The Effect of Local Road Tax Cuts on House Values [*]

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Abstract

We investigate the economic impact of cutting taxes that fund maintenance of local roads. Using Ohio Secretary of State data, we design a quasi-experiment to study city-level referendums introduced to renew local road taxes, which are typically levied via property taxes. We compare housing sale prices between similar cities that narrowly pass or fail road tax levies and use satellite images to fine-tune an Artificial Intelligence (AI) model for classifying road quality. For cities that cut their renewal tax levies, we check the loss in road maintenance funds. Our results show that cities which decide to cut local road taxes have lower funds for road maintenance, poorer road quality and suffer an average decrease in housing prices of $15,350 (9%) over 10 years relative to cities that renewed funding. Heterogeneity analysis reveals differential decline in urban and rural areas and for expensive homes relative to their cheaper counterparts.

Keywords: Road Maintenance, Local Taxation, Housing values, Artificial Intelligence

JEL Codes: R42, H71, R10, C21

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1 Introduction

Roads are an important form of infrastructure investment that affect firms' production functions and people's commuting costs. Many studies examine the importance of roads and how they affect residents' daily lives (Currier, Glaeser and Kreindler 2023; Adukia, Asher and Novosad 2020; Asher and Novosad 2020). Some papers emphasize that building new roads increases employment and entry of new firms (Gibbons et al., 2019), thereby increasing economic activity. Other papers point out the potential for worse outcomes in the form of increased inequality between the rich and the poor (Hettige, 2006) and an exodus of workers seeking access to larger labor markets (Asher and Novosad, 2020).

In this paper, we highlight the importance of maintaining road spending for cities in Ohio. We collect data on more than 3,000 renewal road-tax levy elections conducted within different county subdivisions, including townships, cities and villages, in Ohio (referred to as "cities" for brevity). Using dynamic regression discontinuity design, we examine how these exogeneous cuts in maintenance spending influence housing prices, focusing on cities where voting results were narrowly decided. Our results reveal that when a city cuts its renewal road taxes, it experiences a loss in maintenance funds, its road quality declines by 17% and house prices decreases by around $15,350, with the effect starting four years after the vote and persisting through many years after the vote. This discount in house prices represents about 9 percent of house value over the decade. The delayed effect is consistent with the time it takes for roads to deteriorate noticeably. We find that these price effects are driven by urban areas rather than rural areas. We also observe that higher-priced houses experience larger percentage reductions in value than lower-priced houses, indicating a heterogeneous impact across different quantiles. This suggests that higher-income households are more sensitive to road quality than lower-income households.

Related Literature. Perhaps the study most related to ours is Asher and Novosad (2020), which studies the impact of new roads on villages in India. Like our study, its identification strategy is regression discontinuity, although ours is sharp rather than the fuzzy

form. It argues the main obstacle to identification in prior studies is that the placement of new roads is usually correlated with economic (or political) characteristics rather than exogenous. Its findings suggest this is a serious problem with the literature because, unlike prior studies, it finds no strong link between economic growth and new road placement, suggesting that the estimates of previous studies that find a link are driven by road placement in villages that are already growing. Asher and Novosad (2020) touts its use of village-level rather than regional-level data. We, too, look at economic outcomes at the level of village, city, and township, the most local levels of government. A surprising finding of Asher and Novosad (2020) is that investment in transportation infrastructure does not affect village incomes, assets, or agricultural output. Its measure of assets is a village-level average of a series of binary indicators of ownership of a variety of assets, along with separate regressions for the presence of a 'solid house', refrigerator, and phone; whereas we study the effect of transportation spending on wages, employment, and housing sale prices. Of course, our use of a developed geography contrasts with rural villages in India. Our efforts to achieve identification focus on the maintenance of existing roads, which avoids the endogeneity of the placement of new roads.

Another study that is related to ours is Cellini, Ferreira and Rothstein (2010), which studies the effect of new capital projects for schools, funded via new local bond issues and raised by referendums, on house prices to deduce the value of school facility investments. Both our study and Cellini, Ferreira and Rothstein (2010) employ a dynamic regression discontinuity design and analyze changes in regional property values. Moreover, both papers rely on broadly similar identification strategies and assignment mechanisms: in each case, votes in favor of or against a local referendum serves as the source of exogenous variation. Despite these similarities, a few key distinctions stand out. First, Cellini, Ferreira and Rothstein (2010) draws on all observations in their sample, while we limit our analysis to the optimal bandwidth around the 50% vote-share cutoff, even after using flexible controls. Second, Cellini, Ferreira and Rothstein (2010) implements a fuzzy regression discontinuity

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approach, whereas ours is sharp, focusing exclusively on narrowly passing versus narrowly failing road-tax renewal votes. Lastly, we focus on the Intent-to-Treat (ITT) estimator, which focuses on the effect of election on outcome without controlling for results of subsequent elections and it suits our setting given the independence of renewal referendums coming from exogeneous timing of such elections (more details in Section 3). As Cellini, Ferreira and Rothstein (2010) show, when elections are indeed independent, the ITT estimator is equal to the Treatment on the Treated (TOT) estimator.

We highlight a substantial literature studying the effect of transportation infrastructure on house prices. Hoogendoorn et al. (2019) studies the effect of opening of a tunnel on house prices in the Netherlands, noting that prior research studying transportation infrastructure in developed geographies suffers from reverse causality. It argues that the opening of the Westerscheldtunnel is a fairly exogenous event, with natural borders that prevent contamination of results by the surrounding environment. It finds half the capitalized value of the tunnel occurs more than a year before the tunnel opens. Our data, too, is for a developed nation. Hoogendoorn et al. (2019) argues that the exogeneity of the opening of the tunnel, along with hedonic controls, time trends and postcode fixed effects, identifies its estimates. One novelty of our study is how ordinary the events are that we study. While the opening of a new tunnel is important, it is rare. Votes to renew infrastructure spending are common events in many local governments in the United States, and the quantity of road mainte-nance spending is regularly chosen by governments around the world, if not by voting then directly by bureaucrats. It is therefore also important to study the effects of road main-tenance spending on house prices. Li et al. (2016) studies the overall effect on apartment prices of new subway lines in Beijing, but the estimates may be a net effect of competing factors. Gibbons and Machin (2005), studying the construction of new rail stations for the London underground and light rail services, notes that the effect on house prices captures the net effect of better access, increased crime, and increased noise pollution. Levkovich, Rouwendal and Van Marwijk (2016) looks at the effect of highway development on house

prices in the Netherlands. It separates out accessibility effects from noise pollution and increased traffic effects by looking at different neighborhoods near the highway development. Its repeat sales difference in differences model finds increased house prices from anticipation effects (Kohlhase, 1991). Beenstock, Feldman and Felsenstein (2016) also finds anticipation effects for house prices (but not new construction) for the development of a highway across Israel.

Contribution. Our paper contributes to the literature on three main fronts. First, we establish a new dataset that allows us to study how changes in local taxes affect local infrastructure maintenance and neighborhood home prices. The votes on road maintenance taxes are matched with auditor data on road maintenance spending by Ohio cities, and property sale prices from CoreLogic. Our data also includes images of roads from Google Street Views API before and after cities vote to cut road maintenance. We train an artificial intelligence model to measure road attractiveness to see if cutting road taxes causes a decline in the physical appearance of roads.

Second, we focus on long-term effects of reduced road taxes funded mainly via property taxes, and observe statistically significant decline in house prices occurring after four years and persisting in later years. This delayed effect highlights how the consequences of road tax cuts are not immediate but gradually accumulate as funding is reduced and roads deteriorate. Our research also identifies heterogeneous impacts, showing that urban areas and higher-priced homes suffer more pronounced price declines. By focusing on distributional effects, the study adds nuance to the understanding of how infrastructure maintenance affects local housing markets.

Third, while much of the literature focuses on new infrastructure development and expansion in developing nations, we study infrastructure maintenance in a developed economy. Focusing on existing roads mitigates an important source of identification problems. Our approach provides insights that are more relevant for policymakers in advanced economies, where road infrastructure is well established but requires continuous upkeep.

Roadmap. The rest of the paper is organized as follows. Section 2 provides background information on the data and provides information about the variables used in the study. Section 3 outlines the empirical strategy. Section 4 presents the results of the study and shares the relevant robustness checks. Section 5 suggests some mechanisms and Section 6 concludes.

2 Background & Data

2.1 How are roads funded in Ohio?

Roads in Ohio are funded through a combination of federal, state, and local sources. A significant portion of road funding provided by federal and state governments comes from gas taxes, which are currently set at $0.18 per gallon for federal tax and $0.38 per gallon for Ohio state tax. Additional sources for these 2 levels of government include vehicle registration fees, license plate fees, tolls, and driver's license fees. Funding for local governments largely comes from property taxes. Below, we provide an overview of road funding in Ohio from national, state, and local sources.

Federal Funding. U.S Department of Transportation (USDOT) provides funding for road infrastructure through the Federal Highway Administration (FHWA). The FHWA provides funding for the construction, maintenance, and operation of highways, bridges, and tunnels. The federal government provides funding for road infrastructure through the Highway Trust Fund (HTF), which is funded by the federal gas tax. The HTF is divided into two accounts: the Highway Account and the Mass Transit Account. The Highway Account is used to fund highway construction and maintenance, while the Mass Transit Account is used to fund public transportation projects. The federal government also provides funding for road infrastructure through the Surface Transportation Block Grant Program (STBG) or Bipartisan Infrastructure Law (U.S. Department of Transportation, 2022), which provides funding for road projects that are not eligible for funding through the HTF. Nevertheless,

federal government funding for road infrastructure is generally limited, as in 2022, about $4/5^{\text{th}}$ of the funding came from state and local governments (Peter G. Peterson Foundation, 2024).

State Funding. In Ohio, gas taxes, licensing fees and user fees account for 69% of the state's road funding (Boesen, 2021). The Ohio Department of Transportation (ODOT) is responsible for the allocation of state funds, which are used for both maintenance and new construction. However, about 70% of funds go to new highway construction, 2% is given to local governments as grants and only 4% of state funds is directed towards local road maintenance (Ohio Department of Transportation, 2023). The remaining funds are part of payroll & operating expenses, and other miscellaneous expenses. Hence, most of the road maintenance funding for local, neighborhood-specific roads in Ohio comes from local governments.

Local Funding. Local governments in Ohio fund roads mainly through a portion of property taxes, even though there is variation in such funding across the local governments. These municipalities have the authority to levy property taxes specifically for road maintenance, providing a crucial source of funding for the upkeep of local infrastructure. For instance, as per our correspondance with the Beavercreek Township, 61% of their road funds come from property taxes, and only 8% from gas taxes. Moreover, 77% of funding for roads in Beavercreek township is provided by the local government and 11% of overall local government budget is allocated to roadway maintenance (Public Service Department of Beavercreek Township, 2025). We can see that these local roads are primarily funded by local governments in Ohio and road maintenance funding is a significant part of the local government's budget.

2.2 Local Taxation in Ohio

Background. Ohio consists of 88 counties, each covering about 464 miles^2 (1,200 kilometers^2). Each county was historically divided into about 15 equally-sized townships, which do not

cross county lines. Citizens can petition to incorporate as a village, which has a different type of government structure than a township and the ability to levy both income and property taxes, whereas townships may only levy property taxes. When a village exceeds 5,000 population in Ohio, it is reclassified as a city. Villages and cities may cross township and county lines, dissolve, or annex parts of contiguous townships. Villages, cities and townships, which we call "cities" for brevity, are the most local governmental unit in Ohio. Each local government covers about 18.2 miles^2 (47.1 km^2) on average. The Ohio Revised Code lets local governments collect a small amount of tax without a vote. Beyond this limited amount local governments put tax levies on the ballot to ask for additional money from voters. Ohio has 3,939 local governments in total as of 2022 (Federal Reserve Bank of St. Louis, 2024).

