The Effect of Local Road Maintenance Tax Cuts on House Values *

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Abstract

Most studies focus on the impact of new public infrastructure on local housing markets. In this paper, we avoid endogeneity issues coming from the placement of new infrastructure by studying the maintenance of existing infrastructure and design a quasi-experiment to study local referendums conducted to renew property taxes levied for road maintenance. We compare housing sale prices between similar areas that narrowly pass or fail renewal road tax levies and use satellite images to fine-tune an Artificial Intelligence (AI) vision model for classifying road quality. Our results show that local jurisdictions with close elections that decide to cut road taxes face an average loss of 11% in road maintenance funds, experience a 23% decline in road quality, and suffer a 9% drop in house prices over the course of 10 years relative to areas that renew funding. Heterogeneity analysis reveals a stronger percentage decline in urban areas and for expensive houses relative to rural areas and cheaper houses, along with evidence for dosage-response for larger tax cuts. We rationalize these findings using a Dynamic General Equilibrium (DGE) framework and show that the short-run gains from tax cuts are outweighed by the long-run capitalization of infrastructure decay, revealing how public disinvestment negatively affects property values.

JEL Codes: R42, H71, R10

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1 Introduction

Roads are an important form of infrastructure investment that affect households' amenity values, firms' production functions, and people's commuting costs. 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 and Road Initiative (Huang, 2016) 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 avoid endogeneity issues by studying property taxes that fund the maintenance of existing roads and by utilizing a quasi-experimental setting that arises from analyzing close elections.

We establish a novel dataset that allows us to study how changes in local taxes affect local infrastructure maintenance and neighborhood house prices. We match voting data on local road maintenance taxes from the Ohio Secretary of State with financial records of cities from Ohio Auditor of State and property sale prices from CoreLogic. Our data also includes remote sensory data of local roads in Ohio from Google Earth Pro. Using these rich data, we first estimate the drop in the maintenance budget for a local government after a tax cut from a failed renewal of road maintenance levy. We fine-tune an Artificial Intelligence (AI) vision model using more than 53,000 satellite images from Brewer et al. (2021) to predict changes in road quality after a road maintenance tax cut. Finally, we use an Intent-to-Treat (ITT) Regression Discontinuity (RD) estimator to estimate the effect of cutting local road maintenance taxes on housing prices. Our focus on road maintenance instead of new development, our examination of a developed economy, and our use of a vision model to measure a change in road quality distinguishes our study from the existing literature. Our results reveal that when a city cuts its renewal road taxes, it experiences an 11% loss in maintenance funds, its road quality declines by 21% and its house prices decrease by around $15,350 (9%).

These results are consistent with other work on the impact of roads on house prices.

For instance, Gonzalez-Navarro and Quintana-Domeque (2016) finds that paving roads in Acayucan, Mexico, increases property values by 28%, while Theisen and Emblem (2021) finds 13% higher housing prices in towns nearest to a new highway in Norway. Unlike Beenstock, Feldman and Felsenstein (2016) and Diao, Leonard and Sing (2017), we do not find any anticipation effects but we do find statistically significant effects starting in the fourth year after the vote and continuing at least through the ninth year. This delayed effect highlights how the consequences of road tax cuts are not immediate but gradually accumulate as funding is reduced and roads deteriorate, with these effects to be more pronounced for urban rather than rural areas. We also find an effect on the intensive margin with evidence for dosage-response based on the size of the levy, along with larger house price reductions for more expensive houses than for cheaper houses.

2 Literature Review

Roads and House Prices. Gonzalez-Navarro and Quintana-Domeque (2016) studies the effects of paving roads in Acayucan, Mexico. The government identified 56 neighborhoods that needed paved roads. Of these, 28 were randomly selected for paving. About 1,000 people were surveyed in these neighborhoods before paving (2006) and after (2009), although 11 of the 28 treatment groups were still in the process of being paved at the time of the post-treatment survey. It finds a 17% increase in property values as measured by professional appraisals, a 28% increase in homeowner-estimated property values, a 36% increase in rents, and a 72% increase in vacant land values, along with effects on a few non-housing outcomes. Our study evaluates cuts in road maintenance rather than a switch from unpaved to paved roads, and our Ohio geography contrasts with that of a developing nation. Our housing values are not based on professional appraisals or homeowner valuation but on actual sales transactions; and the fact that we observe sales transactions every year lets us analyze pre- and post-treatment trends in house prices over a long timeframe rather than a one-time change in house values.

We find almost no research on road maintenance. The exceptions include theoretical work by Rioja (2003), which develops a dynamic general equilibrium model to study the optimal amount of road maintenance, which is found to depend on the size of new infrastructure investments and the productivity of infrastructure. Rioja (2003) solves and parameterizes the model, finding that increasing funding away from new infrastructure and toward the maintenance of existing infrastructure decreases the depreciation rate of existing infrastructure, increases the stock of existing infrastructure, and increases economic output and consumption levels.

Another study, Chaurey and Le (2022), assesses the impact of infrastructure maintenance in India. As their research notes, the Indian national government identified 17 major states as the poorest and made an index of "backwardness" for districts in these states. The 115 most backward districts were awarded 450 million rupees and were given discretion on how to spend these funds to "improve and maintain" or "make complementary investments" to existing infrastructure. Money could be used to widen roads, add lanes, add electricity transmission and distribution infrastructure, build new road links to markets, and build bridges, for example. The treatment is therefore a mixture of road and non-road spending and of maintenance and new construction, especially, presumably, in districts that did not already have paved roads and electricity, where Chaurey and Le (2022) finds the largest effects. Unlike Chaurey and Le (2022), we focus on the effects of infrastructure spending on real estate values rather than employment outcomes.

The only other study we find on maintenance and house prices is the classic capitalization study by Edel and Sclar (1974). It studies the effect of local public spending on median house values in the Boston area using decennial Census years from 1930 to 1970. Controlling for latitude and longitude, the tax rate, population density, tenure status, and school expenditures, its ordinary least squares regressions do not find a statistically significant link between road maintenance spending and house prices.

Identification. Asher and Novosad (2020) 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 local road tax cuts on 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. We also find similarities with Boudot-Reddy and Butler (2024), which employs a similar identification strategy but studies new roads instead of the maintenance of existing roads and finds that villagers reward the political party responsible for spearheading the road expansion.

Another study with an identification strategy similar 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. 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) looks at the impact of bond elections for additional funding on new capital infrastructure projects, while we focus on the impact of cutting renewal referendums for existing road maintenance. Second, Cellini, Ferreira and Rothstein (2010) implements a fuzzy regression discontinuity approach, whereas ours is sharp. Lastly, we focus on the Intent-to-Treat (ITT) estimator, which analyzes the effect of an election on outcomes without controlling for the results of subsequent elections[1] (more details in Section 4). As Cellini, Ferreira and Rothstein (2010) shows, when elections are independent, the ITT estimator is equal to the Treatment on the Treated (TOT) estimator.

Road Quality. Measuring road quality remains empirically challenging. Currier, Glaeser and Kreindler (2023) exploits vertical-acceleration signals from millions of Uber trips to construct a Road Roughness Index (RRI) and shows that it moves predictably with resurfacing events. Although RRI is highly correlated with engineering benchmarks such as the International Roughness Index (IRI) and the Pavement Condition Index (PCI), each of these legacy measures has drawbacks that limit their use for large-scale economic analysis: IRI data are collected only where a costly profilometer van has recently driven, and PCI ratings depend on labour-intensive, inspector-specific visual surveys that are updated irregularly. Moreover, the Uber-based approach cannot cover streets outside the platform's service footprint—precisely where many suburban and rural housing markets are located. In contrast, high-resolution satellite imagery is available nationwide and available at low marginal cost. By fine-tuning a vision model on a labelled road-surface dataset, we generate a consistent, wall-to-wall measure of road quality that spans urban, suburban and rural networks alike, and captures the visual cues that prospective home-buyers directly observe.

