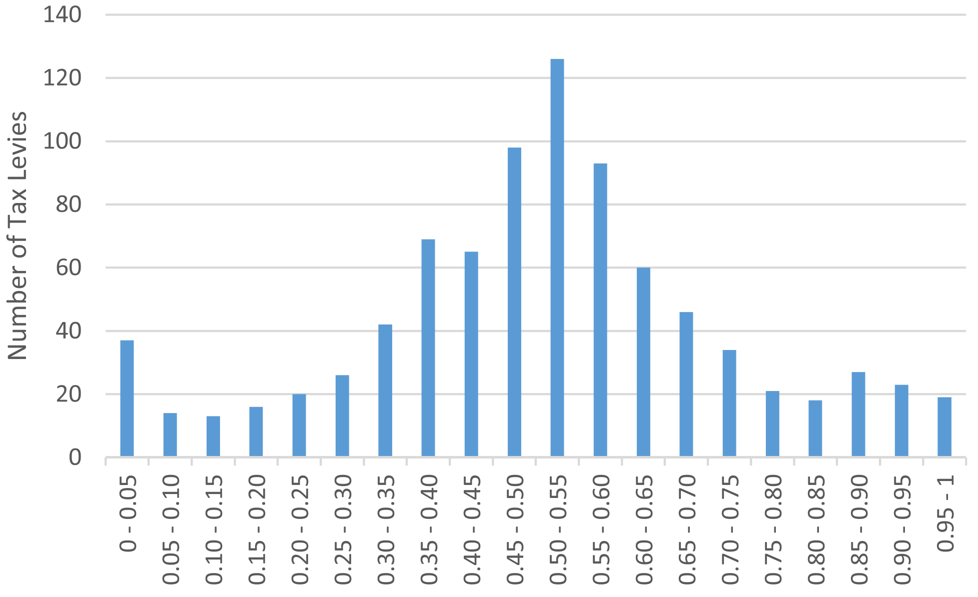


FIGURE 1 Histogram of "Percent in Favor" of renewing parks and recreation tax levies.

explanatory variables control for other factors to isolate the independent relationship between the dependent variable and the key explanatory variable.

In regression discontinuity, no covariates are necessary. One may regress an outcome variable solely as a function of the running variable and a treatment dummy and obtain fully identified estimates. Researchers typically include covariates not to control for omitted variable bias, but to increase the precision of the treatment effect estimates. In fact, these covariates need not be correlated with the outcome variable at all, but they should be correlated with the running variable, the outcome variable, or both (Lee & Lemieux, 2010, p. 293). In sum, covariates in regression discontinuity should be chosen so that they are not themselves outcome variables, as explained in Section 5.5, but so that they increase the precision of the treatment effect estimates.

The Census characteristics listed in Table 1 include "Population," the number of persons in the community. Next, "% With Kids" measures the proportion of households in the community with own children under age 18 living with them. "Unemployment Rate" and "Labor Force Participation Rate" are included to help capture the economic health of the community. "Income" is median family income in the community in constant 2010 US dollars. The age distribution of the community is captured by the variables "% Under 5 Years" and "% Age 5–17," the demographics most likely to use a park extensively.

5.4 | Housing data

The Census and voting data are merged with a housing transaction data set from CoreLogic that includes all single-family house transactions in Ohio from 1995 to 2016, over 3.6 million observations. The regression discontinuity study of Lang (2018) uses Zillow zip-code averages for transaction price and house characteristics, which we believe are fine for approximating individual sale prices and house characteristics. But since our data set has it available, our outcome variable "House Price" measures individual transaction prices, deflated to constant 2010 US dollars.

TABLE 1 Variable means by tax levy renewal status.

Outcome variable	Failed levies	Passed levies
House price (2010 $US)	190,509	229,619
Covariates		
Population	24,943	23,982
% With kids	0.41	0.40
Unemployment rate	0.04	0.04
Income ($10,000 s)	7.75	8.22
% Under 5 years	0.07	0.07
% Age 5–17	0.20	0.20
Labor force participation rate	0.66	0.66
House size	2036	2060
House age	33	32
Stories	1.6	1.6
Excellent condition	0.02	0.01

Note: Sample means shown at the time of the recreation tax levy vote. Global number of observations = 233,928; local number of observations in effective bandwidth = 23,937. Means shown for effective bandwidth year after tax levy vote for RD bandwidth option.

Other individual house characteristics also act as covariates. The variable "House Size" measures the square footage of interior floor space in the house; "House Age" measures how many years old the house is at the time of the transaction; "Stories" measures the number of stories in the house; and "Excellent Condition" is a dummy variable for whether the house is deemed by the county auditor to be in excellent condition.

An arm's length transaction is one in which the parties to the transaction are not related to each other. Dropping observations that represent transactions between family members helps ensure that remaining sales reflect market prices. To this end, we drop house sales where transaction type is coded "nominal," and we drop sales where document type is coded "gift deed," "interfamily deed transfer," or "quit claim." Even after dropping such indicators, the data contains house sales as small as $490. It also contains sales as high as $48 million. To achieve a data set of comparable houses and better approximate a normal distribution, we drop the 5% tails in house price. This reduces skewness from 18 to 1 and kurtosis from 414 to 4; this compares to a normal distribution's skewness of 0 and kurtotis of 3. The resulting data set contains 233,928 houses sold during years in which communities hold a renewal tax levy for parks and recreation purposes. The geographical distribution of house sales is shown in Figure 2.

In Figure 2, purple and blue dots represent houses sold in communities passing tax levies with a "Percent in Favor" vote share between 0.50 and 0.60, and yellow and red dots represent houses sold in communities with tax levies that failed with a vote share between 0.50 and 0.40. The black administrative boundary lines demarcate community boundaries.