The type of tax levy we study is for the renewal of road tax levy, which covers a broad range of activities. Most of the renewal taxes we consider have stated purposes of "road and bridges repair", "road repair", "street fund", and "street improvements", although there are less common stated purposes like "repair and maintenance of streets and sewers system" and "resurfacing and rehabilitation of city streets." The construction of new roads and bridges, in contrast, would be funded with a tax levy for additional money, not a renewal tax; and it would likely be funded by a bond levy lasting 20 or 30 years. We eliminate from our dataset stated purposes that might suggest new road construction like a 30-year 1.9-mill new tax in Moscow Village for "permanent improvements" and 0.5-mill new tax for 20 years in Shawnee Hills Village for "general construction and road and bridges repair." Our dataset includes tax levies like a 2-mill, 5-year renewal in Adams Township (Champaign County) in 1995; a 3-mill, 5-year renewal in Lore City Village in 2016; and a 2.5-mill, 5-year renewal in Pataskala City in 2007.

Levies that had originally passed typically expire, and the most common duration to collect a levy is five years, representing 89% of the road tax levies in our sample. If a tax levy is renewed, taxes and funding continue. If 50% or fewer votes approve the levy, it fails. When a tax levy for additional funding fails, there is no increase in funding, but existing funding

from other tax levies continues as normal. When a renewal tax levy fails, funding from that tax levy stops. 99% of the road tax levies in our sample are property taxes and 1% are income taxes.

Renewal levies. Most regression discontinuity studies that use voting data to look at the impact of funding changes examine new tax levies for additional funding. Cellini, Ferreira and Rothstein (2010) observes that votes for additional tax money may not be statistically independent; a vote may be proposed until it passes. We minimize this source of endogeneity of new votes by only considering renewal votes (Brasington, 2017). While a government may choose when to put a vote for additional funding on the ballot and keep proposing the new tax until it is passed, when a vote passes it has an expiration date. So if a road tax levy for additional funding passes in 2007 to last five years, in 2012 voters will have the chance to renew or reject the tax. The timing of the vote in 2012 is not endogenous, having been set in 2007. If voters renew it in 2012, it will be up for renewal again in 2017.

Spending impact of failing to renew a levy. When a renewal tax levy fails, the local government loses the funding from that tax levy. The local government may still have other tax levies in effect, but the road tax levy that failed to renew is no longer collected. The local government may also have current expense tax levies in effect, which are not earmarked for roads but can be used for any purpose. To investigate the impact of failing to renew a road tax levy on road spending, we use related information from the levy database, along with home prices and demographics information. We use this data to determine the dollar amount in consideration when a household is making a decision on whether to vote for or against a renewal road tax levy, and we call it Average cost of levy per household, which is the tax amount that a household will be paying if the levy is successfully renewed. Average cost of levy per household is computed using the following formula -

$$\text{Average Cost of levy per household}_{it} = \text{millage rate}_{it} \times \text{Average Assessed value}_{it} \quad (1)$$

where i is the city, t is the year, and millage rate is the predetermined millage amount set for the road tax levy. The average assessed value is 35% of the average appraisal value[1], which for our study equals the average sale price of homes in the city. From this, we can also compute the average cost of levy per city by multiplying the average cost of levy per household with the number of households in the city.

Although we acknowledge large variation in the appraisal value across neighborhoods and in millage rates across referendums, we find that the average cost of levy per household is $76, and the average cost of levy per city is $167,011 as shown in Table 1. We do not observe any significant difference in the average cost of levy per household between cities that renew and cut road tax levies. This average cost of levy accounts for 15% of a local government's budget for road maintenance.

Table 1: Descriptive Statistics: Cost of Road Tax Levies

	Aggregate	Renewed	Cut
Panel A: Cost of levy per household			
Mean	76	75	79
	(55)	(53)	(62)
Panel B: Cost of levy			
Mean	167,011	167,648	163,547
	(340,268)	(340,628)	(338,671)

Notes: This table presents descriptive statistics for two measures of the cost of road tax levies. **Panel A** reports the mean and standard deviation (SD) of the cost per household per year. **Panel B** reports the mean and SD of the total cost per city per year. "Aggregate" denotes the full sample, while "Renewed" and "Cut" refer to levies that were renewed and failed to renew respectively. All monetary values are in constant 2010 U.S. dollars and rounded to the nearest integer.

2.3 Tax Cuts & Evidence of Road Quality

To understand the effect of cutting local road tax on road quality, we fine-tune a Vision Transformer (ViT) model, gpt-4o by OpenAI, on satellite imagery data from Brewer et al.

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[1]Assessment ratio of 35%, as set by Ohio Department of Taxation (2020)

(2021). Following the seminal work on text-based Transformers in Natural Language Processing (NLP) by Vaswani (2017), ViTs were introduced by Google Brain's team and are part of a recent class of deep learning models that have shown to outperform Convolutional Neural Networks (CNNs) on image classification tasks (Dosovitskiy, 2020). Below, we outline the steps we take to assess road quality using the ViT model.

Satellite Images for fine-tuning. In order to enable ViTs to accurately predict road quality, we needed large-scale road-image data to fine-tune OpenAI's gpt-4o, a Large Language Model (LLM) with multi-modal ability that has shown to outperform various other models and benchmarks (Achiam et al., 2023). We use the road-image dataset by Brewer et al. (2021) which consists of 53,677 labeled images of road infrastructure across the United States. The dataset includes images of roads in different conditions, and a classification representing quality of the road: 0 (poor), 1 (decent) and 2 (high).

Ohio Satellite Images. We use satellite images from Google Earth Pro for roads in different neighborhoods in Ohio. We collect road images for areas that were within the average effective RD bandwidth provided in Table 3 and ensure that we have pre and post referendum images for both, the treatment and control groups. This allows us to construct a novel database of road images, which we use to classify road quality for areas that were part of our quasi-experiment. Figure 1 presents 2 examples of road images from Ohio, one from a road that was classified as high and the other as poor quality.

(A) A **<u>Poor</u>** Quality Road   (B) A **<u>High</u>** Quality Road

Figure 1: Road Quality Satellite images for Ohio

Fine-tuning process. Fine-tuning a ViT involves taking a pretrained model and adapting it for specific image-classification task. We fine-tune gpt-4o, a pretrained model with over 200 billion parameters, on the road-image dataset by Brewer et al. (2021) to classify road images into three categories: 0, 1 and 2. Next, we divide the data into traning and validation datasets. Using OpenAI's API, we set up a training seed, convert the images into their corresponding base64 encoding and fine-tune the model on the road-image dataset for 10 epochs with a batch size of 9 and a learning rate multiplier of 2. In section 4.1, we present the results from our fine-tuned model, when applied on roads in areas with close elections within our effective bandwidth. For full details on the modeling methodology, fine-tuning results and robustness tests, see Suresh and Rawat (2025).

2.4   Running Variable

The running variable plays a critical role in regression discontinuity, which in this study represents the proportion of votes against the renewal of a road tax levy.

A vote share of more than 50 means the renewal road tax levy fails and tax will no longer be collected, resulting in a stoppage of road funding via that particular tax levy. However, other road tax levies may continue to be in effect, and funds from current expense tax levies may still be used for road maintenance. There are 3,188 referendum results in our sample, 83% of which renew the tax, and 17% of which cut taxes and road maintenance. We quantify the size of the cut in road spending a city faces in two ways. The Great Financial Crisis falls in the middle of our dataset, so readers might wonder if voting behavior was affected, but we find vote share the same to two decimal points during and outside the years 2008-2009.

Our key identification assumption is that the election results are not predetermined and vote share is not precisely manipulated to fall just above or below the cutoff. This asumption allows use to exploit the randomization around the cutoff and provides the variation needed to identify the causal effect of cutting road tax. We test this assumption using a density test detailed below and covariate balance tests (see Appendix B).

Density test. Classic regression discontinuity design (RDD) assumption states that agents cannot precisely manipulate the running variable to fall just above or below the cutoff. In our context, it means that the election results are not determined prior to when the ballot takes place. In other words, no individuals, organizations, higher levels of government, foreign governments, or the firm that programs the voting machines are dictating the precise vote share for the renewal road tax levy referendums raised by a city. The standard way to test this assumption is to perform a density test like that of Cattaneo, Jansson and Ma (2020), which is based on the idea that manipulation of elections might cause a clustering of votes just to one side of the cutoff, with a pronounced drop-off on the other side of the cutoff. The p-value of this density test is 0.98. A histogram of vote share is shown in Figure 2 that graphically illustrates the lack of abrupt change in density.
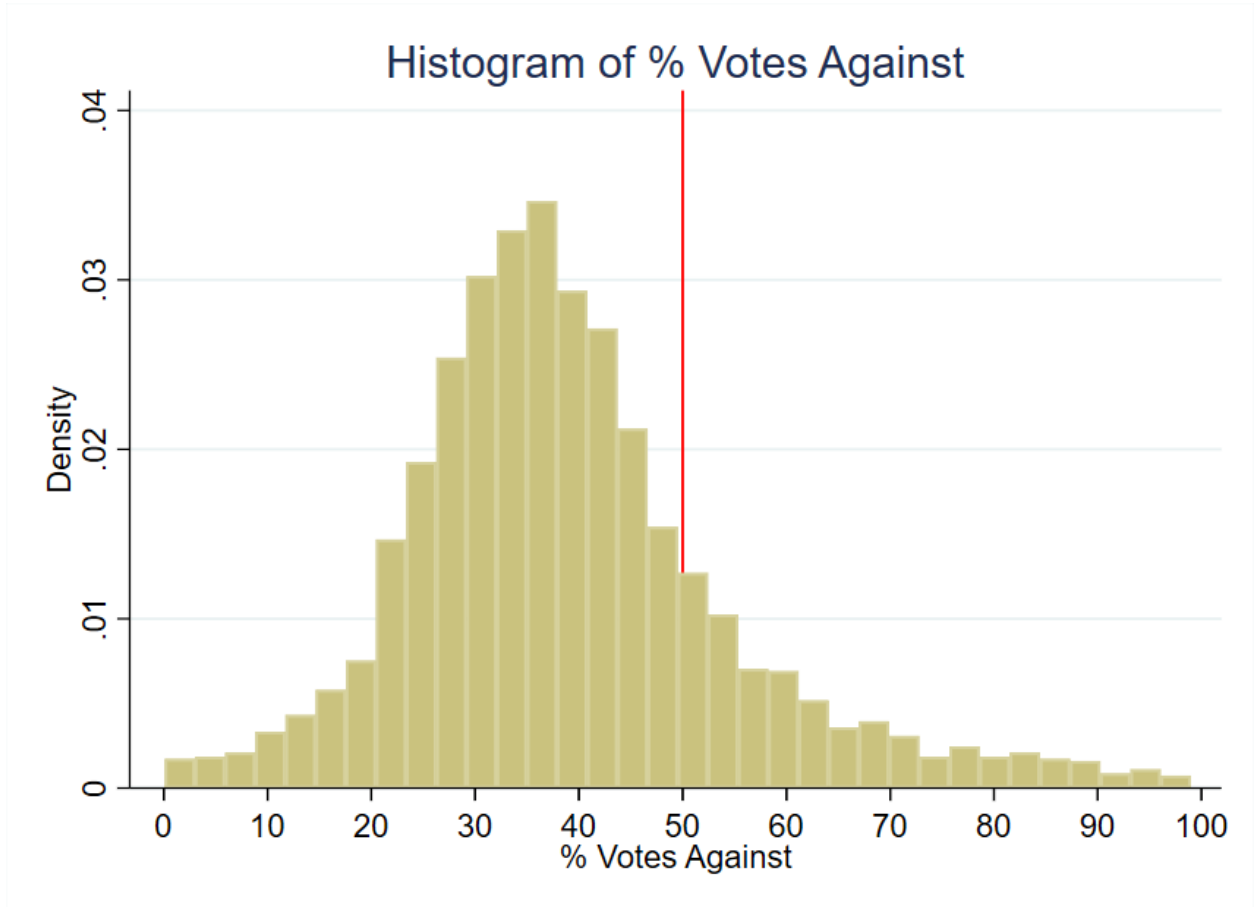