We find another study, Wong et al. (2017), which examines the effect of local political

[1] Recent papers such as Hsu and Shen (2024) and Biasi, Lafortune and Schönholzer (2025) have focused on improving upon methods from Cellini, Ferreira and Rothstein (2010). However, these papers focus on the effects of new capital projects rather than maintaining existing capital infrastructure. Given the exogeneity of the timing of renewal elections and our focus on existing maintenance, the ITT estimator is more appropriate for our setting.

reform in rural Chinese villages on local infrastructure projects and finds that these reforms result in younger village leaders and higher quality roads. It measures road quality by creating a village-level road quality rating, which is based on the observations of field surveyors that use their "road quality evaluation scheme". They emphasize minimization of subjectivity in the evaluation process, as the surveyors are trained to use a standardized set of criteria to assess road quality, such as number of bends per 100 meters and road width. Unlike Wong et al. (2017), which relies on surveyor-based evaluations, our road quality measure is derived from high-resolution satellite imagery processed through our fine-tuned vision model, ensuring consistency and scalability across diverse geographic regions.

Transportation and House Prices. We highlight a substantial literature studying the effect of transportation infrastructure on house prices. Hoogendoorn et al. (2019) studies the effect of the opening of a tunnel on house prices in the Netherlands, noting that prior research on transportation infrastructure in developed regions often 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. Hoogendoorn et al. (2019) finds that half the capitalized value of the tunnel occurs more than a year before the tunnel opens and argues that the exogeneity of the tunnel's opening, along with hedonic controls, time trends, and postcode fixed effects, identifies its estimates. Our data also pertains to a developed nation. One novelty of our study is how ordinary the events are that we study. While the opening of a new tunnel is significant, it is rare. Votes to renew infrastructure spending are common events in many local governments in the United States, and the amount of road maintenance spending is regularly determined by governments worldwide, either through voting or directly by bureaucrats. It is therefore important to study the effects of road maintenance 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 represent the 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 for the development of a highway across Israel.

Contribution. Our paper contributes to the literature on three main fronts. First, 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 with pre-determined renewal period and narrowing down on close elections helps us avoid endogeneity issues coming from placement and the timing of new infrastructure. Our approach provides insights that are more relevant for policymakers in advanced economies, where road infrastructure is well established but requires continuous upkeep. Second, we compute a novel measure of road quality using a fine-tuned vision model trained on satellite images. Current literature has relied on measures based on Pavement Condition Index (PCI) or International Roughness Index (IRI) to measure road quality, but these are not available for many localities. Similarly, Currier, Glaeser and Kreindler (2023) measures road roughness using vertical acceleration data from a smartphone app, but this also requires large-scale data available mainly for large metropolitan areas and is not available for smaller towns. Our approach allows us to measure road quality at the local level, bypassing the limitations of existing measures. Third, 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 houses 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.

Roadmap. The rest of the paper is organized as follows. Section 3 provides background information and presents the variables used in the study. Section 4 outlines the empirical strategy. Section 5 presents the results of the study and shares the relevant robustness checks. Section 6 examines the mechanisms underlying our findings and Section 7 concludes.

3 Background & Data

3.1 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 two 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 is responsible for federal road infrastructure funding and allocates funds through the Federal Highway Administration (FHWA). The FHWA provides funding for the construction, maintenance, and operation of highways, bridges, and tunnels 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. Under the Bipartisan Infrastructure Law (BIL) (U.S. Department of Transportation, 2022), the HTF funds key programs like the National Highway Performance Program (NHPP), which targets the National Highway System (NHS), and the Surface Transportation Block Grant (STBG) program, which provides flexibility for a broader range of federal-aid roads. However, federal funding for local road infrastructure is largely restricted to major roads, leaving most neighborhood streets dependent on local revenue. As of 2022, about $4/5^{\text{th}}$ of all road funding came from state and local governments (Peter G. Peterson Foundation, 2024).

State Funding. The Ohio Department of Transportation (ODOT) is responsible for maintaining the Interstate system, the U.S. and State Routes. In Ohio, gas taxes, licensing fees and user fees account for 69% of the state's road funding (Boesen, 2021), which can be used for both maintenance and new construction. However, about 70% of funds goes to highway construction[2], 2% is given to local governments as grants and only 4% of state funds are directed towards roadway maintenance (Ohio Department of Transportation, 2023). The remaining funds are part of payroll and 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 property taxes, although the extent of this funding varies across different localities. 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 correspondence with Beavercreek Township, 61% of its 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 the overall local government budget is allocated to roadway maintenance (Public Service Department of Beavercreek Township, 2025). We can see that 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]According to (Ohio Department of Transportation, 2023), most of this category is used for presevering existing infrastructure rather than new capacity.

3.2 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. Our data on renewal road tax levies include 3,184 referendums and cover 617 local governments in Ohio.

The type of tax levy we study is for the renewal of road tax levies. 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 such as 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 about 90% 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 RD 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. The exogeneity of the timing of a renewal tax levy contrasts with the timing of new taxes for additional money as explained by Cellini, Ferreira and Rothstein (2010) because local governments can endogenously choose the timing of a new tax levy to coincide with positive economic conditions or positive sentiment toward government action, making an election more likely to pass.

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 tax levies for other purposes in effect, but the road tax levy that failed to renew is no longer collected. We 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 Road Tax per household, which is the tax amount that a household will pay if the levy is successfully renewed. Average Road Tax per household is computed following Equation 1:

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

where i is the city, t is the year, and millage-rate [3] is the predetermined millage amount set for the road tax levy. The Average Assessed value is 35% of the average appraisal value[4], which for our study equals the average sale price of houses in the city for that year. From this, we can also compute the average road tax per city by multiplying the average road tax per household with the number of households in the city.

Although we acknowledge a large variation in the appraisal value across cities and in millage rates across referendums, we find that the average road tax per household is \$76, and the average road tax per city is \$167,011 as shown in Table 1. We do not observe any significant difference in the average road tax per household between cities that renew and cut road tax levies, which suggests the levies up for renewal are not systematically different between areas that renew and areas that fail to renew them. Whenever a renewal tax levy fails, the local government loses the funding from that tax levy, which is the average road tax per city. This loss in funding directly impacts local government's spending budget needed to maintain local roads in Ohio and accounts for 11% of a local government's budget for road maintenance [5].

[3] Millage-rate is property tax rate expressed in mills (tax per \$1,000 of assessed value).

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

[5] For townships within our effective bandwidth, we use the average expenditure by the public works department as our base, which is \$1,528,404 as per their audited financial reports. All local roads in such municipalities are funded via this department. Average loss in maintenance spending for areas that cut their renewal levy is \$163,547, thus giving us $11\% = \frac{163{,}547}{1{,}528{,}404} \times 100$

Table 1: Spending impact of failing to renew a Road Tax Levy

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

Notes: This table presents descriptive statistics for two measures of the money collected via road tax levies. **Panel A** reports the mean and standard deviation (SD) of the road tax per household per year. **Panel B** reports the mean and SD of the total road tax collected per city. "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.

3.3 Running Variable

The running variable plays a critical role in RD, which in this study represents the proportion of votes against the renewal of a road tax levy. A vote share of 50% or more against the renewal road tax levy means the levy fails and the tax will no longer be collected, resulting in a stoppage of road funding via that particular tax levy. There are 3,184 referendum results in our sample, 83% of which renew the tax, and 17% of which cut taxes and road maintenance. 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 assumption allows us 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 C).

Density test. A classic RD 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 1 that graphically illustrates the lack of abrupt change in density.

Although Table 3 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 that suggest covariate smoothness around the cutoff are found in Appendix C. A formal way to assess covariate smoothness is to use each covariate as an outcome variable in a regression of the running variable and the treatment effect dummy. When we do so, the p-value of the treatment effect dummy varies from 0.234 to 0.981, as shown in Table 13, indicating no statistically significant jump in covariate values.