5.5 | Tests of the appropriateness of the data

Table 1 shows variable means for the effective bandwidth split by whether or not the community passes the tax levy renewal. It is important for the vote-passing and failing communities close to the cutoff to be comparable to each other, so that the only meaningful difference between them is whether the levy passes or fails. On the other

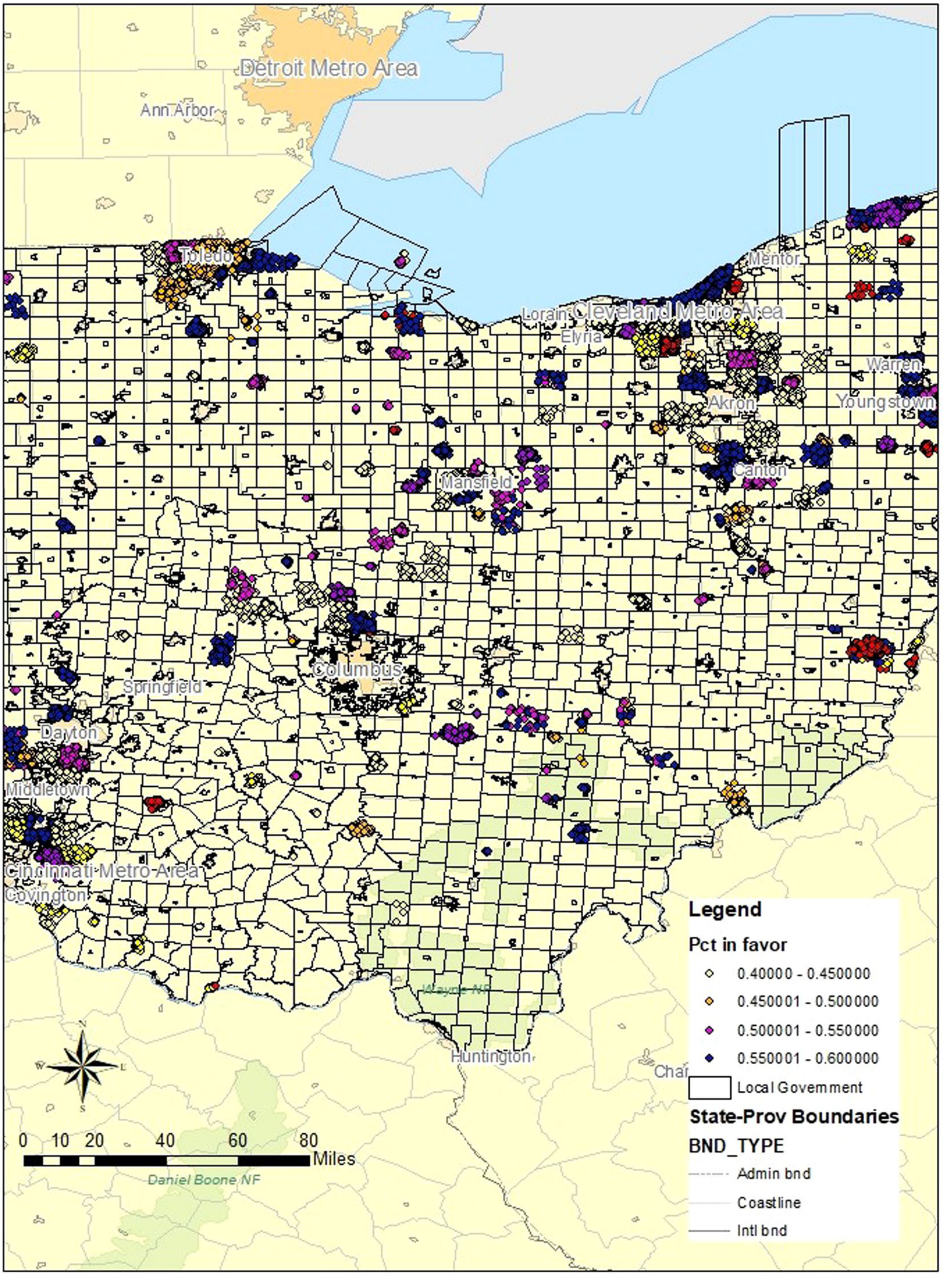


FIGURE 2 Geographical distribution of house sales in years with tax levies for parks and recreation.

hand, the outcome variable, "House Price," need not be comparable between pass and fail groups. Table 1 shows higher prices at time t for the set of communities that successfully renew their levies, but the treatment effect measures the change in house prices that results in later years.

Gelman and Imbens (2019) show that local linear and quadratic polynomials are more appropriate than higher-order polynomials in regression discontinuity studies. Recent work by Pei et al. (2021) creates an asymptotic mean squared error estimator to help determine whether a local linear or quadratic form better fits the data. This estimator shows that a quadratic form is the better choice for our data (MSE of 0.016 vs. 0.027), so a squared term is used for D in Equation (3) and a cubic term for b to estimate the bias-correction term $h^{D+1}\tilde{\beta}(b)$. This determination is further supported by comparing the adjusted R^2 and AIC of regressions that are linear and quadratic in vote share.

We provide a plot of the outcome variable as a function of the running variable in Figure 3 using $D = 2$ for the third year after the vote, the year for which significant treatment effects are first seen. There may be a discontinuity at 0.50, but the figure only uses binned observations from the raw data. Regressions that control for the running variable are necessary to formally test for a significant treatment effect.

In regression discontinuity, the running variable "Percent in Favor" captures the value of community characteristics, so that communities that vote 37% in favor of the tax have similar levels of income, population, and so forth, and that communities that vote 75% in favor likewise have similar levels of income and population. Critically, communities that vote 50% in favor also have similar characteristics, so that the only factor that changes in a discontinuous manner is whether the community is treated or not. It follows that the characteristics of the community should evolve slowly at the cutoff, too, so that what might look like a statistically significant treatment effect is not in fact an artifact of a jump in a covariate causing a change in the outcome. To this end, the outcome variable "House Price" in Equation (3) is replaced by each covariate in Table 1 one at a time and a regression is run. No statistically significant treatment effects are found, suggesting that the covariates evolve smoothly at the cutoff. The graphs associated with these regressions are shown in the Appendix. This testing includes variables related to economic conditions, so there is indeed random assignment of economic conditions between treated and control groups within a narrow bandwidth of the cutoff.