Figure 2: Histogram of Running Variable

Although Table 2 shows covariate balance between sets of cities that pass and fail to renew road tax levies, covariate values could still jump from one side of the cutoff to another. A drop in education levels, for example, could cause a drop in house prices that might coincide with a change in treatment, so that what might look like a treatment effect from cutting taxes and spending might in fact be caused by lower education levels. Graphs of covariate smoothness around the cutoff are found in the Appendix B. A formal way to assess covariate smoothness is to use each covariate as an outcome variable in a regression of the running variable and a treatment effect dummy. When we do so, the p-value of the treatment effect dummy varies from 0.13 to 0.98, indicating no statistically significant jump in covariate values.

2.5 Outcome Variables

Median Housing Prices. Our house price data comes from a CoreLogic® dataset of actual sales transaction prices in Ohio from 1995 through 2021 containing over 7 million observations. The dependent variable, *Median House Price*, reflects the median sale price of houses within a specific city and year. For example, for the houses sold in Delaware Township during the year 2002, the median sale price was $205,041. We take precaution to only include arm's-length transactions, and we restrict our attention to single-family residential structures for comparability. The overall sample mean for the 10-year period from the time of vote considered in this study is $166,082 in constant 2010 dollars with a standard deviation of $372,135 which suggests presence of some outliers. Although our use of median sale price addresses outliers, one of our robustness checks drops 1% tails and re-estimates the treatment effects. The mean for this winsorized sample is $150,375 in constant 2010 dollars with a standard deviation of $116,020.

Figure 3 shows house prices from 5 years after the vote graphed against the percent of votes for the tax levy. The points represent the mean house price for the 10 representative bins of vote shares for the average effective bandwidth around the cutoff of 50%. The
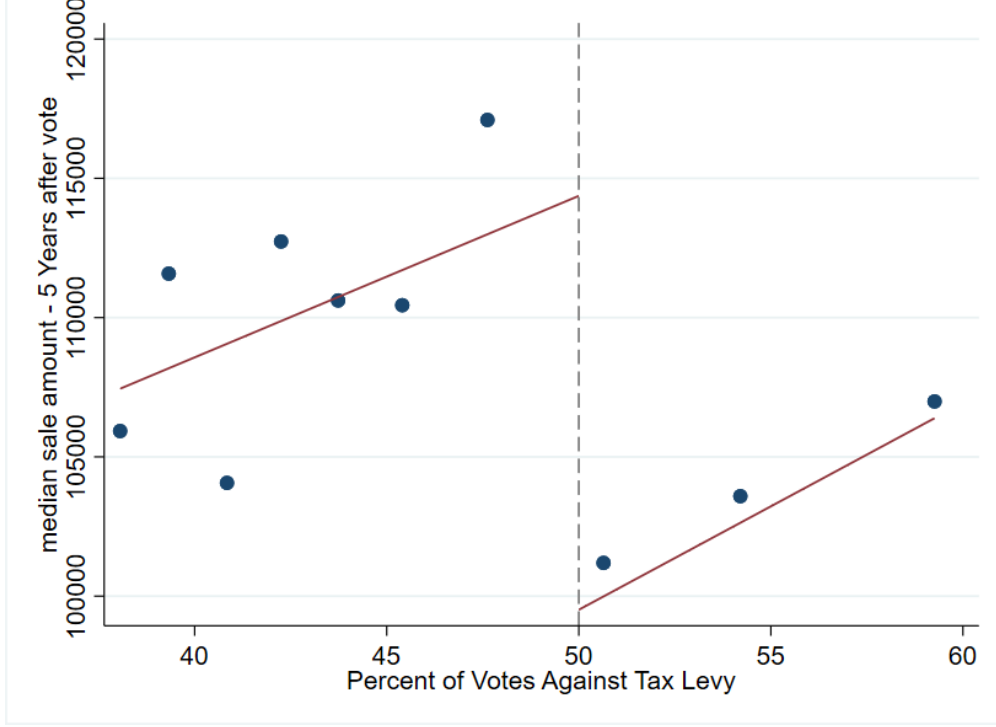
Figure 3: Median Sale Price of Homes: 5 years after vote

graph shows a clear discontinuity in house prices at the cutoff, which is the basis for our identification strategy. We observe a sharp decline after the cutoff in house prices, indicating that the failure to renew a road tax levy has a negative impact on house prices.

2.6   Covariates

Covariates can be useful in regression discontinuity studies, although they are not necessary for the identification of treatment effects. One use of covariates is to increase the precision of treatment effect estimates. The other is to see if cities that barely pass and fail tax levies are similar to each other like the theory of regression discontinuity says they should be. Table 2 shows covariate means for both the global sample of all votes in the data set as well as the local sample within a representative effective bandwidth of the 0.50 cutoff. The effective bandwidth displayed in Table 2 is the mean bandwidth for all the housing outcome regressions. The first columns for the global sample show similar values of characteristics between cities that renew and cut road taxes and spending, but it is the two rightmost

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columns that are critical for the credibility of the regression discontinuity design.

Table 2 demonstrates the covariate balance between cities that renew their tax levies and those that do not, indicating similar demographic and economic profiles across both groups. The data, captured at the time of the vote, shows minimal differences within the effective bandwidth: the mean population differs by only 301, and median family income varies by $436, measured in 2010 U.S. dollars. Other variables, including poverty rates, married household percentages, educational attainment, age distribution, and racial composition, show differences of two percentage point or less, bolstering the comparability of the two groups.

Table 2: Variable Means & Standard Deviation by Tax Levy Renewal Status

Variable	Global			Effective	
	Full Sample	Renewed	Cut	Renewed (Control)	Cut (Treatment)
Population	5,072	4,733	5,139	4,885	5,186
	(7,936)	(7,291)	(8,058)	(7,036)	(8,229)
Poverty Rate	0.11	0.11	0.11	0.10	0.11
	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)
% with Kids	0.39	0.40	0.39	0.39	0.39
	(0.08)	(0.08)	(0.08)	(0.07)	(0.08)
% Households with Children under 18	0.09	0.09	0.09	0.09	0.08
	(0.06)	(0.05)	(0.06)	(0.06)	(0.05)
% Less than High School Education	0.16	0.18	0.15	0.18	0.16
	(0.11)	(0.12)	(0.11)	(0.12)	(0.10)
% Some College Education	0.25	0.24	0.25	0.24	0.25
	(0.06)	(0.06)	(0.06)	(0.07)	(0.06)
% Unemployment Rate	0.05	0.05	0.05	0.05	0.06

Table 2 – continued from previous page

Variable	Global			Effective	
	Full Sample	Renewed	Cut	Renewed (Control)	Cut (Treatment)
	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)
% Renters	0.20	0.20	0.20	0.19	0.20
	(0.11)	(0.10)	(0.11)	(0.09)	(0.11)
% White	0.96	0.97	0.96	0.97	0.97
	(0.07)	(0.07)	(0.07)	(0.07)	(0.08)
% Black	0.02	0.02	0.02	0.02	0.02
	(0.07)	(0.06)	(0.07)	(0.07)	(0.07)
% Married	0.59	0.60	0.59	0.61	0.60
	(0.09)	(0.08)	(0.09)	(0.08)	(0.09)
% Separated	0.01	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Income Heterogeneity Index	0.10	0.09	0.10	0.09	0.09
	(0.08)	(0.07)	(0.08)	(0.07)	(0.06)
Median Family Income	61,018	58,761	61,467	59,934	60,370
	(17,649)	(13,915)	(18,270)	(13,655)	(15,713)
% Under 5 Years Old	0.06	0.06	0.06	0.06	0.06
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
% Aged 5 to 17	0.20	0.21	0.20	0.20	0.20
	(0.05)	(0.04)	(0.05)	(0.04)	(0.05)
% Aged 18 to 64	0.60	0.60	0.60	0.60	0.60
	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)
% Racial Minority	0.04	0.03	0.04	0.03	0.03
	(0.07)	(0.07)	(0.07)	(0.08)	(0.07)

Table 2 – continued from previous page

Variable	Global			Effective	
	Full Sample	Renewed	Cut	Renewed (Control)	Cut (Treatment)
Number of Observations	3,184	2,656	528	653	269

Table 2 shows that the treatment and control groups are well-balanced with respect to the covariates considered, suggesting that the groups are comparable and any observed differences in outcomes can more confidently be attributed to the treatment effect rather than to pre-existing differences.

3 Empirical Strategy

In this section, we describe our empirical strategy to estimate the causal effect of cutting road maintenance spending on different outcome variables. One key feature of our quasi-experiment design is the exogeneity of timing of the election. The timing of the election is determined by the natural expiration of the road maintenance tax levy, which is typically 5 years, and is not impacted by factors such as the prevailing economic conditions or whether a road tax levy was passed or failed in earlier years.

3.1 Regression Discontinuity in Panel Data setting

Suppose that local government in area i and year t has to conduct a referendum to renew their road tax levy. The referendum is conducted to determine whether the road maintenance tax levy should be renewed or not. Let v_{it} be the vote share against the renewal tax levy and v^* be the threshold determining the result of the referedum (levy fails to renew if $v_{it} > v^*$). Let $F_{it} = 1(v_{it} > v^*)$ be an indicator to represent if the renewal road tax levy fails, and y_{it} be some outcome variable of interest. We can write,

$$y_{it} = \alpha + \theta F_{it} + \epsilon_{it}$$

where α is the intercept, θ is the parameter of interest representing the causal effect of renewing a road tax levy and ϵ_{it} is the error term representing all other determinants of the outcome. Then, around a narrow enough window around the threshold v^*, we can estimate the causal effect of renewing a road tax levy on the outcome variable y_{it} by comparing the outcome variable for cities that narrowly passed the referendum to those that narrowly failed the referendum Lee and Lemieux (2010).

3.2 Intent-to-Treat (ITT) Estimator

We follow a model of Regression Discontinuity Design (RDD) similar to Cellini, Ferreira and Rothstein (2010) and estimate the **Intent-to-Treat** or ITT estimator. We prefer using ITT estimator instead of the alternative **Treatment on the Treated** (TOT) estimator because the ITT estimator is more suited to our setting given the independence of the renewal elections. As described in Cellini, Ferreira and Rothstein (2010), when the elections are independent, the ITT estimator equals the TOT estimator.