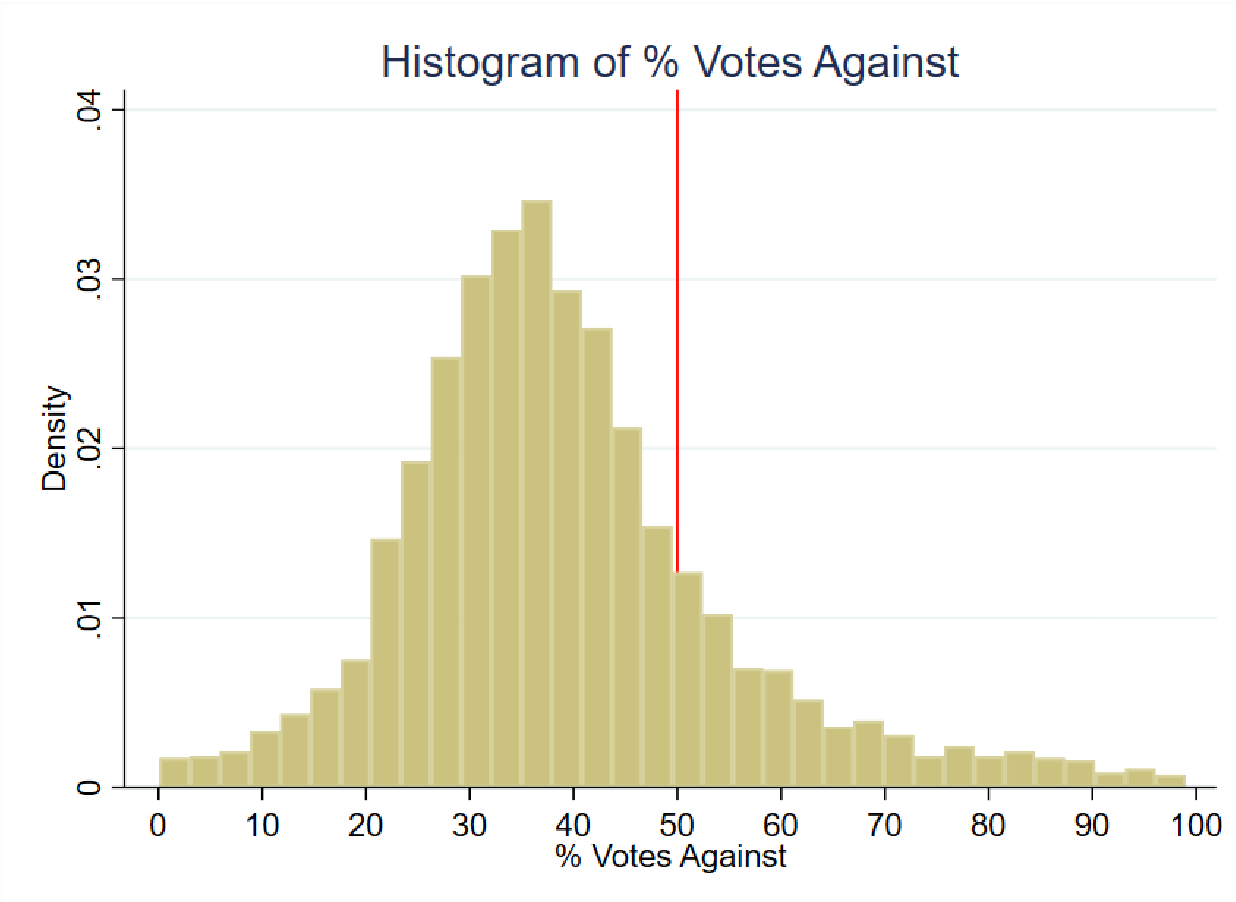


Figure 1: Histogram of Running Variable

3.4 Tax Cuts & Evidence of Road Quality

Roads in Ohio last 15-20 years and deteriorate faster with poor drainage, utility cuts to the road, and snowplow damage (City of Hudson, 2020). To understand the effect of cutting local road taxes on road quality, we fine-tune an AI vision model on satellite imagery data from Brewer et al. (2021). Following the seminal work on text-based Transformers in Natural Language Processing (NLP) by Vaswani (2017), vision transformers (ViTs) were introduced by Google Brain's team and are part of a recent class of deep learning models that have shown to outperform classic Convolutional Neural Networks (CNNs) on image classification tasks (Dosovitskiy, 2020). Nevertheless, a more recent class of vision models learns from ViTs and uses CNNs to achieve high accuracy on image classification tasks (Woo et al., 2023). We use one such model to classify road quality from satellite images[6]. Below, we outline the steps we take to assess road quality.

Satellite Images for fine-tuning. Fine-tuning a vision model involves taking a pre-trained model and adapting it for a specific image-classification task. In order to ensure appropriate fine-tuning and enable our vision model to accurately predict road quality, we need large-scale road-image data. We use the road-image dataset of Brewer et al. (2021) which consists of 53,677 labeled satellite images of roads in different conditions, and a classification representing quality of each road: 0 (poor), 1 (decent) and 2 (high). We divide the data into training, validation and test datasets, and use 70% of the data for training, 15% for validation and 15% for testing. For a detailed analysis of the road-image dataset, see the Appendix, Section E. In section 5.1, we present the results from our fine-tuned model, when applied to roads in areas with close elections within the effective bandwidth.

Ohio Satellite Images. While the fine-tuning of our vision image-classification model uses images from Brewer et al. (2021), our testing sample uses hand-collected satellite images from Google Earth Pro for roads in different cities in Ohio. Even though Google Earth Pro covers all of Ohio and provides satellite images of roads for cities we study, these images may differ in resolution and quality depending on the location. The images are also not always available for all time periods, and the time period of the images may not be consistent across different locations[7]. For our analysis, it is imperative to ensure we have images before and after the referendum for both the treatment and control groups. However, we also found that some areas did not have images available for both periods, or the images were of poor quality. We restrict our sample to include only those areas where we could obtain high-resolution images for both periods[8]. We collect road images for cities with votes within

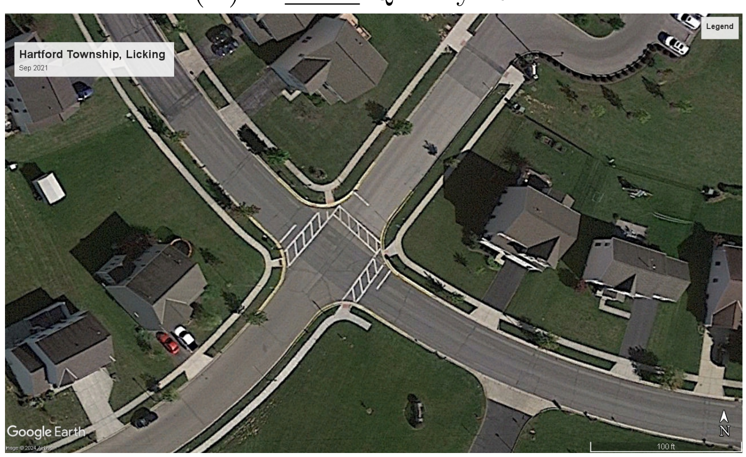
[6]see the Appendix, Section E for details.

[7]For example, Jersey Township in Licking County, Ohio had a road tax levy **renewal** in 2019, but the most temporally adjacent satellite images available are from 2017 and 2023. Marion Township in Hocking County had a road tax levy **cut** in 2018, and the proximate satellite images available are from 2014 and 2021.

[8]On average, pre-referendum images are from 2.3 years before the election, and post-referendum images are from 3.3 years after the election.

the average effective RD bandwidth provided in Table 5 and ensure that we have pre- and post-referendum images for both- the treatment and control groups[9]. Figure 2 presents two examples of road satellite images for Ohio, one from a road classified as high and the other as poor quality.

(A) A **Poor** Quality Road



(B) A **High** Quality Road

Figure 2: Road Quality Satellite images for Ohio

Road Quality Rating. Our fine-tuned vision model classifies each satellite image into one of three categories: 0 (poor), 1 (average), or 2 (high). This rating is based on visual cues such as pavement quality, visible cracks, patchwork, and overall surface condition, as learned by the vision model from the labeled training data. For each image, the model outputs

[9]Other papers such as Gonzalez-Navarro and Quintana-Domeque (2016) also have one pre- and post-intervention year, collected via surveys, rather than a comprehensive panel.

both the predicted class and a confidence score, which reflects the model's certainty in its prediction. These ratings provide a standardized, objective measure of road quality across locations and time periods, enabling us to compare changes in road conditions before and after tax levy referendums for both treated and control groups.