6 | RESULTS

6.1 | Main results

Table 2 shows the treatment effect estimates of renewing versus failing a park tax levy on house prices 1 and 2 years after the vote. Table 2 shows the treatment effect estimates of renewing versus failing a park tax levy on

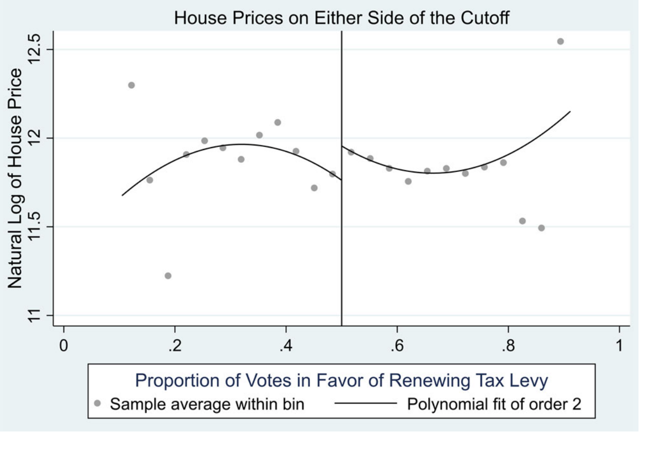


FIGURE 3 House prices on either side of the cutoff.

TABLE 2 Effect on house prices of voting to renew parks and recreation tax levies 1 and 2 years later.

	Bandwidth option				
	RD	TWO	SUM	COMB1	COMB2
Year $t + 1$					
Treatment effect τ	0.01	0.03	0.02	0.02	0.02
Standard error	0.06	0.06	0.06	0.06	0.06
p-value	0.82	0.54	0.33	0.75	0.79
Bandwidth estimate *h*	0.04	0.03, 0.04	0.04	0.04	0.04
Bandwidth bias	0.10	0.09	0.08	0.08	0.09
Year $t + 2$					
Treatment effect τ	0.09	0.12	0.08	0.08	0.09
Standard error	0.05	0.05	0.05	0.05	0.05
p-value	0.12	0.02	0.13	0.13	0.11
Bandwidth estimate *h*	0.04	0.03, 0.06	0.04	0.04	0.04
Bandwidth bias	0.09	0.08	0.08	0.08	0.08

Note: Outcome is natural log of house price in year $t + 1$ and $t + 2$ in communities that vote on a parks and recreation renewal tax levy in period t, so that a 0.12 treatment effect estimate represents a 12% rise in house prices, for example. All estimates obtained using a triangular kernel with standard errors clustered at the community level. MSE-optimal bandwidths estimated using a robust bias-corrected bandwidth selection procedure with the following options: RD—a common bandwidth on both sides of the cutoff, TWO—allows two distinct bandwidths on either side of the cutoff, SUM—selects the bandwidth for the sum of the RD and TWO estimates, COMB1—selects the minimum of RD and SUM, and COMB2—selects the median of RD, TWO, and SUM for each side of the cutoff separately. Two bandwidth estimates are shown for the TWO option to show the separate estimates on the left and right of the 0.50 cutoff. Estimates use squared polynomial for the point estimates and cubic for the bias correction term. The covariates from Table 1 are included in all regressions. Number of observations = 188,407 housing sales in period $t + 1$ and 186,456 in $t + 2$.

house prices 1 and 2 years after the vote. The traditional "RD" bandwidth estimate is 0.04 for both years, meaning that house sales in cities with vote share between 0.46 and 0.54 are included in the regressions. This bandwidth estimate ranges from 0.05 to 0.08 in other years, with a mean of 0.056 across all years studied. No estimate is statistically significant, although the *p*-values decrease from about 0.70 to about 0.12 between year $t + 1$ and year $t + 2$.

The only exception is for year $t + 2$ when bandwidths of different magnitudes are allowed on either side of the cutoff. This specification suggests a treatment effect. If it were to be believed instead of considered random chance, it would suggest a 12% premium for houses in communities that successfully renew their park tax levies, compared to communities that barely vote against the tax levy.

A clearer story emerges 3 years after the vote. Table 3 shows a statistically significant treatment effect for all bandwidth options. The estimates are in a narrow band from 0.09 to 0.12, suggesting that barely voting against a park tax levy causes an approximately 11% drop in house prices, relative to communities that barely renew the tax. The word "causes" is a strong word, but perhaps appropriate. Regression discontinuity is a causal inference technique, and all the assumptions for its use are met, so the treatment effect should be fully identified.

The next rows of Table 3 show treatment effect estimates 4 years after the vote. Again, a narrow range of estimates is found that is statistically significant across bandwidth selection methods. The results suggest that barely renewing a park tax levy causes a 13% rise in house prices compared to communities that barely vote to cut funding.

TABLE 3 Effect on house prices of voting to renew parks and recreation tax levies 3–5 years later.

	Bandwidth option				
	RD	TWO	SUM	COMB1	COMB2
Year $t + 3$					
Treatment effect τ	0.11	0.12	0.09	0.11	0.12
Standard error	0.05	0.05	0.04	0.05	0.05
p-value	0.02	0.01	0.04	0.02	0.01
Bandwidth estimate *h*	0.05	0.04, 0.07	0.07	0.05	0.05
Bandwidth bias	0.10	0.11	0.14	0.10	0.11
Year $t + 4$					
Treatment effect τ	0.13	0.13	0.14	0.14	0.13
Standard error	0.06	0.06	0.06	0.06	0.06
p-value	0.03	0.03	0.02	0.02	0.03
Bandwidth estimate *h*	0.07	0.07, 0.03	0.06	0.06	0.07
Bandwidth bias	0.12	0.11	0.10	0.10	0.11
Year $t + 5$					
Treatment effect τ	0.15	0.17	0.17	0.15	0.17
Standard error	0.05	0.05	0.05	0.05	0.05
p-value	0.01	0.01	0.01	0.01	0.01
Bandwidth estimate *h*	0.05	0.05, 0.02	0.06	0.05	0.05
Bandwidth bias	0.08	0.09	0.09	0.08	0.09

Note: Outcome is natural log of house price in year $t + 3$, $t + 4$, and $t + 5$ in communities that vote on a parks and recreation renewal tax levy in period t, so that a 0.11 treatment effect estimate represents an 11% increase in house prices, for example. Other notes are the same as those for Table 2, except that the number of observations = 174,843 housing sales in $t + 3$, 173,670 in $t + 4$, and 169,096 in $t + 5$.