We operationalize our ITT estimator using the following regression equation -

$$Y_{i,t+\tau} = \alpha_\tau + \kappa_t + F_{it}\theta_\tau^{ITT} + P_g(v_{it}, \gamma_\tau) + Z_{it}\beta_\tau + \epsilon_{i,t+\tau}$$

Above, we have a municipality i that held an election in year t and we study this municipality's outcome τ years later. $Y_{i,t+\tau}$ represent the outcome variable for city, village or township i at year $t + \tau$. The outcome variables we study include house prices, wages, and employment. We define treatment as failure of a city, village or township to renew its road maintenance tax levy, which is represented by the indicator F_{it} and θ_τ^{ITT} is the causal effect of failing to renew road tax on the outcome. $P_g(v_{it}, \gamma_\tau)$ is a polynomial function of the running variable v_{it}, which is the percent of votes against the renewal tax levy. α_τ and κ_t represent

timing and year-specific fixed effects. We estimate θ_τ^{ITT} by comparing the outcome variable for cities that narrowly passed the referendum to those that narrowly failed the referendum.

We use the bandwidth selection method described in Calonico et al. (2019) to find the mean optimal bandwidth h and then conduct a local polynomial regression after choosing a weighting scheme k. The bandwidth h determines size of the neighborhood around the cutoff v^*, where the neighborhood is $(v^* - h, v^* + h)$. Only observations with values of the running variable within this neighborhood are used to compute the treatment effect estimate $\hat{\tau}$ and, for a small enough neighborhood, the continuity assumption central to the RDD estimator is satisfied. The weighting scheme k determines the weights of the observations within the neighborhood $(v^* - h, v^* + h)$ and is another important factor in determining θ_τ. The most common types of weighting schemes are uniform, triangular and Epanechnikov. We use the default Mean Squared Error Regression Discontinuity (MSERD) method to compute the effective bandwidth (h) and bias bandwidth (b) for each outcome variable. This method identifies the bandwidth that minimizes the trade-off between bias and variance of the treatment effect estimate. All observations are used to estimate h and b, but only the observations within the selected bandwidth h i.e. effective observations within h percentage points on either side of the vote share cutoff v^* are used to identify our treatment effect estimates θ_τ, for different τ years.

4   Results

4.1   Road Quality decline

To assess the impact of cutting local road taxes on road quality, we utilized a fine-tuned Vision Transformer (ViT) model, leveraging labeled satellite imagery data for U.S. roads. The ViT model, fine-tuned using a large dataset of road images, assigned quality ratings on a scale of 0 (poor) to 2 (high) to our collected satellite imagery for Ohio neighborhoods, as explained in Section 2.3. We compared road quality ratings from the fine-tuned ViT model

before and after the referendum in areas that failed to renew their road tax levy versus those that successfully passed the levy. These results are visually summarized in Figure 4, which shows the change in road quality ratings for both groups.

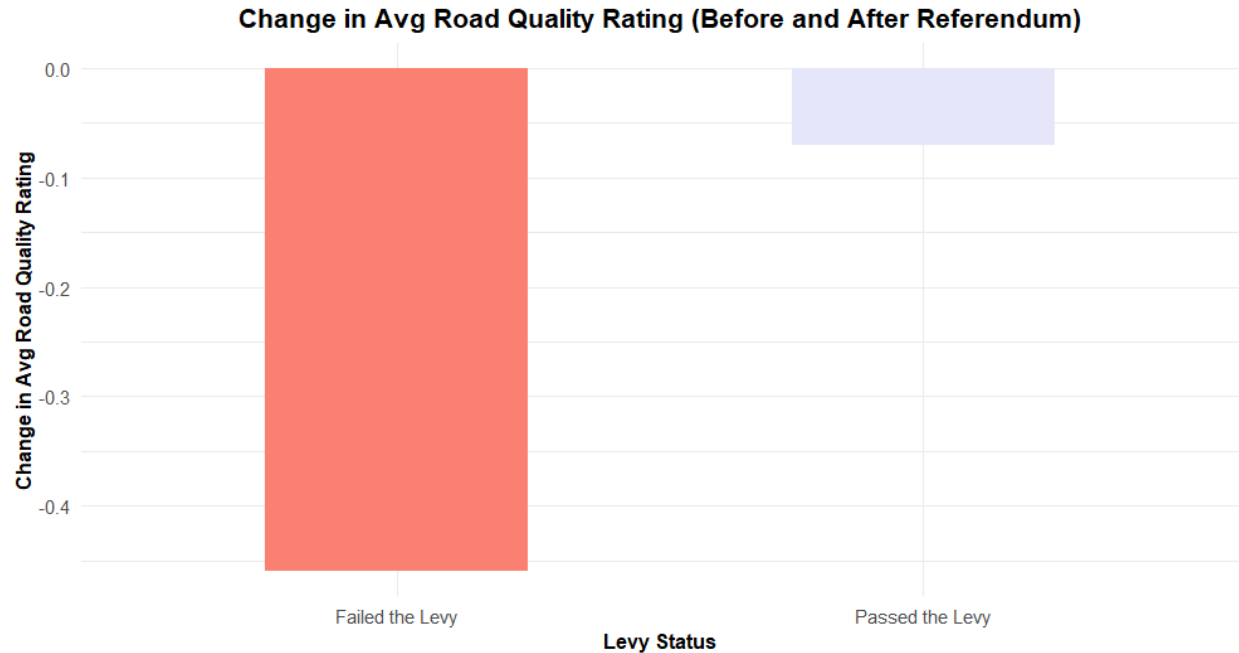**Change in Avg Road Quality Rating (Before and After Referendum)**

Figure 4: Change in Road Quality Ratings Before and After Referendum

Areas that failed to renew their tax levy experienced an average decline in road quality ratings of -0.5 points, equivalent to a 17% deterioration in road quality relative to the three-point scale. In contrast, areas that maintained their tax levies saw a negligible decline of -0.08 points, suggesting effectively no deterioration in road quality. In neighborhoods where renewal taxes were cut, the decline in road quality highlights the potential long-term implications of fiscal policy decisions on public infrastructure maintenance.

4.2 House Prices decline

Table 3 below shows the ITT estimates of failing a road tax levy on housing sale prices starting four years after the vote.

Each treatment effect estimate represents the discount in median sale price for cities that cut road taxes in the years after voting on a road tax levy relative to otherwise similar cities

Table 3: Effect on median house prices of failing to renew a road tax levy

Year relative to vote	$t+4$	$t+5$	$t+6$	$t+7$	$t+8$	$t+9$	$t+10$
Treatment effect	-21,684	-22,415	-17,539	-16,001	-21,973	-19,890	-16,042
Standard error	(7,838)	(8,348)	(8,465)	(6,918)	(9,111)	(7,756)	(8,915)
Effective bandwidth (h)	8.508	10.573	9.613	13.390	8.240	7.233	6.404
Bias bandwidth (b)	16.430	19.894	17.133	23.953	15.327	14.073	16.469
Effective Observations	469	605	525	801	402	324	260
Total Observations	2,614	2,531	2,438	2,324	2,199	2,115	2,017

Outcome is median house price in constant 2010 U.S. dollars. Unit of observation is the city-year, so a treatment effect of -\$21,684 means that four years after the vote, cities that failed to renew road taxes and its associated spending have houses that sell for \$21,684 less than cities that vote to renew road taxes and spending. Treatment effects for years prior to $t+4$ are statistically insignificant at the 5% level; full results shown in Table 7. The regressions include covariates related to the demographics and socioeconomic factors of the cities, drawn from Table 2.

that renew the taxes. Treatment effect estimates for years 4 through 9 after the vote are statistically significant in Table 3 as the p-values are below the canonical 0.05 threshold. The estimate for year $t+10$ has a smaller estimate and is only significant at the 10% level, suggesting that the effect of tax and service cuts on house prices may peter out ten years after the vote. Overall, we find an average reduction of \$15,349 in median house price over the 10 year period for houses in cities that vote to cut road tax levies representing 9% of overall home value.[2]

Figure 5 provides an event-study plot that summarizes the treatment effects for each time period. In the graph, we include placebo years up to 3 years before the treatment to

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[2]The average treatment effect estimate of \$15,349 was divided by the mean home sale price in the dataset of \$166,000 to get 9%.
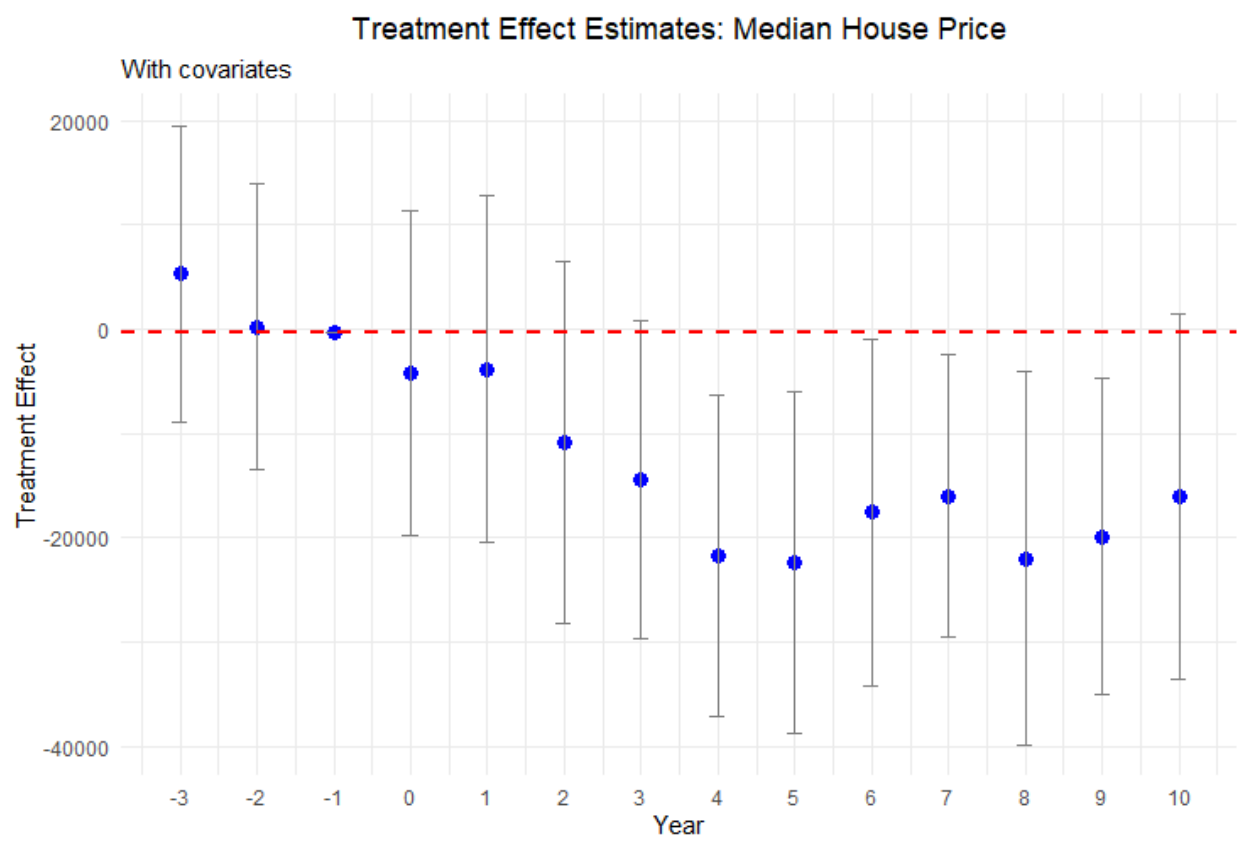
Figure 5: Effect plot for Median Housing Price

show that the median housing prices are statistically identical for cities above and below the threshold prior to the treatment. Each blue dot represents the treatment effect estimate for that year and the bar around it represents the 95% confidence interval for that estimate. For year 0, which is the year of the vote, we see a slight decrease in the estimate. However, this effect is not statistically significant, as evidenced by the confidence interval containing the null effect. Up to year 3, we observe that the treatment effect estimates are fairly close to zero, and the confidence interval overlaps with zero. As stated previously and shown in Table 3, we start to see a sizable increase in treatment effect from year 4 onwards and continue to observe it through year 9 after the vote.