Road Quality Score. Even though our original model output is a road quality rating[10], we also convert the discrete model output to a continuous measure of road quality between 1 and 100. This conversion uses two variables: the predicted road quality rating and confidence score[11], and allows us to map our road quality rating to a number similar to Pavement Condition Rating (PCR) used by ODOT, and call this Road Quality Score (RQS). The formula for this conversion is as follows:

$$\mathrm{RQS} = 1 + [(\hat{r} + \tau \cdot \hat{c}) \cdot w \cdot \mathbb{I}\{\hat{r} \neq 0\} + (1 - \tau) \cdot \hat{c} \cdot w \cdot \mathbb{I}\{\hat{r} = 0\}] \tag{2}$$

where $\hat{r}$ is the predicted road quality rating, $\hat{c}$ is the confidence score, w is a relative weight for allotted size of each road quality rating group, 0, 1, and 2. τ is a parameter for confidence weights[12]. The RQS ranges from 1 to 100, where 1 indicates poor quality and 100 indicates high quality. We use this RQS in our analysis to assess the impact of cutting local road taxes on road quality.

Next, we share a summary of means and standard deviations of the predicted road quality rating and road quality score for the treatment and control groups, before and after the referendum.

[10]This is a direct result of our training data from Brewer et al. (2021) which uses the discrete 0 (low), 1 (medium), 2 (high) road-quality metric.

[11]During the prediction process, we asked the fine-tuned vision model its confidence in the prediction i.e. the probability of the prediction being correct, a number between 0 and 1.

[12]We set this equal to 0.4.

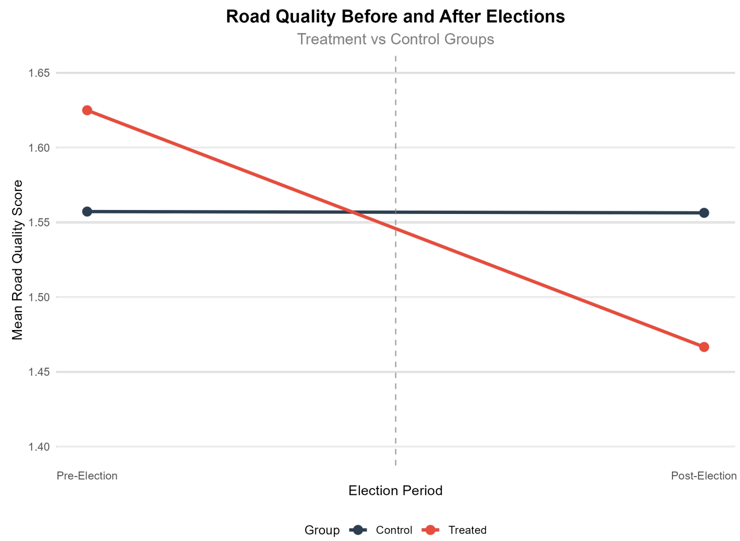
Table 2: Predicted Road Quality by Treatment Status and Period

	Treated (Failed Levy)	**Control (Renewed Levy)**
Panel A: Road Quality Rating (0, 1, 2)		
Pre-Election	1.62	1.56
	(0.490)	(0.499)
Post-Election	1.47	1.56
	(0.505)	(0.499)
Panel B: Road Quality Score (1–100)		
Pre-Election	74	71
	(17.3)	(17.9)
Post-Election	68	70
	(18.0)	(17.2)

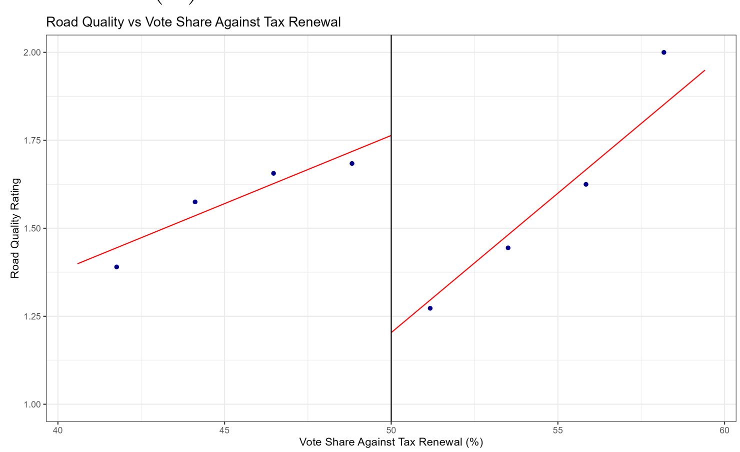
Notes: The control group comprises townships within the effective RD bandwidth that successfully renewed their road tax levies; the treated group comprises those that failed to renew. Ratings are derived from our fine-tuned vision model. Scores follow Equation 2. Standard deviations are in parentheses.

Panel A of Table 2 summarizes the Road Quality Rating (RQR), showing that treated areas i.e. jurisdictions that cut their renewal road tax levies, started with a slightly higher average RQR (1.62) than control areas (1.56) before the referendum, but experienced a notable decline from 1.62 to 1.47 after the referendum, while control areas saw no change and remained at 1.56.

Panel B of Table 2 presents the continuous Road Quality Score (RQS), where treated areas also began with a higher average RQS (74) compared to control areas (71) before the referendum. After the referendum, RQS dropped notably from 74 to 68 in treated cities, but only slightly from 71 to 70 in control cities. This pattern suggests that failing to renew a road tax levy leads to a decline in both RQR and RQS, while renewing the levy is associated with fairly stable road quality.



(A) Before and after referendum



(B) Road Quality vs Vote Share Against Renewal (%)

Figure 3: Difference in Road Quality Rating by Treatment Status and Vote Share

Figure 3 Panel A graphically illustrates the difference in road quality rating before and after the referendum for both treated and control groups. The graph shows a clear decline in road quality rating for the treated group after the referendum, while the control group remains relatively stable. Panel B of Figure 3 shows road quality rating five years after the referendum graphed alongside the percent of votes against the tax levy. The points represent the local average road quality rating for the representative bins of vote shares within the average effective RD bandwidth around the cutoff of 50%. The graph suggests a discontinuity in road quality rating at the cutoff, which is the basis of our identification strategy, suggesting that the failure to renew a road tax levy has a negative impact on road quality. Section 5.1 provides a more detailed analysis of the impact of cutting road taxes on road quality, using the predictions from the model as the outcome variable, and employs regression discontinuity to estimate the effect of cutting road maintenance taxes on road quality.

3.5 Outcome Variable: Median House Price

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 votes considered in this study is $166,082 in constant 2010 dollars with a standard deviation of $372,135 which suggests the presence of outliers. Although our use of median sale price addresses outliers, one of our robustness checks drops 1% tails and re-estimates the treatment effects (Figure 9).

Figure 4 shows house prices from five years after the vote graphed alongside the percent of votes against the tax levy[13]. 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 graph suggests a discontinuity in house prices at the cutoff, which is the basis of our identification strategy, suggesting that the failure to renew a road tax levy has a negative impact on house prices.

[13]after deflating to 2010 U.S. dollars

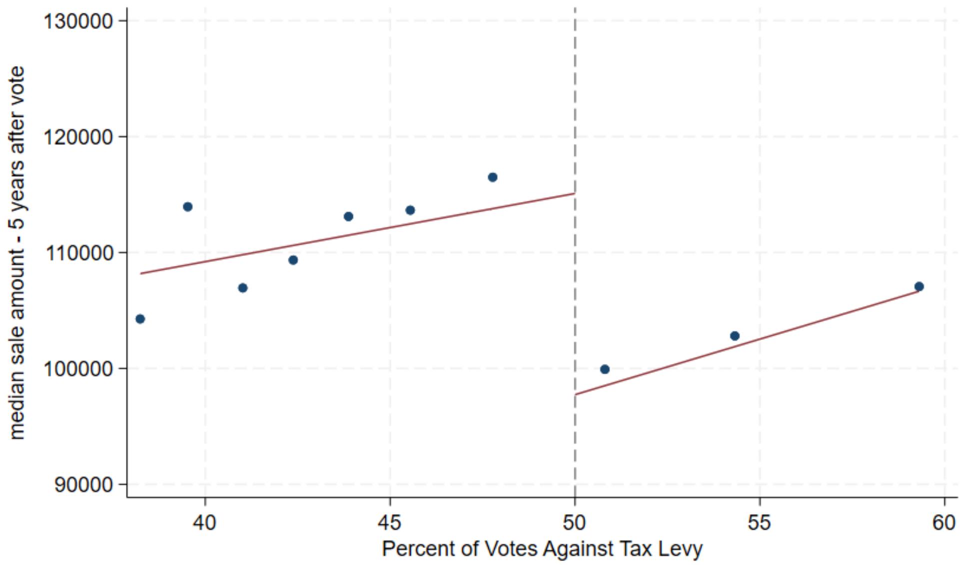


Figure 4: Median Sale Price of Houses: 5 years after vote

3.6 Covariates

Covariates can be useful in RD 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 RD says they should be. Table 3 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 3 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 columns that are critical for the credibility of the RD design.