The pattern of significance extends to 5 years after the vote. Estimates range from 0.15 to 0.17, suggesting that voting to renew park tax levies causes houses to sell for about 16% more than communities that vote to cut park funding for maintenance and repairs.

The magnitude of the effects may be put in context by comparing them with other estimates from the literature. Lutzenhiser and Netusil (2001) find being within 1500 feet of a natural park is worth 16% of a house's sale price. Livy and Klaiber (2016) find a 3.8% increase in house prices after playground replacement. Cheshire and Sheppard (1995) find a 14.7% premium to proximity to intensively farmed and wooded land in Darlington, England, and Black (2018) find a 20% increase in value for homes adjacent to newly preserved open space.

6.2 | Extensions

The first extension we try is to account for communities that fail a tax, but manage to pass a new parks and recreation tax within the 5-year timeframe after the tax failure that we examine in the tables. We identify only seven such failed tax levies, representing 274 house sales within the effective bandwidth. When these 274 houses are removed, perhaps not surprisingly, the results are essentially unchanged.

We next assess whether there is a difference in treatment effect estimates for large and small tax levies. Splitting the sample into greater and less than the median millage saps the statistical power needed to reject the null hypothesis of no treatment effect in either separate regression. We then run the regression using the full sample but adding an interaction term between tax levy millage and a dummy variable for whether the levy failed to renew. This formulation is done with caution, because the programming of Calonico et al. (2019) cannot be used with interaction terms, but, like Lang (2018), we use observations within the effective bandwidth provided by that technique as an approximation to the proper bandwidth. The interaction term is statistically significant and suggests a stronger effect for larger tax levies. This dose–response result suggests that the mechanism of effect is actually decreased park maintenance rather than a signaling mechanism.

We also look for heterogeneous treatment effects using the traditional RD bandwidth option and the years after the vote that Table 3 shows have a significant treatment effect. We look for different effects by population, income, prevalence of children, unemployment rate, age distribution of the community, labor force participation rate, house size, house age, number of stories in the house, number of bathrooms in the house, and importance of the tax levy relative to current park spending levels. The only difference we find is by median sale price of the house. Using the same interaction term strategy as for tax levy size, we find houses with above-median sale price enjoy a bigger percentage gain in value when taxes and park spending are renewed. The gap between more expensive and cheaper houses increases with time, as illustrated in Table 4.

7 | FALSIFICATION TESTS

As a falsification test, we test for pre-trends by seeing if voting on a tax levy in period t causes a change in house prices in periods $t-1$ and $t-2$. If a vote on parks and recreation tax levies in 1998 causes a change in house prices in 1997 or 1996, it casts doubt upon the treatment effect estimates achieved in Tables 2 and 3.

Table 5 shows no statistical significance, with p-values that range from 0.41 to 0.75. This test suggests that house prices were evolving similarly between pass-levy communities and fail-levy communities before the votes.

As a further falsification test, we change the cutoff for treatment from the correct value of 0.50 to false cutoffs of 0.45 and 0.55. If significant treatment effects are observed for false cutoffs, choppiness in the data is driving the results and might be driving the results at the correct cutoff, too. No estimate is statistically significant, suggesting that random patterns in the data are not responsible for the significant treatment effects observed in a narrow range around the 0.50 cutoff.

TABLE 4 Effect on more expensive and cheaper houses of voting to renew parks and recreation tax levies 3–5 years later.

Sample	Year after vote $t+3$	$t+4$	$t+5$
Baseline (from Table 3)	11%	13%	16%
Less expensive houses	10%	10%	12%
More expensive houses	12%	16%	20%

Note: Numbers show premium for renewing versus failing a parks and recreation tax levy is 10% for less expensive houses and 12% for more expensive houses 3 years after the vote, for example. Percentages come from least squares regressions where outcome is natural log of house price in year $t+3$, $t+4$, and $t+5$ in communities that vote on a parks and recreation renewal tax levy in period t. The covariates from Table 1 are included in the regressions, which use the optimal estimated RD bandwidths from Table 3. Interaction term between treatment effect dummy and dummy for whether house sells for above-median price included in regressions, and estimates translate to the effects in the table. p-values for all interactions terms <0.01. Standard errors clustered at the community level. Number of observations same as Table 3.

TABLE 5 Effect on previous years' house prices of voting to renew parks and recreation tax levies.

	Bandwidth option				
	RD	TWO	SUM	COMB1	COMB2
Year $t-1$					
Treatment effect τ	−0.04	−0.07	−0.05	−0.04	−0.05
Standard error	0.08	0.08	0.08	0.08	0.08
p-value	0.59	0.41	0.51	0.59	0.53
Bandwidth estimate *h*	0.06	0.05, 0.06	0.06	0.06	0.06
Bandwidth bias	0.09	0.12	0.11	0.09	0.11
Year $t-2$					
Treatment effect τ	−0.06	−0.05	−0.03	−0.03	−0.05
Standard error	0.07	0.08	0.08	0.08	0.08
p-value	0.45	0.55	0.75	0.75	0.54
Bandwidth estimate *h*	0.08	0.06, 0.05	0.06	0.06	0.070
Bandwidth bias	0.17	0.11	0.13	0.13	0.13

Note: Outcome is natural log of house price in years $t-1$ and $t-2$ in communities that vote on a parks and recreation renewal tax levy in period *t*, so that a −0.04 treatment effect estimate represents a 4% decrease in house prices, for example. Other notes are the same as those for Table 2, except that the number of observations = 169,335 housing sales in $t-1$ and 167,537 in $t-2$.