4.3 Heterogeneity Analysis

We show the results of our heterogeneity analysis, where we explore the differential impact of cutting road maintenance spending on house prices based on urban vs rural neighborhoods, the size of the tax levy and housing price quantiles.

Urban vs Rural neighborhoods: We perform heterogeneity analysis to assess whether different types of neighborhoods and differing size of tax levies result in a differential impact on the housing prices. We first compare urban and rural areas in Ohio. We consider two different ways to define a city as urban: one including only urbanized areas and another one including both urban areas and urbanized clusters. The event study plot in Figure 6 shows this result.

As shown by Figure 6, we do not find any significant differences in housing prices after a renewal tax levy fails to pass for rural areas. On the other hand, we do find a statistically significant decline in housing prices in urban areas starting six years after voting. The standard errors are somewhat smaller for the rural estimates due to a larger number of observations. The difference in point estimates between urban and rural areas may stem from differences in housing supply elasticity (Brasington 2002). Overall, we find that housing
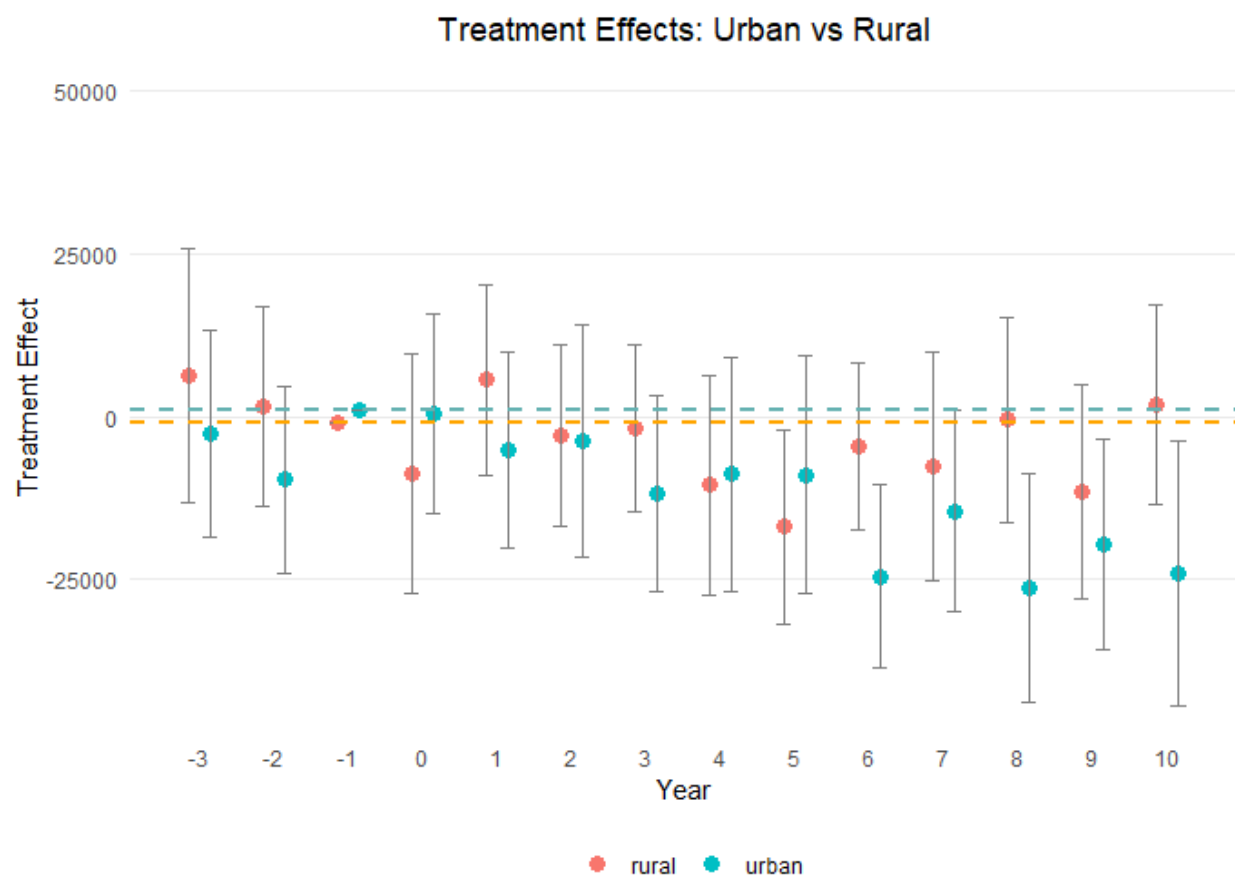
Figure 6: Median Housing Price in Urban and Rural Areas

prices decrease by \$13,302 on average over the decade[3] after cutting road maintenance tax levies in urban areas.

Tax magnitude: We check whether the treatment effect is greater for bigger tax levies than smaller tax levies. For this analysis, we split the tax levies based on the millage of a tax levy that is voted upon, where one mill is one tenth of one percent. Mean millage in our dataset is 1.9. We split the data into two parts: small tax levies ≤ 1.9 mills and large tax levies > 1.9 mills. We do not identify a consistent statistically significant difference in treatment effects between smaller and larger tax levies, suggesting a lack of dose-response. Figure 7 illustrates this result.
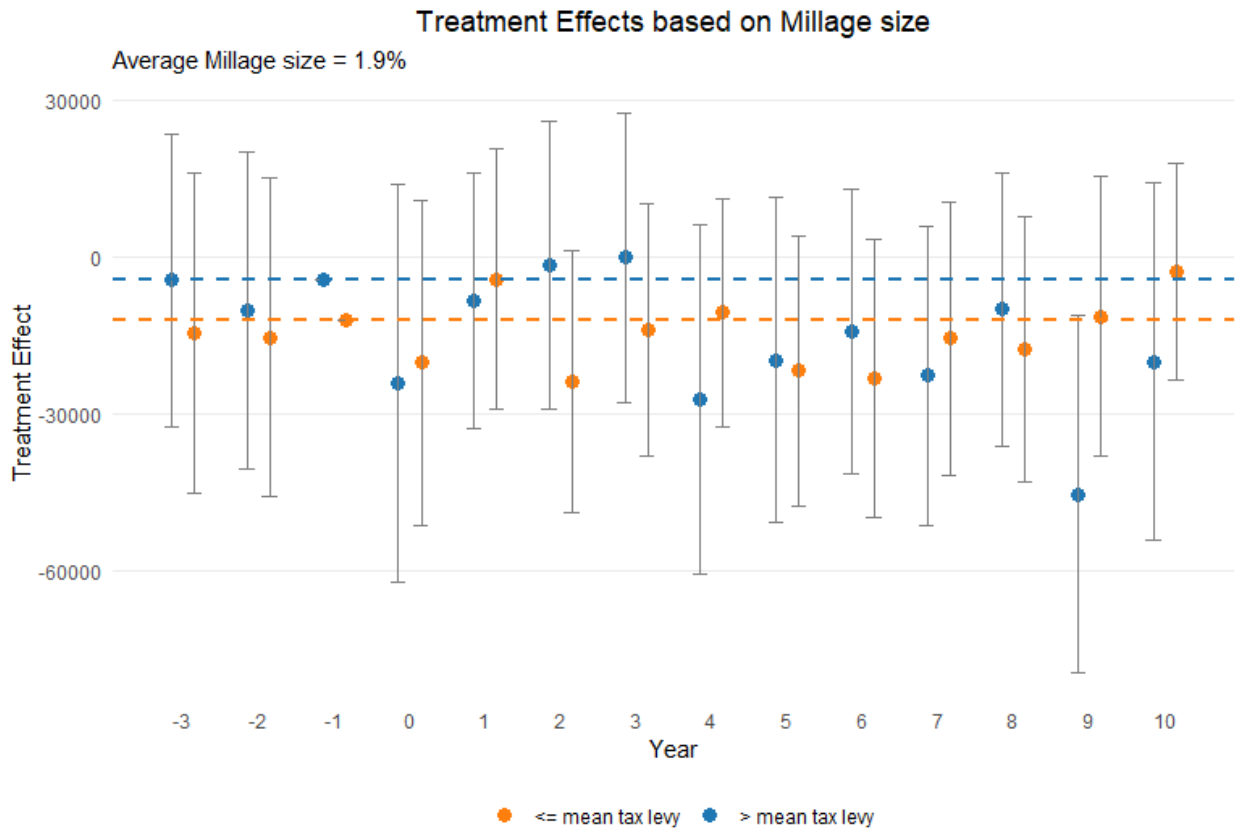
Figure 7: Median Housing Price based on Millage size

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[3]This is equal to $7.6\% = \frac{13,302}{175,217} \times 100$, where the denominator is average sale price of homes in urban areas in our dataset

All the years before the reduction in road spending have treatment effect estimates with confidence intervals containing zero. Starting in year $t+2$, we observe statistically significant treatment effects in some years after the vote such as year 5, 6 and 8 that show a for tax levies with millage size equal to or below the mean. However, we do not see a consistent decrease in housing prices. Hence, we conclude that the decrease in housing prices of cities that fail to maintain their roads does not vary significantly based on the size of the tax levy.

RDD Quantile Estimation: We further analyze our results by estimating quantile-level treatment effects, as suggested by Frandsen et al (2012), to study how the treatment's impact varies at different quantiles of the outcome variable.

Table 4: Quantile-level Treatment Effects of Cutting Road Spending on Median House Prices

Percentile	$t+4$	$t+5$	$t+6$	$t+7$	$t+8$	$t+9$	$t+10$
10%	-6,433	-22,570	-9,602	-12,984	-11,217	-6,569	-1,326
	(9,364)	(9,065)	(9,205)	(8,420)	(9,136)	(10,809)	(8,793)
20%	-5,400	-15,070	4,014	-14,682	-15,040	-3,228	624
	(9,983)	(9,886)	(7,443)	(8,502)	(8,160)	(10,435)	(8,509)
70%	-21,760	-11,171	-38,082	-36,685	-21,356	-25,605	-18,600
	(12,333)	(11,806)	(12,835)	(12,163)	(12,218)	(13,984)	(9,872)
80%	-28,478	-16,379	-38,460	-37,470	-28,950	-27,800	-18,658
	(13,343)	(11,404)	(18,623)	(12,169)	(12,507)	(12,421)	(11,808)
90%	-51,470	-34,604	-38,510	-27,039	-29,010	-49,093	-36,662
	(18,409)	(15,837)	(22,194)	(16,308)	(16,640)	(14,498)	(19,110)

The outcome is median house price in constant 2010 U.S. dollars. The unit of observation is the city-year, so a treatment effect of -\$28,478 means that at the 80th percentile of house prices four years after the vote, cities that fail to renew road taxes and its associated spending have houses that sell for \$28,478 less than cities that vote to renew road taxes and spending. The regressions include covariates related to the demographics and socioeconomic factors of the cities, drawn from Table 2.