The data, captured at the time of the vote and as observed in Table 3, shows minimal differences within the effective bandwidth: 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 points or less, bolstering the comparability of the treatment and control groups. This similarity suggests that any observed differences in outcomes can more confidently be attributed to the treatment effect rather than to pre-existing differences[14].

Table 3: Variable Means & Standard Deviation by Road 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)
% Renters	0.20	0.20	0.20	0.19	0.20
	(0.11)	(0.10)	(0.11)	(0.09)	(0.11)
Unemployment Rate	0.05	0.05	0.05	0.05	0.06
	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)
% White	0.96	0.97	0.96	0.97	0.97
	(0.07)	(0.07)	(0.07)	(0.07)	(0.08)

Continued on next page

[14]We also find similar covariate means for cities that have no road tax levies, which we explain in Section 5.4

Table 3 – continued from previous page

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

4 Empirical Strategy

In this section, we describe our empirical strategy to estimate the causal effect of cutting road maintenance spending on house prices. One key feature of our quasi-experimental design is the exogeneity of the timing of the election. The timing is determined by the natural expiration of a 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.

4.1 Regression Discontinuity in Panel Data Setting

Suppose that local government in area i and year t conducts a referendum to renew an existing road tax levy. Let v_{it} be the vote share against the renewal tax levy and v^* be the threshold determining the result of the referendum (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 the outcome variable median housing sale price. We can write Equation 3 as follows:

$$y_{it} = \alpha + \theta F_{it} + \epsilon_{it} \tag{3}$$

where α is the intercept, θ is the parameter of interest representing the causal effect of cutting a renewal road tax levy and ϵ_{it} is the error term representing all other determinants of the outcome. Around a narrow enough window around the threshold v^*, we can estimate the causal effect of cutting a renewal road tax levy on the outcome variable y_{it} by comparing the outcome variable for cities that narrowly pass the referendum to those that narrowly fail it.

4.2 Intent-to-Treat (ITT) Estimator

We follow a model of RD design similar to Cellini, Ferreira and Rothstein (2010) and estimate the **Intent-to-Treat** or ITT estimator. We prefer using the 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 Equation 4:

$$Y_{i,t+\tau} = \alpha_\tau + \kappa_t + F_{it}\theta_\tau^{ITT} + P_g(v_{it}, \gamma_\tau) + Z_{it}\beta_\tau + \epsilon_{i,t+\tau} \tag{4}$$

Equation 4 shows a city i that holds an election in year t and we study this city's outcome τ years later. $Y_{i,t+\tau}$ represents the outcome variable for city i at year $t+\tau$. 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}. 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. Z_{it} is a vector of control variables that include city-level demographics, economic conditions, and other relevant covariates. $\epsilon_{i,t+\tau}$ is the error term.

We use the bandwidth selection method of 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 the size of the neighborhood around the cutoff v^*, defined as $(v^* - h, v^* + h)$. Only observations within this neighborhood are used to compute the bias-corrected treatment effect estimate $\hat{\tau}$. For a sufficiently small neighborhood, the continuity assumption central to the RD estimator is considered valid. We also cluster the standard errors by city to account for any serial correlation between years within each city. The weighting scheme k determines the weights of the observations within the neighborhood $(v^* - h, v^* + h)$ and is crucial in estimating θ_τ. Common weighting schemes include 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 the 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 those within the effective bandwidth h are used to identify our treatment effect estimates θ_τ for different τ years.

5 Results

5.1 Road Quality Decline

First, we present the results of our analysis on the impact of cutting road maintenance taxes on road quality. As discussed in Section 3.4, we use a fine-tuned AI vision model to assess road quality based on satellite imagery data. The model gives us a road quality rating of 0 (poor), 1 (medium), or 2 (high), which we also convert to a continuous variable called Road Quality Score (RQS) ranging from 1 to 100.

Table 4: Change in Road Quality after an Election

	(1)	(2)
RD Estimate	-0.477**	-15.634**
	(0.222)	(7.366)
Covariates	✓	✓
Clustered SEs	✓	✓
Effective Sample	178	178

Notes: Column (1) shows the RD estimate of the effect of cutting road maintenance taxes on the Road Quality Rating (RQR, 0–2), and Column (2) shows this effect on the continuous Road Quality Score (RQS, 1–100). Standard errors are in parentheses. The table reports covariate-adjusted sharp RD estimates from a polynomial regression ($p = 1$, $q = 2$) at the 50% vote-share cutoff. The bandwidth is mean effective RD bandwidth from Table 5. Inference is based on bias-corrected point estimates with robust standard errors. Standard errors are clustered by ten-digit FIPS code. Significance levels: *p $<$ 0.10, **p $<$ 0.05, ***p $<$ 0.01.

Table 4 reports RD estimates of the discontinuity in post-election road quality at the 50% threshold. Column (1) uses the categorical Road Quality Rating (RQR, 0–2), and Column (2) uses the continuous Road Quality Score (RQS, 1–100), both derived from the fine-tuned vision model described in Section 3.4. As shown in Table 4, we find that cutting road maintenance taxes does lead to a decline in road quality of -0.477 for the RQR metric.

This suggests that relative to similar cities that renew their road maintenance taxes, cities that cut their road maintenance taxes experience a decline of almost half a category in road quality after the election. The estimate for the RQS metric is -15.634, indicating a decline of about 15.6 points on a 100-point scale. Using the pre-election means among treated cities of 1.625 (RQR) and 74 (RQS), these estimates of difference in road quality correspond to approximately 29.4% and 21.2% reduction in road quality. The limited sample size is an artifact of our focus on close elections, limited availability of high-resolution satellite imagery for some cities and years, and the cleaning process to ensure that the road segments analyzed are representative of public roads. Although these results may be sensitive to data availability, sample size and the performance of the vision model, they provide suggestive evidence that cutting road maintenance taxes leads to a significant decline in road quality. The next section presents our main results on the impact of cutting road maintenance taxes on house prices.

5.2 House Prices Decline

Table 5 below shows the ITT estimates of failing a road tax levy on housing sale prices. Each treatment effect estimate represents the discount in median sale price for cities that cut road taxes relative to otherwise similar cities that renew the taxes. We show dynamic effects starting three years before the vote and extending to ten years after the vote. Treatment effect estimates for years 4 through 9 after the vote are statistically significant in Table 5. 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 house value[15]. The 9% reduction we estimate may be compared to Gonzalez-Navarro and Quintana-Domeque (2016) which observes a change in property values of 17-28% after

[15]The average treatment effect estimate of $15,349 was divided by the mean house sale price in the dataset of $166,000 to get 9%.

paving previously unpaved roads and the 13% increase in house prices Theisen and Emblem (2021) finds in towns nearest to a newly-constructed highway[16].

Table 5: Effect on median house prices of failing to renew a road tax levy

Panel A: Years $t-3$ through $t+3$							
Year	$t-3$	$t-2$	$t-1$	t	$t+1$	$t+2$	$t+3$
Treatment effect	5,307	166	–273	–4,261	–3,908	–11,001	–14,733
Standard error	(7,341)	(6,943)	(7,391)	(7,955)	(8,719)	(9,405)	(7,989)
Effective bandwidth (h)	10.30	10.06	8.76	7.97	9.78	9.63	11.93
Bias bandwidth (b)	20.22	17.48	14.39	13.03	17.50	18.22	22.69
Effective observations	724	725	650	587	774	778	1,003
Total observations	2,552	2,631	2,696	2,784	2,764	2,699	2,640
Panel B: Years $t+4$ through $t+10$							
Year	$t+4$	$t+5$	$t+6$	$t+7$	$t+8$	$t+9$	$t+10$
Treatment effect	–21,701	–21,706	–17,365	–15,975	–21,984	–19,857	–16,090
Standard error	(7,747)	(8,751)	(8,355)	(7,248)	(9,074)	(7,751)	(9,027)
Effective bandwidth (h)	8.52	11.20	9.79	13.16	8.25	7.28	6.24
Bias bandwidth (b)	16.25	20.16	17.32	23.42	15.28	14.09	16.27
Effective observations	688	922	764	1,061	591	505	402
Total observations	2,614	2,531	2,438	2,324	2,199	2,115	2,017

Notes: Outcome is median house price in constant 2010 U.S. dollars. Unit of observation is the city–year, so a treatment effect of –$21,701 in year $t+4$ means that four years after the vote, cities that fail to renew road taxes have median sale prices $21,701 lower than similar cities that renew. Covariates include the demographic and socioeconomic controls listed in Table 3.