The theory of regression discontinuity suggests that if the covariates we observe are balanced, the ones that are unobserved are balanced, too (Dunning, 2012; Murnane & Willett, 2010). This characteristic stems from the randomization of observations around the cutoff. It clearly becomes less plausible the farther is an observation from the cutoff, but if observations sufficiently close to the cutoff are as good as randomly assigned, any characteristic one can measure should be similar between communities that barely pass and barely fail their votes. If covariate imbalance were driving statistically significant treatment effects in periods $t+3$ and beyond, it should also be driving significant estimates in periods $t-2$ through $t+2$, but it is not.

For example, poverty is an omitted covariate. If poverty is imbalanced between pass and fail levy samples, and poverty is correlated with house prices, an imbalance in poverty could be the true driver of the estimated treatment effect. The theory of regression discontinuity says there should be balance near the cutoff, and in fact the mean poverty rate is 9% for the fail levy sample and 8% for the pass levy sample. Educational attainment is also omitted, but it too is balanced near the cutoff. The percent of persons with graduate degrees, the percent with bachelor's degrees, and the percent with some college but less than a bachelor's degree are all within one percentage point of each other between pass and fail levy samples near the cutoff. A similar balance is found for single-parent households, % renter-occupied dwellings, % racial minority, % Hispanic, and variables measuring marital status. Unobserved influences seem to be balanced as theory suggests.

If an imbalance of any factor between pass-levy and fail-levy samples is responsible for the significant treatment effect estimates in periods $t+3$ through $t+5$, this same imbalance should be causing significant estimates in periods $t+2$, $t+1$, t-1, and t-2. The lack of statistical significance in periods $t-1$ and $t-2$ in particular implies there is no such imbalance. The evidence suggests that the reduced maintenance and repair of parks and recreational facilities is responsible for the change in house prices.

8 | CONCLUSION

Hundreds of studies have examined the effect of open spaces on house prices, including golf courses, lakes, parks, and nature preserves, testifying to the importance of the topic. We study communities that cut current expenditures on parks and recreational areas. Few of these studies use causal inference techniques, and only one uses regression discontinuity. Cutting park maintenance and repair seems not to be perceived by the housing market in the first year or two after the vote, arguing against an announcement effect (Kohlhase, 1991). But fewer worker hours and reduced replacement of equipment and reduced supplies like mulch, leaf blowers and fertilizer and the like seem to cause a drop in house prices of 11% in year 3, 13% in year 4, and 16% in year 5 after the vote. We cannot know for sure, but this pattern of results is consistent with parks and recreational areas starting out in good shape and becoming cumulatively worse over time. We also find the effect on house prices is more pronounced for expensive homes. The gap in treatment between houses that sell for above-median and below-median prices house prices is 2% the third year after the vote, 6% in the fourth year, and 8% in the fifth year.

There are a couple of ways to quantify the effects of voting to cut park funding. The first focuses on park spending's impact on house prices. The typical park tax levy is 0.85 mills, where 1 mill raises $1 for every $1000 of assessed house value. The mean sale price in the sample is $233,928, and house prices drop by about 13%, so voting against the tax levy causes a 0.13 × $233,928 = $30,410 drop in house price. How much does each house save in taxes? Ohio assesses 35% of appraised value, so 0.35 × $233,928 = $81,875 of taxable value for the average house. A 0.85 mill levied on $81,875 of taxable value yields $70 in park taxes per house per year. Discounting at 5% means that communities that vote to cut park funding save $70/0.05 = $1,400 in lifetime taxes at a cost of $30,410 in house value.

Another way to quantify the effects is to find the percent change in house price caused by a percent change in property taxes. The percent change in house price is about 13%. The average effective property tax rate in Ohio is 148 mills (smartasset.com, 2021). Cutting 0.85 mills represents a 0.58 percent drop in the property tax rate. This means that a less than 1% change in tax rates causes a 13% change in house prices. Or, borrowing the figure from Section 3, a 16% cut in parks and recreation spending causes a 13% drop in house prices. Clearly, in Ohio, park spending has a large impact on house prices. Preserving park maintenance spending in Ohio provides a large return in the form of higher house prices.

ACKNOWLEDGMENTS

David M. Brasington is grateful to Olivier Parent for making the map, to CoreLogic for supplying the housing data set, to Jeff A. DiBartolomeo for discussant comments at the ARES meetings, and for funding from the Lindner College of Business, the University of Cincinnati Third Century Grant, and the Charles P. Taft Research Center Faculty Release Fellowship. Brasington received funding from the Lindner College of Business, the University of Cincinnati University Research Council Third Century Grant, and the Charles P. Taft Research Center.

CONFLICT OF INTEREST STATEMENT

The author declares no conflict of interest.

DATA AVAILABILITY STATEMENT

I would be glad to share the voting and Census data upon request, but my contract with CoreLogic prohibits any form of sharing the housing transaction data.

ORCID

David M. Brasington http://orcid.org/0000-0001-5406-8543

REFERENCES

Abbott, J. K., & Allen Klaiber, H. (2013). The value of water as an urban club good: A matching approach to community-provided lakes. *Journal of Environmental Economics and Management*, *65*(2), 208–224.

Anderson, S. T., & West, S. E. (2006). Open space, residential property values, and spatial context. *Regional Science and Urban Economics*, *36*(6), 773–789.

Berk, R., Barnes, G., Ahlman, L., & Kurtz, E. (2010). When second best is good enough: A comparison between a true experiment and a regression discontinuity quasi-experiment. *Journal of Experimental Criminology*, *6*(2), 191–208.

Black, K. J. (2018). Wide open spaces: Estimating the willingness to pay for adjacent preserved open space. *Regional Science and Urban Economics*, *71*, 110–121.

Brasington, D. M. (2017a). What types of people sort to which public services? *Papers in Regional Science*, *96*(3), 537–553.