Table 4 shows the treatment effect heterogeneity of cutting road spending on high and low quantiles of median house prices. The top percentiles consistently exhibit a statistically significant decline in house sale prices, beginning in year 6 after the reduction in road spending. In contrast, the lower percentiles do not demonstrate a consistent treatment effect. This
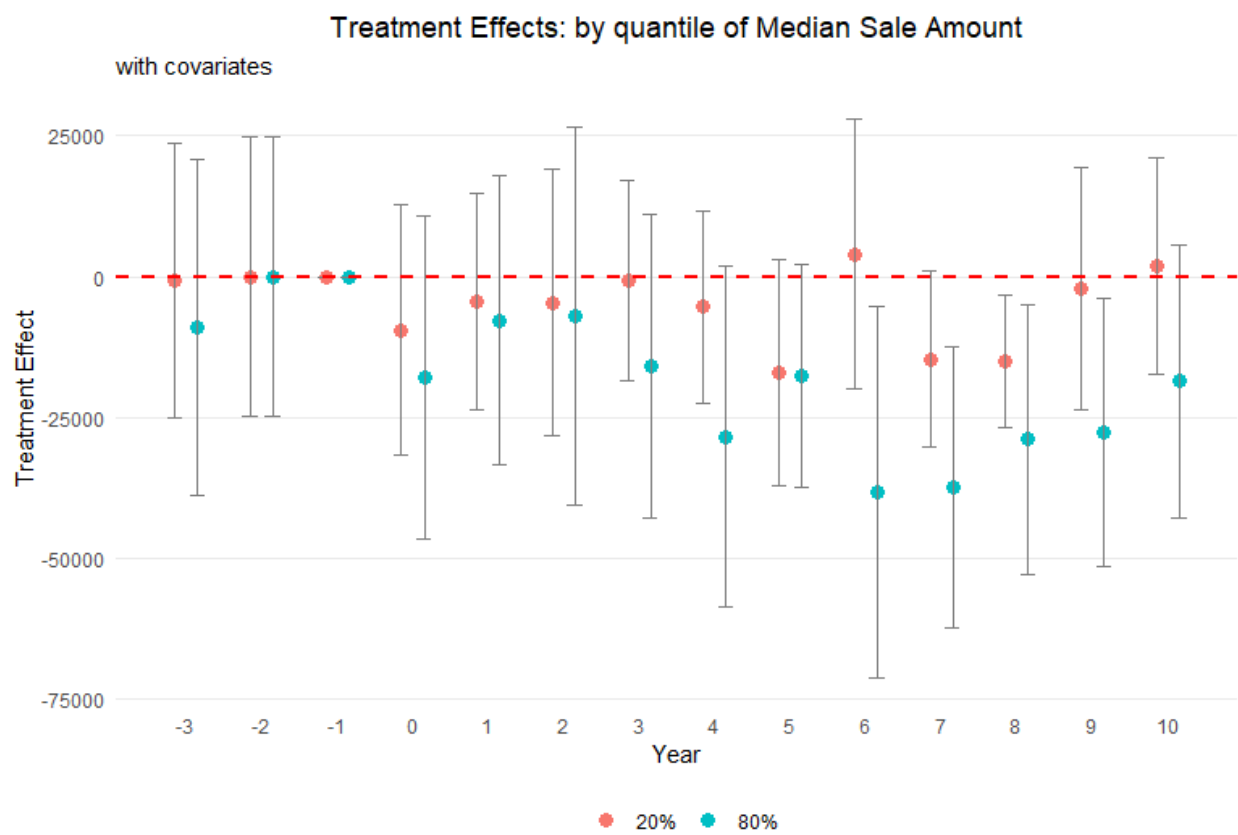
Figure 8: Median Housing Price based on Quantiles: 20% and 80%

suggests a differential impact, where higher-valued properties are more sensitive to road disrepair than lower-valued houses. Figure 8 contrasts the treatment effects of the 20th and 80th percentiles of home sale prices in an effect plot to highlight this differential impact of reduction in road maintenance spending.

4.4 Robustness Tests

We conduct several robustness tests to ensure the validity of our results. We test the sensitivity of our results to different bandwidths, covariates, and functional forms. We also check for the presence of pre-trends and perform a placebo test to confirm the validity of our RDD.

Removing contradictory observations: In this test, we focus on ensuring the independence and exogeneity of our dataset, a critical aspect in assessing the impact of renewal tax levies on housing prices. An additional concern is the potential bias introduced by tax levies for additional money that might pass after renewal tax levy decisions. To address this concern, we exclude observations from our analysis if a tax levy for additional funding is introduced and passed within a ten-year period following a renewal tax levy vote. This approach is premised on the notion that the introduction of new funding through additional levies could confound the effects of decreased road taxes on housing prices. For example, consider a scenario where a city votes on a renewal tax levy in the year 2000. If that city subsequently introduces and passes a tax levy for additional road spending in 2004, we exclude all votes for that city from 2005 through 2010. This exclusion ensures that the effect on house prices from the 2000 vote are captured for uncontaminated years but not for years after 2004 when the effect of additional road taxes may counteract the drop in tax money from the year 2000 vote. It allows us to isolate and examine the pure impact of the drop in funding from failing renewal levies on housing prices.

Upon implementing this data filtration, we observe that the treatment effect of the renewal levies on housing prices, measured from $t+1$ to $t+10$, remains largely consistent with

Table 5: Effect on Median Sale Amount of Failing to Renew a Road Tax Levy

year relative to vote	$t+4$	$t+5$	$t+6$	$t+7$	$t+8$	$t+9$	$t+10$
Treatment effect	-19,015	-15,609	-18,403	-20,842	-18,950	-28,634	-23,823
standard error	(9,563)	(10,283)	(10,246)	(9,629)	(9,179)	(7,770)	(10,594)
Effective bandwidth (h)	6.88	5.92	8.36	9.42	7.91	5.43	7.05
Bias bandwidth (b)	12.14	15.94	14.53	17.87	15.24	11.90	17.33
Effective Observations	475	389	561	611	475	300	374
Total Observations	2,389	2,274	2,145	2,016	1,890	1,787	1,666

The outcome is median house price in constant 2010 U.S. dollars. The unit of observation is the city-year, so a treatment effect of -$19,015 means that four years after the vote, cities that fail to renew road taxes and its associated spending have houses that sell for $19,015 less than cities that vote to renew road taxes and spending. The regressions include covariates related to the demographics and socioeconomic factors of the cities, drawn from Table 2.

our initial findings. This consistency in treatment effect, despite the exclusion of potentially confounding data, lends further credence to our results. The standard errors increase slightly due to reduction in sample size caused by the aforementioned data filtration process.

Placebo cutoffs: In our primary analysis, the pivotal threshold for the vote share running variable is 50%, indicating whether a renewal levy passes or fails. Although we find significant treatment effects using this 50% threshold, it could be random jumps in the data rather than cutting road taxes and funding that are responsible for the significant estimates. To this end we conduct a series of placebo tests using alternative cutoffs: 30%, 40%, 60%, and 70%. Table 6 below summarizes the results from the placebo cutoffs analysis.

Table 6 does not show consistently significant treatment effects for any of the placebo cutoffs for our parameter of interest. This absence of significance at thresholds other than the true 50% reinforces the idea that the effects we observe at the 50% mark are not a mere coincidence or a result of random variation in the data, but are indeed attributable to the

Table 6: Robust Treatment Effect Estimate for Placebo Cutoffs

Years after vote	30%	40%	60%	70%
$t+4$	2,578	9,149	9,419	-12,987
	(8,209)	(7,284)	(11,462)	(14,365)
$t+5$	-6,381	-29,077	6,383	41,683
	(9,086)	(20,680)	(10,786)	(17,836)
$t+6$	7,681	5,573	-1,095	-14,226
	(9,616)	(8,120)	(8,612)	(15,733)
$t+7$	1,162	3,982	-12,050	22,261
	(10,468)	(8,191)	(9,396)	(19,765)
$t+8$	4,334	12,881	3,593	31,696
	(9,670)	(8,625)	(10,061)	(6,902)
$t+9$	851	7,381	-6,935	42,790
	(5,921)	(6,333)	(7,114)	(15,663)
$t+10$	10,032	-35,038	324	-8,566
	(10,599)	(35,569)	(8,220)	(17,281)

Robust treatment effect estimate for placebo cutoffs as per the estimator from Calonico et al. (2017). The unit of observation is city-year level. Standard errors are shown in parentheses below each estimate.

dynamics surrounding the passing or failing of renewal tax levies.

Winsorization: The debate over whether to include or exclude outliers continues, with some research suggesting that trimming outliers does not improve mean squared error (e.g., Bollinger and Chandra, 2005). We now drop the 1% tails to help curtail the influence of outliers. The overall sample mean after dropping 1% tails is $144,268 in constant 2010 dollars with a standard deviation of $109,624. After performing this winsorization step, we re-estimate the treatment effect of failing to renew a road tax levy on housing outcome variables. The results from this estimation process are summarized in Figure 9 below:

The treatment effect estimates with winsorization mimic those from our baseline regression results qualitatively and quantitatively.
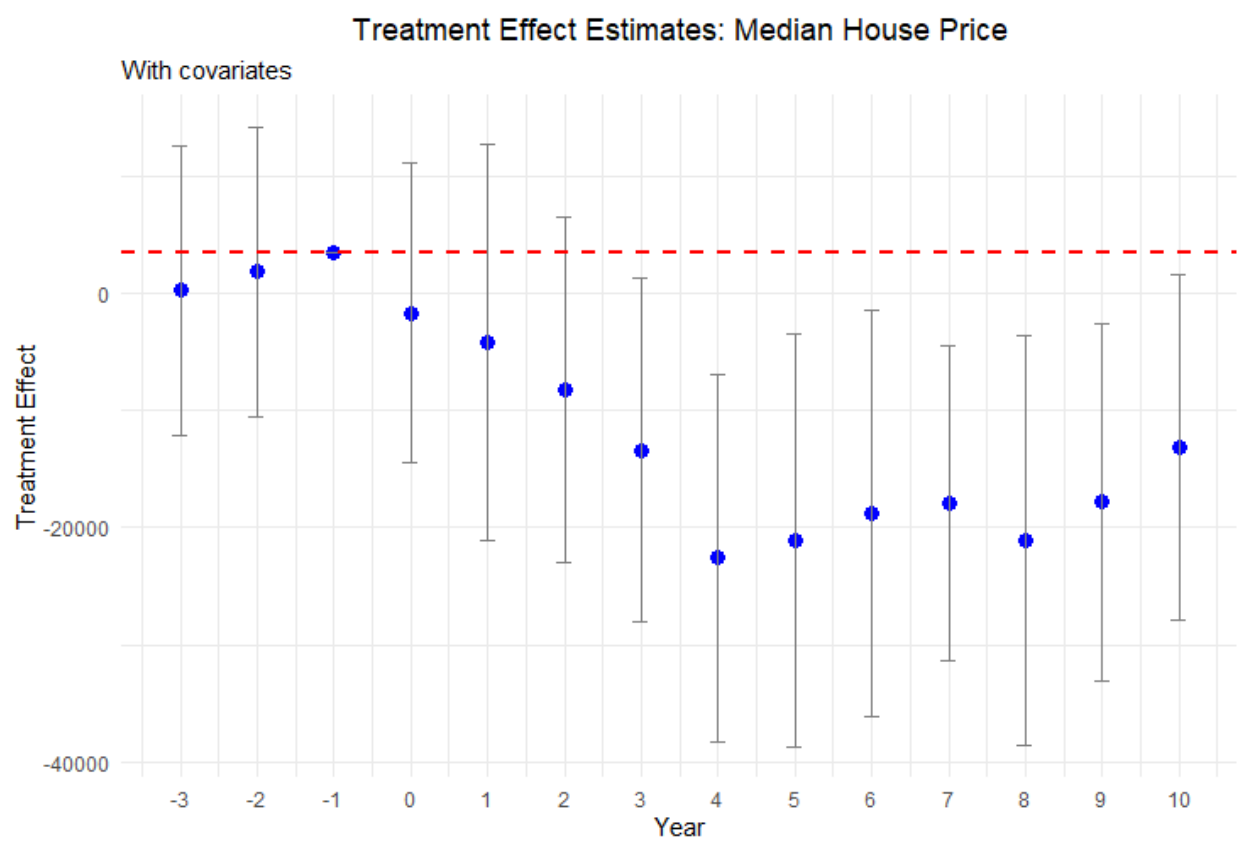
Figure 9: Median Housing Price after 1% Winsorization