[16]Note that these studies focus on the effect of developing paved roads whereas we focus on deterioration of existing paved roads.

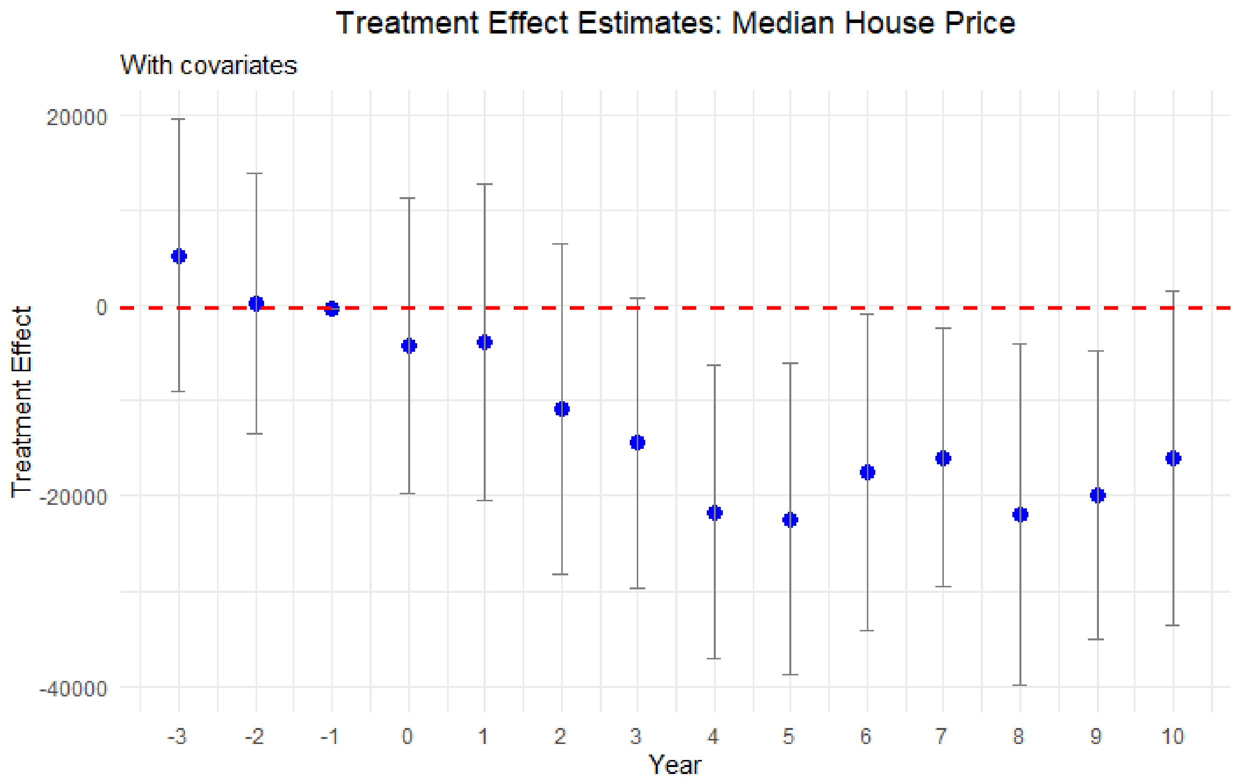


Figure 5: Effect plot for Median Housing Price

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 show that the median housing prices are statistically identical for cities above and below the threshold prior to the treatment. Each dot represents the treatment effect point 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 5, 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.

5.3 Heterogeneity Analysis

We show the results of our heterogeneity analysis, where we explore the differential impact of cutting road maintenance spending on house prices in urban and rural neighborhoods, for tax levies of different sizes, and for different housing price quantiles.

Urban vs Rural neighborhoods: Analyzing regional heterogeneity is important because the impact of public goods like road maintenance on house prices can differ substantially between areas due to factors such as differences in elasticity of housing supply. In this section, we check how treatment effects differ between urban and rural areas[17].

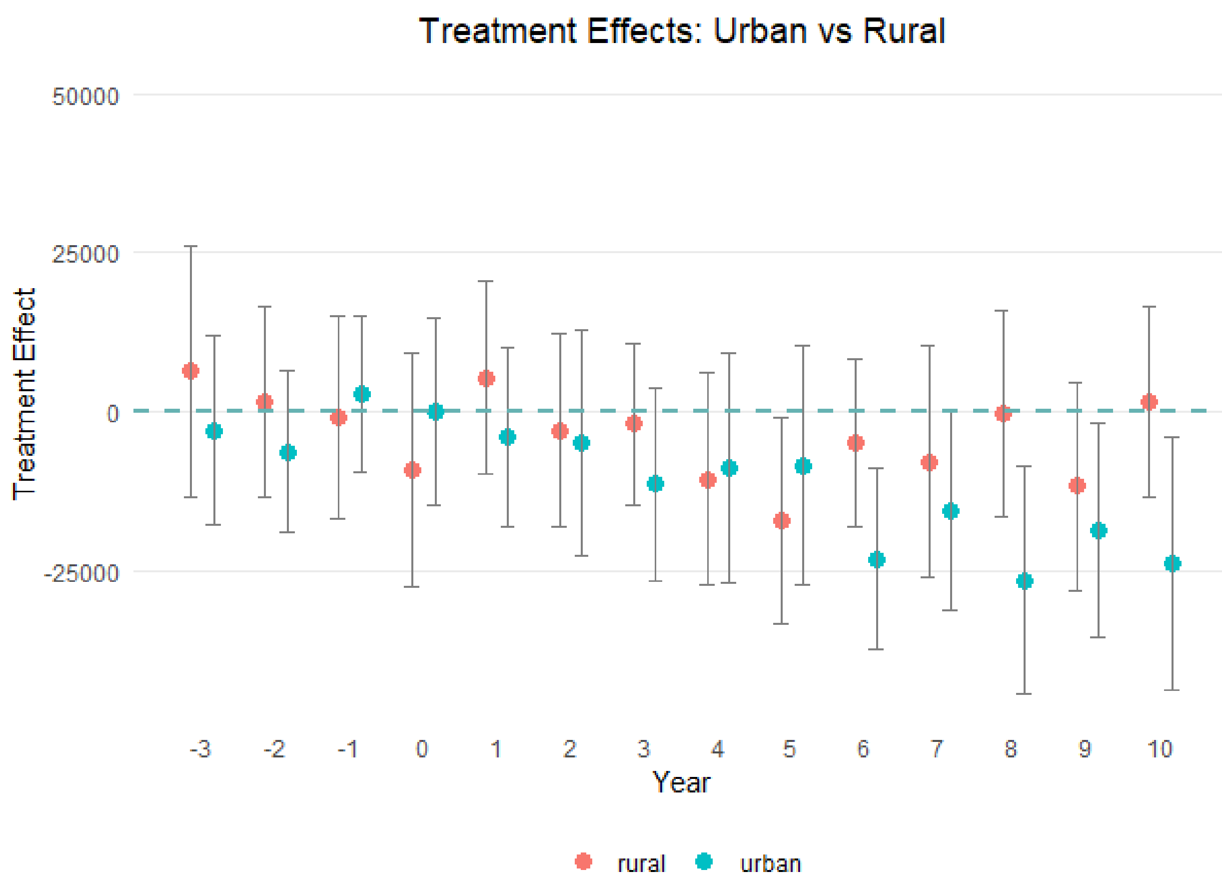


Figure 6: Median Housing Price in Urban and Rural Areas

[17]Urban areas are identified using data from U.S Census Bureau. We strictly consider urban areas with atleast 0.3 allocation factor in 2010 and do not consider urbanized clusters, making our classification of urban areas more restrictive.

As shown by Figure 6, we do not find any significant differences in housing prices in rural areas after a renewal tax levy fails to pass. 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 stronger effects in urban than rural areas may stem from a more inelastic housing supply (Brasington, 2002). Overall, we find that housing prices decrease by $13,302 on average over the decade[18] after cutting road maintenance tax levies in urban areas.