Brasington, D. M. (2017b). School spending and new construction. *Regional Science and Urban Economics*, *63*, 76–84.

Buddelmeyer, H., & Skoufias, E. (2004). *An evaluation of the performance of regression discontinuity design on PROGRESA*. The World Bank.

Calonico, S., Cattaneo, M. D., Farrell, M. H., & Titiunik, R. (2017). rdrobust: Software for regression-discontinuity designs. *The Stata Journal: Promoting Communications on Statistics and Stata*, *17*(2), 372–404.

Calonico, S., Cattaneo, M. D., Farrell, M. H., & Titiunik, R. (2019). Regression discontinuity designs using covariates. *The Review of Economics and Statistics*, *101*(3), 442–451.

Carlino, G. A., & Saiz, A. (2019). Beautiful city: Leisure amenities and urban growth. *Journal of Regional Science*, *59*(3), 369–408.

Cattaneo, M. D., Idrobo, N., & Titiunik, R. (2019). *A practical introduction to regression discontinuity designs: Foundations*. Cambridge University Press.

Cattaneo, M. D., Jansson, M., & Ma, X. (2018). Manipulation testing based on density discontinuity. *The Stata Journal: Promoting Communications on Statistics and Stata*, *18*(1), 234–261.

Cheshire, P., & Sheppard, S. (1995). On the price of land and the value of amenities. *Economica*, *62*(246), 247–267.

Clark, D. E., & Cosgrove, J. C. (1990). Hedonic prices, identification, and the demand for public safety. *Journal of Regional Science*, *30*(1), 105–121.

Cohen, J. P., & Coughlin, C. C. (2008). Spatial hedonic models of airport noise, proximity, and housing prices. *Journal of Regional Science*, *48*(5), 859–878.

Dunning, T. (2012). *Natural experiments in the social sciences: A design-based approach*. Cambridge University Press.

Fernandez, L., Cutter, B., Sharma, R., & Scott, T. (2018). Land preservation policy effect or neighborhood dynamics: A repeat sales hedonic matching approach. *Journal of Environmental Economics and Management*, *88*, 311–326.

Franco, S. F., & MacDonald, J. L. (2018). Measurement and valuation of urban greenness: Remote sensing and hedonic applications to Lisbon, Portugal. *Regional Science and Urban Economics*, *72*, 156–180.

Fuller, R. A., Irvine, K. N., Devine-Wright, P., Warren, P. H., & Gaston, K. J. (2007). Psychological benefits of greenspace increase with biodiversity. *Biology Letters*, *3*(4), 390–394.

Gelman, A., & Imbens, G. (2019). Why high-order polynomials should not be used in regression discontinuity designs. *Journal of Business & Economic Statistics*, *37*(3), 447–456.

Ho, C. S., & Hite, D. (2008). The benefit of environmental improvement in the southeastern United States: Evidence from a simultaneous model of cancer mortality, toxic chemical releases and house values. *Papers in Regional Science*, *87*(4), 589–604.

Hong, K., & Zimmer, R. (2016). Does investing in school capital infrastructure improve student achievement? *Economics of Education Review*, *53*, 143–158.

Irwin, E. G., & Bockstael, N. E. (2001). The problem of identifying land use spillovers: Measuring the effects of open space on residential property values. *American Journal of Agricultural Economics*, *83*(3), 698–704.

Jauregui, A., Tidwell, A., & Hite, D. (2017). Sample selection approaches to estimating house price cash differentials. *The Journal of Real Estate Finance and Economics*, *54*(1), 117–137.

Kohlhase, J. E. (1991). The impact of toxic waste sites on housing values. *Journal of Urban Economics*, *30*(1), 1–26.

Lang, C. (2018). Assessing the efficiency of local open space provision. *Journal of Public Economics*, *158*, 12–24.

Lee, D. S., & Lemieux, T. (2010). Regression discontinuity designs in economics. *Journal of Economic Literature*, *48*(2), 281–355.

Livy, M. R., & Klaiber, H. A. (2016). Maintaining public goods: The capitalized value of local park renovations. *Land Economics*, *92*(1), 96–116.

Lutzenhiser, M., & Netusil, N. R. (2001). The effect of open spaces on a home's sale price. *Contemporary Economic Policy*, *19*(3), 291–298.

McMillen, D. P., & Redfearn, C. L. (2010). Estimation and hypothesis testing for nonparametric hedonic house price functions. *Journal of Regional Science*, *50*(3), 712–733.

Murnane, R. J., & Willett, J. B. (2010). *Methods matter: Improving causal inference in educational and social science research*. Oxford University Press.

Ohler, A., & Blanco, G. (2017). Valuing public goods, the time to capitalization, and network externalities: A spatial hedonic regression analysis. *Land Economics*, *93*(1), 127–144.

Parent, O., & vom Hofe, R. (2013). Understanding the impact of trails on residential property values in the presence of spatial dependence. *The Annals of Regional Science*, *51*(2), 355–375.

Pei, Z., Lee, D. S., Card, D., & Weber, A. (2021). Local polynomial order in regression discontinuity designs. *Journal of Business & Economic Statistics*, *40*(3), 1259–1267.

Schroeder, T. D. (1982). The relationship of local public park and recreation services to residential property values. *Journal of Leisure Research*, *14*(3), 223–234.

Shadish, W. R., Clark, M. H., & Steiner, P. M. (2008). Can nonrandomized experiments yield accurate answers? A randomized experiment comparing random and nonrandom assignments. *Journal of the American Statistical Association*, *103*(484), 1334–1344.

Shadish, W. R., Galindo, R., Wong, V. C., Steiner, P. M., & Cook, T. D. (2011). A randomized experiment comparing random and cutoff-based assignment. *Psychological Methods*, *16*(2), 179–191.

Smartasset.com. (2021). *Ohio property taxes*. https://smartasset.com/taxes/ohio-property-tax-calculator#LgiVkeu9C2

Song, Y., & Knaap, G. J. (2004). Measuring the effects of mixed land uses on housing values. *Regional Science and Urban Economics*, *34*(6), 663–680.