5 Mechanisms

A key question surrounding our findings is *why* cutting road taxes would lead to a sustained decline in local house prices. In this section, we argue that these results are consistent with classical insights from the literature on local public finance, especially the work of Tiebout (1956) and Oates (1972). Specifically, cutting road-maintenance tax levies reduces local government funds, which in turn constrains the government's ability to provide road-upkeep services. This deterioration in road quality then directly and indirectly lowers housing values. Three interconnected channels explain this relationship.

Road Tax Cuts and Reduced Road Maintenance Funds. Local governments in Ohio, like those in many other U.S. states, rely on property taxes and dedicated road levies to fund local public goods. The Oates (1972) decentralization theorem highlights that local authorities can generally provide public services in a manner better aligned with resident preferences than would a higher-level government, provided they have adequate revenue sources. When a renewal levy fails at the ballot box, one of these critical revenue sources vanishes. Consistent with the premise that local road quality is a "local public good," losing a dedicated revenue stream substantially weakens a city's ability to maintain or upgrade roads. Empirically, we surmise the size of the drop in maintenance funds available to local governments (see Section 1). In the spirit of Tiebout (1956), local residents effectively "vote" on their tax and expenditure bundles, which results in a budgetary contraction. Absent compensating resources from state or federal authorities, the lower tax revenue directly reduces spending on maintenance, with ripple effects for local roads and infrastructures.

Declining Road Quality. Results from our AI model-based road-quality measure in Section 4.1 show a marked decrease in road quality following the cessation of renewal levies. The resulting deterioration in road quality may be accompanied by ride discomfort, declining aesthetics, and reduced usability of neighborhood roads. This is precisely what a local public goods framework would predict: fewer financial resources for upkeep immediately impact the daily "consumption" of roads. Residents observe these changes slowly over time, often after

four to five years, before road conditions become visibly poor.

Capitalization into house Prices. Falling road quality imposes a disamenity on local residents. As recognized by Oates (1969), property values in a community incorporate expectations of both the level of taxation and the quality of local services. While homebuyers benefit from understanding they might pay lower property taxes in a municipality with a failing levy, they also factor in the anticipated (and eventually realized) deterioration of roads. Several mechanisms help explain the ultimate discount in property values:

1. **Appearance and Neighborhood Appeal.** Rough roads, potholes, and poorly patched surfaces reduce aesthetic appeal. Potential buyers, seeing visible signs of neglect, bid less.

2. **Vehicle Damage and Travel Costs.** Chronic maintenance problems translate into higher expenditures on tires, suspensions, and alignments, as well as more time lost to slower commutes.

3. **Expectation of Reduced Public Services.** A failed renewal levy may signal broader fiscal stringency, causing residents to expect other future service cutbacks. Uncertainty about service levels weighs negatively on property values.

In this sense, the local "bundling" of taxes and roads is priced into real estate transactions, echoing the original proposition of Tiebout (1956) that local public goods are *capitalized* in property values. Our event-study evidence confirms that the negative price effects are most pronounced several years after the renewal levy fails, consistent with the time needed for visible road quality decline to become salient to homeowners and prospective buyers.

Short-Run versus Long-Run Trade-Off. Importantly, a trade-off emerges over time. In the short run, residents who voted down the road tax levy experience immediate relief in terms of lower property tax bills. Their out-of-pocket expenses decline, which can be a tangible financial benefit—particularly in communities where property tax burdens were already viewed as high. However, as road quality starts to visibly deteriorate from around

the fourth year onward (see 3), the same residents find themselves negatively affected by a decline in house prices. This lag reflects the time it takes for infrastructure disrepair to become apparent, be it through more frequent potholes or visibly eroding surfaces. By the time these problems are indisputable, the neighborhood's real-estate market has had sufficient time to register the disamenity, capitalizing it as a discount in property values. This dynamic captures a fundamental insight of local public finance: the preferences of current taxpayers can diverge from the longer-term public interest (Buchanan and Tullock, 1962; Alesina and Tabellini, 1990). While voters may respond to short-term cost savings from a failing levy, the forgone maintenance ultimately reduces the community's appeal and leads to diminished investment returns—particularly for homeowners whose largest asset is their property. Hence, the immediate reduction in tax liabilities ultimately collides with the reality of lower market valuations once inadequate road upkeep becomes plainly visible.

In summary, our findings align with longstanding theories of local public finance. Under the logic proposed by Oates (1972), letting local jurisdictions decide on road tax levies can promote efficient provision of local services, as long as there is sufficient willingness among residents to sustain the quality of road infrastructure. However, once communities choose (either inadvertently or intentionally) to forgo a renewal levy, the immediate reduction in funds and subsequent lagged deterioration in road quality erode property values. From the household's perspective, subpar road maintenance becomes an undesirable aspect of the local package they are effectively "purchasing" when buying a home, leading to a downward shift in housing demand—and hence, in equilibrium sale prices.

6 Conclusion

A great deal of existing research has focused on the effect of new roads on house prices, especially in developing nations, providing valuable policy insights and spurring development initiatives like China's Belt (Huang, 2016) and Road Initiative and India's Pradhan Mantri

Gram Sadak Yojana (Asher and Novosad, 2020). However, the endogeneity of road placement makes it difficult to identify causal effects. In this paper, we study taxes that fund maintenance of existing roads and avoid endogeneity issues by utilizing a quasi-experiment setting that naturally arises from the voted levies enacted by local governments in Ohio. We study more than 3,000 referendums raised by cities, villages, and townships to renew local road taxes funding existing roads and use dynamic regression discontinuity design to estimate the effect of failure to renew a levy on housing prices. We also estimate the decline in spending experienced by local governments when they face a tax cut due to failure of a renewal tax levy and fine-tune a Vision Transformer model to predict and identify the changes in road quality in these areas. We find an average decline of 17% in road quality in areas that cut their renewal taxes and a decline in home sale price of 9% over the 10-year period. We find no anticipation effects, unlike Beenstock, Feldman and Felsenstein (2016) and Diao, Leonard and Sing (2017), but we find statistically significant effects starting four years and continuing onto the later years. We find these effects to be more consistent for urban rather than rural areas. We find a lack of dose-response based on the size of the levy, but we find larger house price reductions for more expensive houses than for cheaper houses. Future research could extend the dynamic regression discontinuity identification strategy we employ to other geographies, using vote share as a running variable for places that vote on road spending, or using time as a running variable for cities that directly change road spending without a referendum. Moreover, it could also shift away from studying votes to maintain spending toward studying votes to increase road spending, although the endogeneity of choosing when to propose such a referendum makes identification for such settings more challenging.

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Appendix

A Additional Treatment Effects Tables & Effect Plots

A.1 Full set of Treatment Effects for Housing Prices

The full set of treatment effects provided in Table 7 support Table 3 which provides treatment effects for the housing price outcome variable in the main body of the paper. These treatment effects are estimated using a regression model that controls for various factors.

Table 7: Full set of estimates - Median Housing Price

Year relative to vote	Estimate	Standard error	p-value	Confidence interval
$t-3$	3,468	7,465	0.642	[-11,164, 18,099]
$t-2$	-1,703	6,617	0.797	[-14,673, 11,267]
$t-1$	1,784	6,950	0.797	[-11,838, 15,407]
$t+1$	-6,197	9,662	0.521	[-25,134, 12,741]
$t+2$	-17,059	9,518	0.073	[-35,715, 1,597]
$t+3$	-9,823	8,244	0.233	[-25,981, 6,335]
$t+4$	-19,535	9,289	0.035	[-37,741, -1,329]
$t+5$	-21,531	9,147	0.019	[-39,459, -3,604]
$t+6$	-16,994	7,558	0.025	[-31,809, -2,180]
$t+7$	-16,991	7,357	0.023	[-31,111, -2,272]
$t+8$	-23,323	9,449	0.014	[-41,842, -4,803]
$t+9$	-30,620	9,586	0.001	[-49,408, -11,833]
$t+10$	-16,411	9,342	0.079	[-34,721, 1,898]

Supplements Table 3 in text. Full set of treatment effect estimates of renewing road tax levies relative to cutting road tax levies from 3 years before the vote to 10 years after the vote. Covariates from Table 2 used in all regressions. Outcome is median house price in constant 2010 U.S. dollars. Unit of observation is the city-year. A treatment effect of -$19,535 means that four years after the vote, cities that vote to cut road taxes and its associated spending have houses that sell for $19,535 less than cities that vote to renew road taxes and spending.

B Additional Robustness Tests

B.1 Covariate Discontinuity Tests & Smoothness Plots

Table 8: Covariate Discontinuity Test Results

Variable	Estimate	Standard error	p-value	Confidence interval
Population	-388	1,094	0.722	[-2,532, 1,755]
Poverty Rate	0.017	0.014	0.234	[-0.011, 0.045]
% with Kids	-0.007	0.012	0.539	[-0.030, 0.015]
% Households with Children under 18	0.0001	0.007	0.981	[-0.014, 0.014]
% Less than High School Education	-0.004	0.020	0.834	[-0.043, 0.035]
% Some College Education	-0.012	0.011	0.274	[-0.034, 0.009]
% Unemployment Rate	-0.002	0.006	0.733	[-0.013, 0.009]
% Renters	-0.005	0.015	0.754	[-0.035, 0.025]
% White	-0.007	0.011	0.499	[-0.028, 0.014]
% Black	-0.004	0.009	0.685	[-0.021, 0.014]
% Married	-0.013	0.015	0.374	[-0.042, 0.016]
% Separated	0.001	0.002	0.485	[-0.002, 0.004]

Estimates indicate the treatment effect of failing to renew a road maintenance tax levy on each covariate considered during our study. Confidence intervals are presented in square brackets.