Tax magnitude: We check for dose-response by splitting the sample by the size of the tax levy. First, we consider splitting the sample based on the median size of the tax levies. The median size of the tax levies in our sample is 1.9 mills, however we find that the tax levies above and below the median size do not show any significant differences in treatment effects. Next, we focus on the top quartile of the tax levies, which in our sample is the set of tax levies ranging from 2.1 to 8 mills. Figure 7 below presents the results for this subsample. The treatment effect estimates for these larger tax cuts are both statistically significant and of greater magnitude, indicating a more pronounced decline in house prices when the reduction in road tax levies is more substantial.

Despite the reduction in sample size leading to wider standard errors in Figure 7, the overall pattern in decline of house price remains, with mean treatment effect for this subsample being approximately $30,000 which is about double the effect observed in the full sample. This finding supports the presence of an intensive margin effect: the larger the tax cut, the greater the negative impact on housing prices. Such a pattern is consistent with a dose-response relationship, where the magnitude of the fiscal shock translates directly into the size of the decline in property values.

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

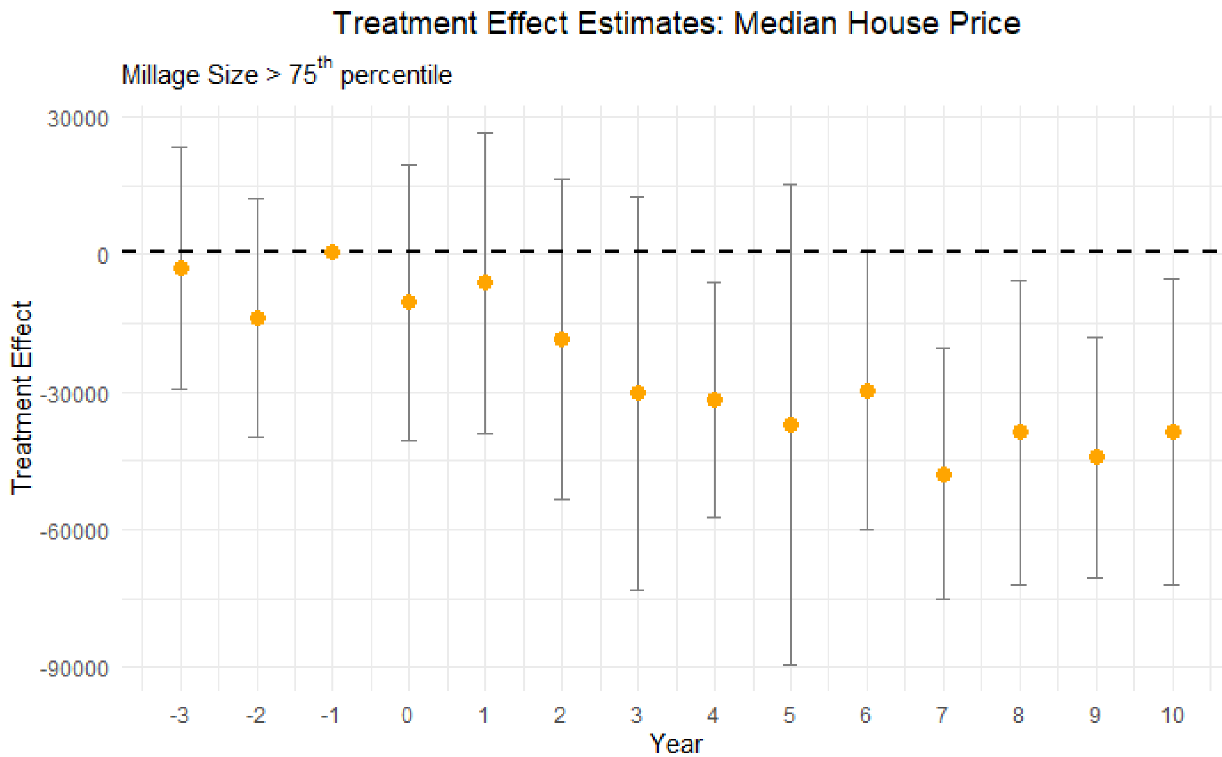


Figure 7: Effect of Large Road Maintenance Tax Cuts (>2 mills) on Median House Price

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

Table 6 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 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 house sale prices in an effect plot to highlight this differential impact of reduction in road maintenance spending.

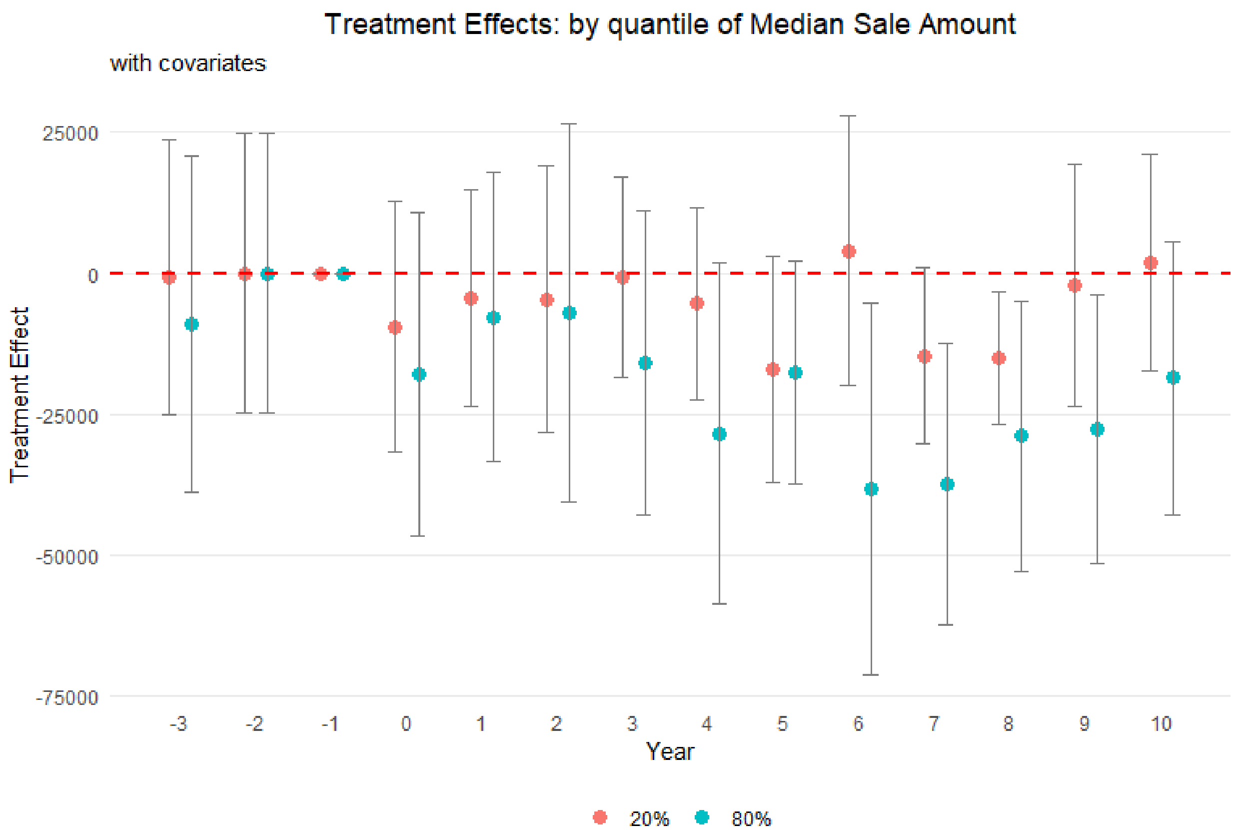


Figure 8: Treatment Effect of Cutting Road Maintenance Taxes for 20^{th} and 80^{th} Percentiles of Median House Price

Table 6: 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)

Notes: 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 the associated spending have houses that sell for $28,478 less than cities that vote to renew them. Covariates include demographic and socioeconomic characteristics as described in Table 3.

5.4 Robustness Tests

We conduct several robustness tests to ensure the validity of our results. We test the sensitivity of our treatment effect estimates by removing contaminated observations and winsorizing the outcome variable to reduce the influence of outliers. Lastly, we consider other tax levies to alleviate endogeneity concerns.