Stadelmann, D. (2010). Which factors capitalize into house prices? A Bayesian averaging approach. *Journal of Housing Economics*, *19*(3), 180–204.

Thistlethwaite, D. L., & Campbell, D. T. (1960). Regression-discontinuity analysis: An alternative to the ex post facto experiment. *Journal of Educational Psychology*, *51*(6), 309–317.

Tiebout, C. M. (1956). A pure theory of local expenditures. *Journal of Political Economy*, *64*(5), 416–424.

US Census Bureau. (2018). *2018 state & local government finance historical datasets and tables*. https://www.census.gov/programs-surveys/gov-finances.html

vom Hofe, R., Mihaescu, O., & Boorn, M. L. (2018). Are homeowners willing to pay more for access to parks? Evidence from a spatial hedonic study of the Cincinnati, Ohio, USA park system. *Journal of Regional Analysis & Policy*, *48*(3), 3851.

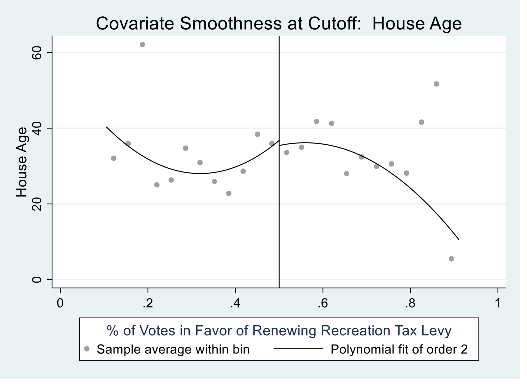
vom Hofe, R., Parent, O., & Grabill, M. (2019). What to do with vacant and abandoned residential structures? The effects of teardowns and rehabilitations on nearby properties: vom Hofe et al. *Journal of Regional Science*, *59*(2), 228–249.

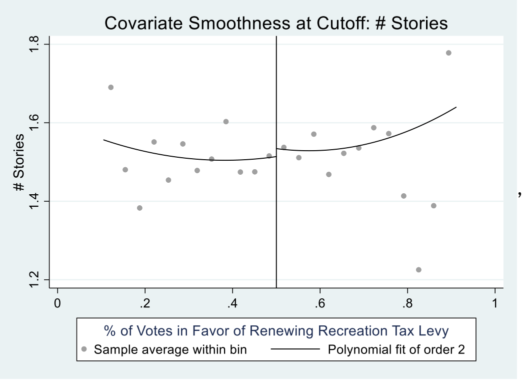
Wu, W., & Dong, G. (2014). Valuing the "green" amenities in a spatial context. *Journal of Regional Science*, *54*(4), 569–585.

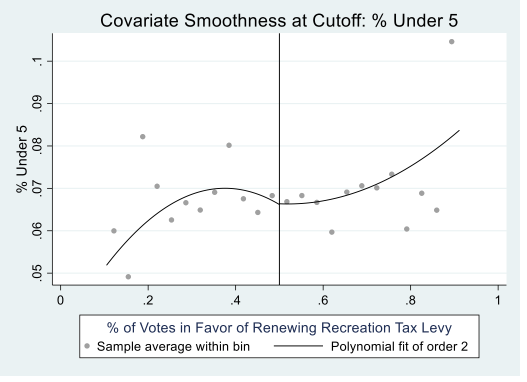
Yinger, J. (2015). Hedonic markets and sorting equilibria: bid-function envelopes for public services and neighborhood amenities. *Journal of Urban Economics*, *86*, 9–25.

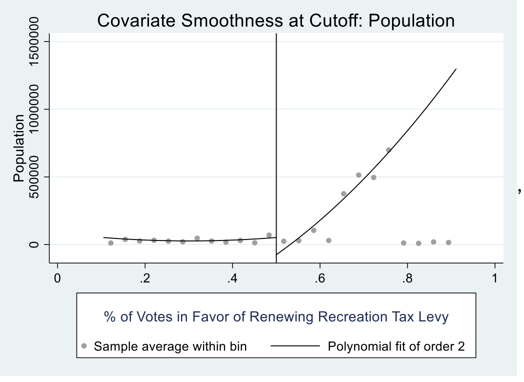
How to cite this article: Brasington, D. M. (2023). The effect of open space maintenance spending on house prices. *Journal of Regional Science*, *63*, 1053–1072. https://doi.org/10.1111/jors.12655

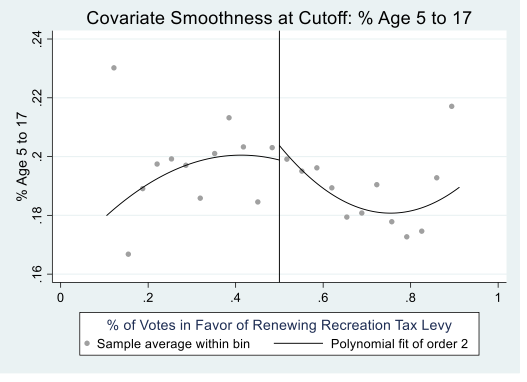
APPENDIX

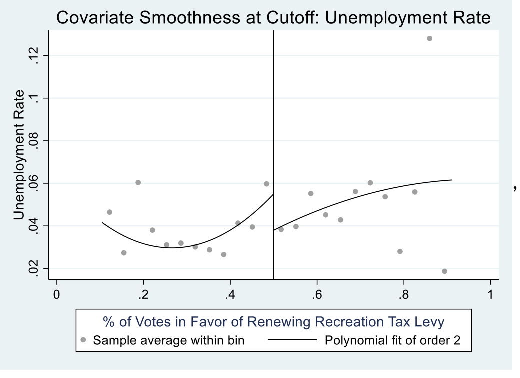

Covariate Smoothness at Cutoff: House Age
House Age
0
20
40
60
0
.2
.4
.6
.8
1
% of Votes in Favor of Renewing Recreation Tax Levy
Sample average within bin
Polynomial fit of order 2