Pct Rent

Poverty Rate

Pct With Kids

Pct Single Parent Hhld

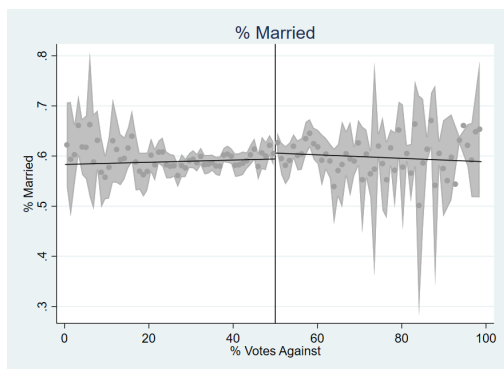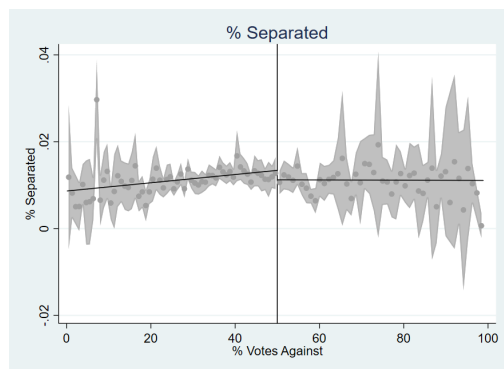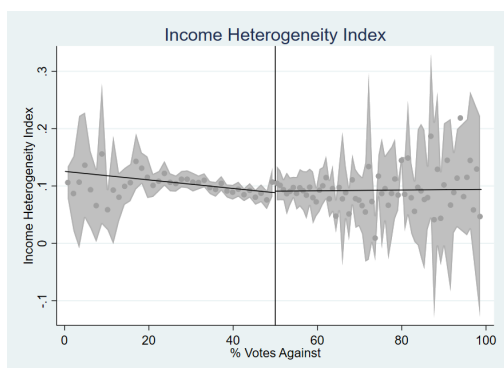Pct Less than HS

Pct Some College

Pct Black

Population
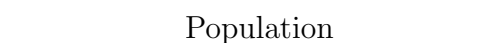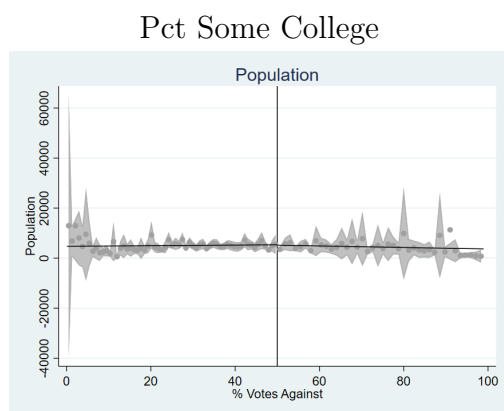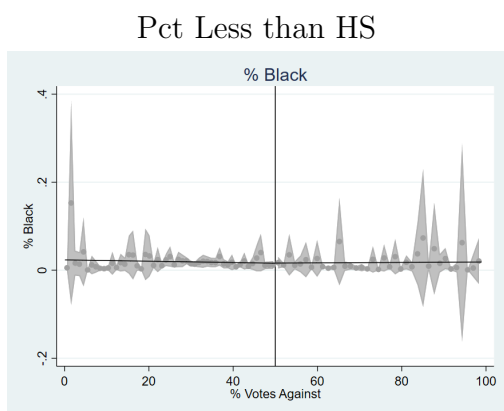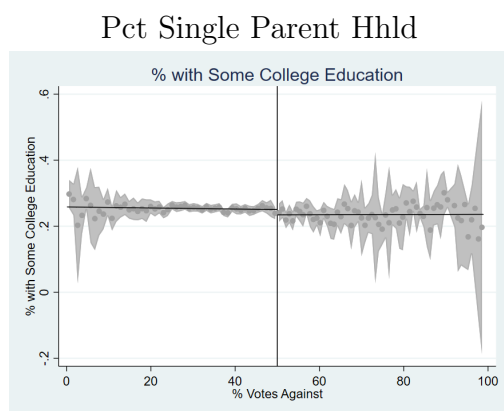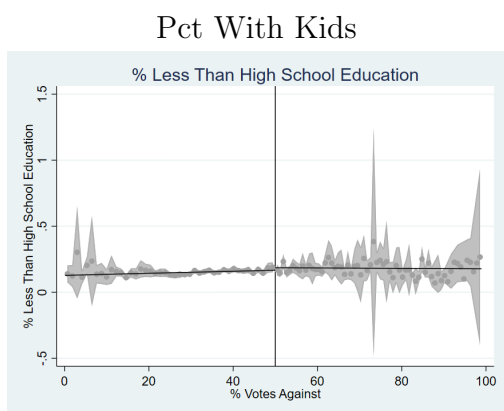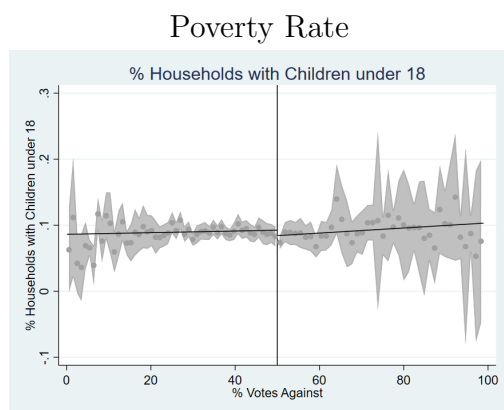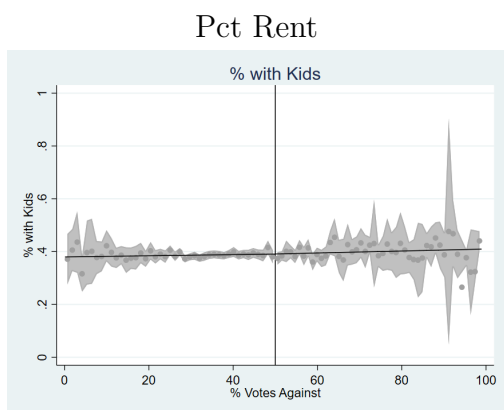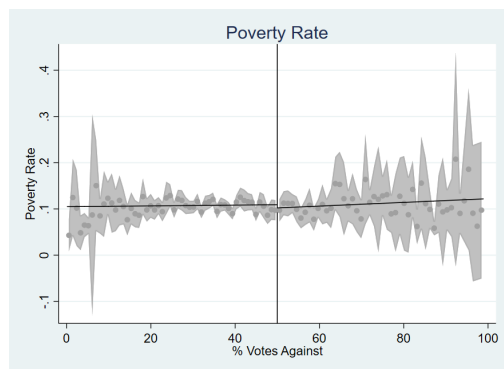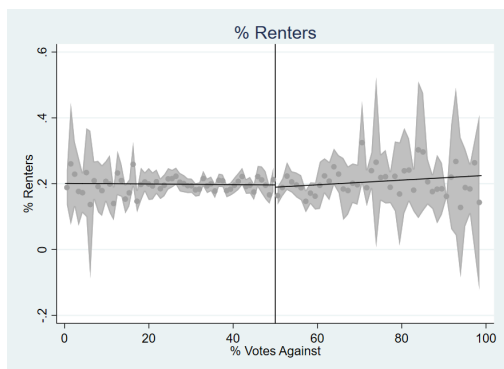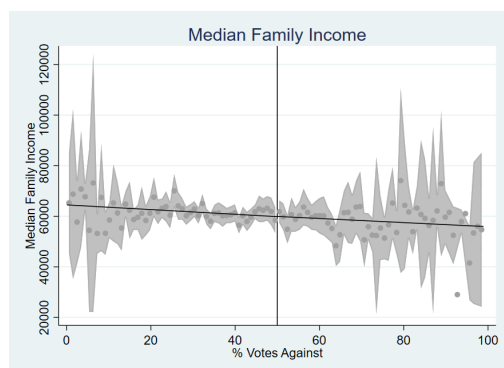
Figure 10: Covariate Discontinuity Plots - Part 1
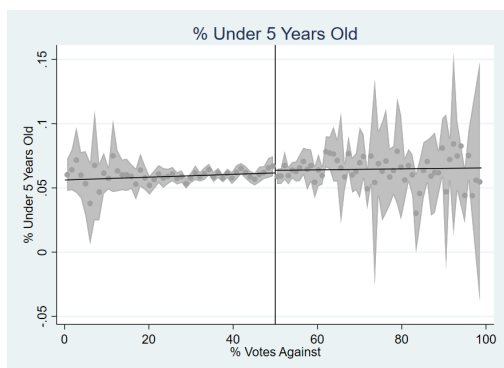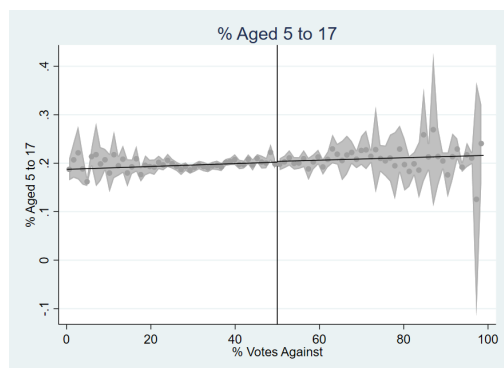
Pct Married
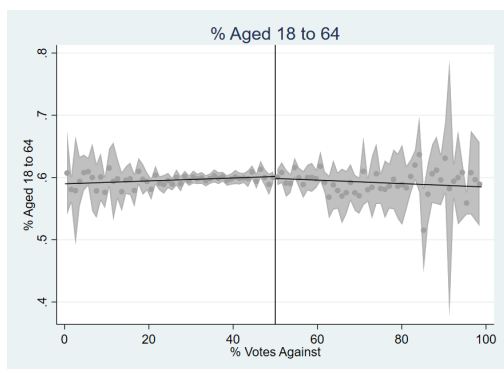

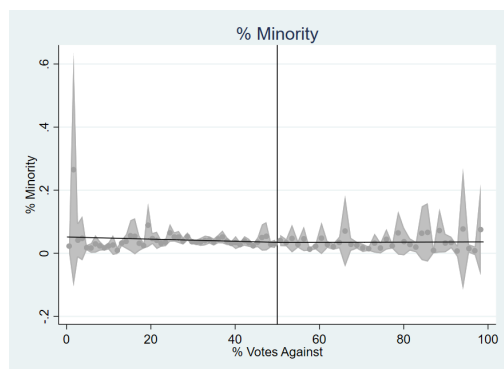
Pct Separated

Income Herfindahl Index

Median Family Income

Pct Less than 5

Pct 5 to 17

Pct 18 to 64

Pct Minority

Figure 11: Covariate Discontinuity Plots - Part 2

C   Additional Information

C.1   More Details on Predicting Road Quality

Coming very soon.

C.2   Road tax levies vs. Other types of levies

Table 9: Correlation of Road Tax Levy Referenda Results with Other Types of Levies

	Police	Fire	Current Expenses	Recreational	School
Estimate	0.248	0.053	0.388***	0.015	-0.024
	(0.153)	(0.043)	(0.099)	(0.317)	(0.092)

Notes: This table presents coefficients from regressions correlating road tax levy referendum outcomes with outcomes for other types of levies (police, fire, current expenses, recreational, school). Standard errors are reported in parentheses. The coefficient for "Current Expenses" is statistically significant at the 1% level (***). All regression control for year and neighborhood fixed effects, as well as neighborhood characteristics. The school levy analysis is conducted at the county level, while all other analyses are at the county subdivisions level. Statistical significance levels are indicated as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.