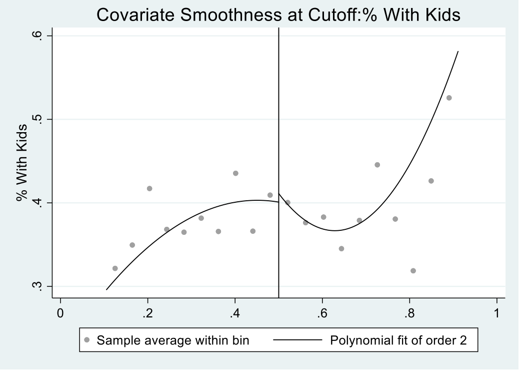

Covariate Smoothness at Cutoff: # Stories
Stories
1.2
1.4
1.6
1.8
0
.2
.4
.6
.8
1
% of Votes in Favor of Renewing Recreation Tax Levy
Sample average within bin
Polynomial fit of order 2

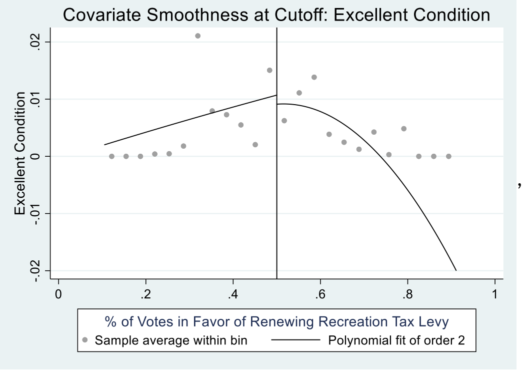

Covariate Smoothness at Cutoff: % Under 5
% Under 5
.05
.06
.07
.08
.09
.1
0
.2
.4
.6
.8
1
% of Votes in Favor of Renewing Recreation Tax Levy
Sample average within bin
Polynomial fit of order 2

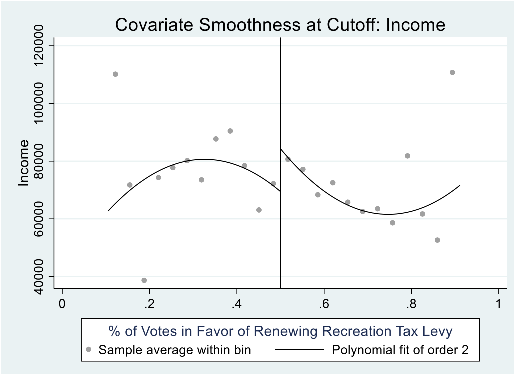

Covariate Smoothness at Cutoff: Population
Population
0
500000
1000000
1500000
0
.2
.4
.6
.8
1
% of Votes in Favor of Renewing Recreation Tax Levy
Sample average within bin
Polynomial fit of order 2

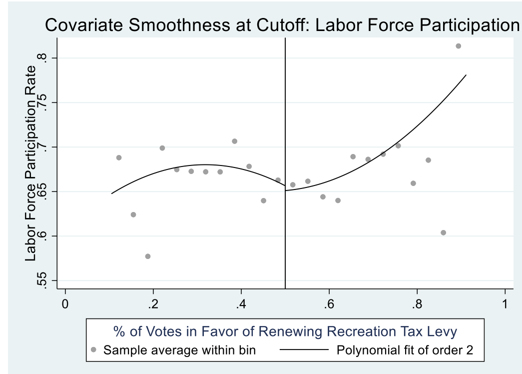

Covariate Smoothness at Cutoff: % Age 5 to 17
% Age 5 to 17
.16
.18
.2
.22
.24
0
.2
.4
.6
.8
1
% of Votes in Favor of Renewing Recreation Tax Levy
Sample average within bin
Polynomial fit of order 2

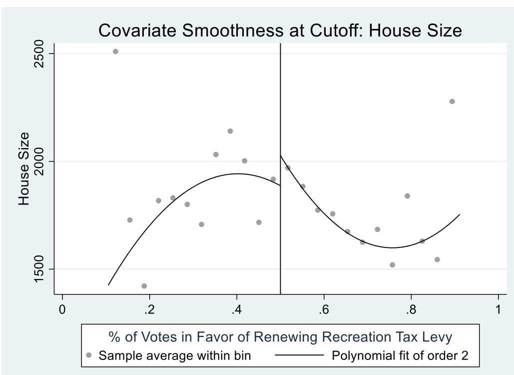

Covariate Smoothness at Cutoff: Unemployment Rate
Unemployment Rate
.02
.04
.06
.08
.1
.12
0
.2
.4
.6
.8
1
% of Votes in Favor of Renewing Recreation Tax Levy
Sample average within bin
Polynomial fit of order 2


Covariate Smoothness at Cutoff:% With Kids
% With Kids
.3
.4
.5
.6
0
.2
.4
.6
.8
1
Sample average within bin
Polynomial fit of order 2


Covariate Smoothness at Cutoff: Excellent Condition
Excellent Condition
-.02
-.01
0
.01
.02
0
.2
.4
.6
.8
1
% of Votes in Favor of Renewing Recreation Tax Levy
Sample average within bin
Polynomial fit of order 2


Covariate Smoothness at Cutoff: Income
Income
40000
60000
80000
100000
120000
0
.2
.4
.6
.8
1
% of Votes in Favor of Renewing Recreation Tax Levy
Sample average within bin
Polynomial fit of order 2


Covariate Smoothness at Cutoff: Labor Force Participation
Labor Force Participation Rate
.55
.6
.65
.7
.75
.8
0
.2
.4
.6
.8
1
% of Votes in Favor of Renewing Recreation Tax Levy
Sample average within bin
Polynomial fit of order 2


Covariate Smoothness at Cutoff: House Size
House Size
1500
2000
2500
0
.2
.4
.6
.8
1
% of Votes in Favor of Renewing Recreation Tax Levy
Sample average within bin
Polynomial fit of order 2