Removing contaminated observations: In this test, we focus on independence and exogeneity. Estimates may be biased if tax levies for additional money 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, as these votes may contaminate the treatment effect of the renewal tax levy vote. For example, consider a scenario where a city votes to fail its renewal tax levy in the year 2000 i.e. funding for road maintenance is cut. 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 elections in the year 2000.

Table 7: Effect on median house prices of failing to renew a road tax levy – uncontaminated observations only

Panel A: Years $t-3$ through $t+3$							
Year relative to vote	$t-3$	$t-2$	$t-1$	t	$t+1$	$t+2$	$t+3$
Treatment effect	5,295	–369	1,207	–2,482	–3,107	–12,236	–15,026
Standard error	(7,255)	(7,359)	(7,776)	(8,482)	(9,270)	(10,123)	(8,746)
Effective bandwidth (h)	10.25	9.70	8.84	7.48	9.19	8.20	10.09
Bias bandwidth (b)	20.08	16.41	14.45	12.03	16.04	16.33	20.21
Effective observations	719	682	630	510	680	604	763
Total observations	2,552	2,574	2,593	2,640	2,627	2,536	2,449
Panel B: Years $t+4$ through $t+10$							
Year relative to vote	$t+4$	$t+5$	$t+6$	$t+7$	$t+8$	$t+9$	$t+10$
Treatment effect	–21,082	–16,827	–17,565	–18,179	–20,427	–29,970	–23,593
Standard error	(9,145)	(11,228)	(10,128)	(9,202)	(9,829)	(9,059)	(10,838)
Effective bandwidth (h)	6.85	5.64	8.48	9.34	7.41	5.51	6.20
Bias bandwidth (b)	13.19	14.82	14.53	18.06	14.23	11.74	16.54
Effective observations	473	367	573	609	442	303	322
Total observations	2,389	2,274	2,145	2,016	1,890	1,787	1,666

Notes: Sample excludes any potentially contaminated city–year observations. Outcome is median house price in constant 2010 U.S. dollars. Each coefficient is the difference between treated and control cities in the indicated year; standard errors are heteroskedasticity-robust. All regressions control for the demographic and socioeconomic covariates listed in Table 3.

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 consistent with our initial findings. This consistency in treatment effect, despite the exclusion of potentially confounding data, lends credence to our results. The standard errors increase slightly due to the 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 8 below summarizes the results from the placebo cutoffs analysis.

Table 8: 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)

Notes: Robust treatment effect estimates at placebo cutoffs using the estimator from Calonico et al. (2017). The unit of observation is the city-year. Standard errors are reported in parentheses below each estimate.

Table 8 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 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 (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 $150,505 in constant 2010 dollars with a standard deviation of $115,646. 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:

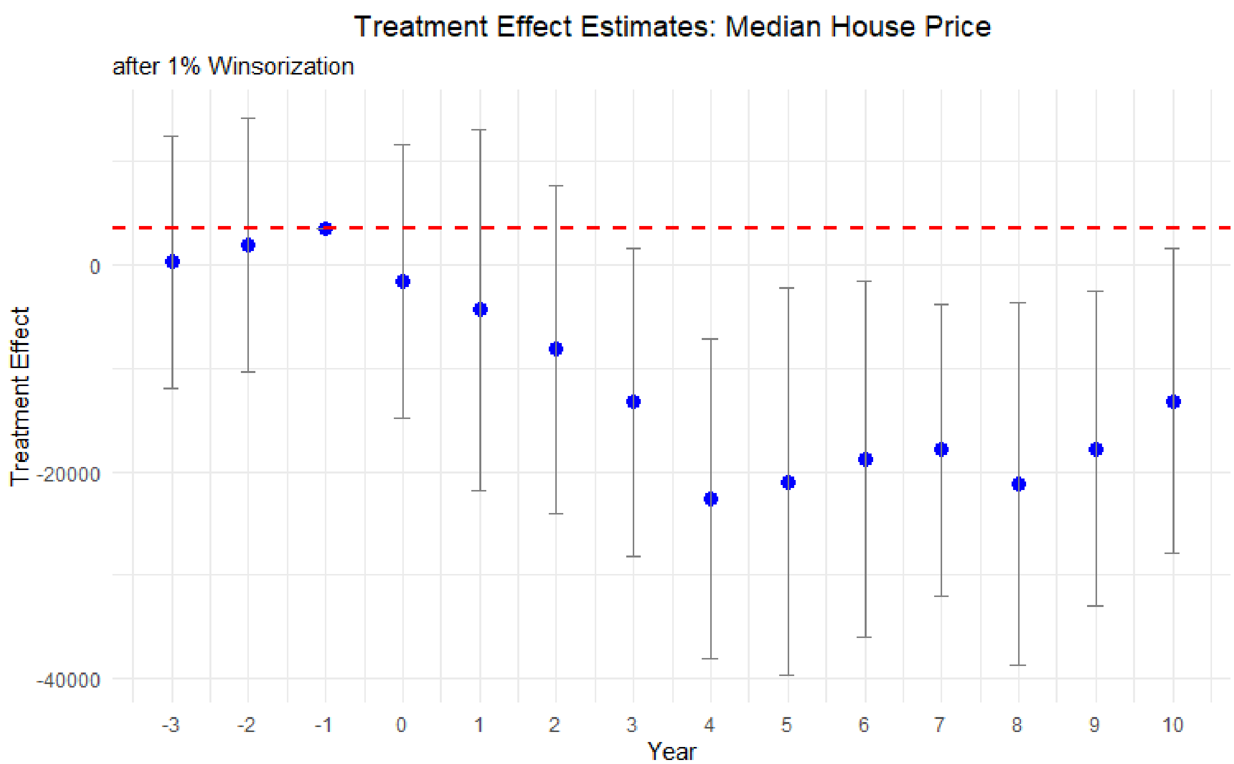


Figure 9: Effect of Cutting Road Maintenance Taxes on Median Housing Price after 1% Winsorization

The treatment effect estimates with winsorization mimic those from our baseline regression results qualitatively and quantitatively.

Other Tax Levies: A potential threat to identification is that a cut in road tax levy might be correlated with cuts in other local tax levies, or may simply reveal an underlying, time-varying taste for lower taxes or smaller government. If the same electorate simultaneously rejects (or approves) levies for police, fire, recreation, or schools, our estimated road-maintenance effect could in fact be picking up broader changes in local public-service bundles that are themselves capitalised into housing prices. To address this concern, we examine whether the election results of a renewal road levy is systematically correlated with the results of other levy referenda held on the same ballot or within the same fiscal year i.e. whenever a road tax levy is cut, are there elections and subsequent tax cuts for other tax levies as well?

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

	Police	Fire	Recreational	School
Estimate	0.248	0.053	0.015	-0.024
	(0.153)	(0.043)	(0.317)	(0.092)

Notes: This table presents coefficients from regressions associating road tax levy referendum outcomes with outcomes for other types of levies (police, fire, recreational, school). Standard errors are reported in parentheses. 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 9 reports coefficients from separate regressions in which each column regresses the pass/fail outcome of another levy type on the pass/fail outcome of road tax levy[19], controlling for year fixed effects, jurisdiction fixed effects, and demographic covariates. For school levies we cluster to the county level as an approximation, while all other specifications are at the township/city ("county subdivision") level. The point estimates are small and statistically indistinguishable from zero: a failed road-maintenance renewal is neither more nor less likely to coincide with the failure of levies earmarked for police, fire, recreation, or schools. For example, the largest coefficient, 0.248 for police levies, has a standard error of 0.153 and is not significant even at the 10 percent level. These results suggest that voters treat the road-maintenance question separately from other local tax questions, and that decisions to cut

[19] This is represented by a dummy variable indicating a cut in road tax levies, with 1 as denoted as a cut and 0 denoted as a renewal.

or renew road funding are not simply proxies for a general anti-tax or austerity sentiment. Consequently, our main RD estimates are unlikely to be confounded by contemporaneous changes in other public services.

A more comprehensive approach to address this concern is to check for balance in other tax levies around the cutoff. As the theory of regression discontinuity suggests, cities close to the 50% vote cutoff are as good as randomized along the cutoff. If true, there should be no difference in funding changes between the pass-levy and fail-levy groups. To confirm this, we collect all 50,000 local tax levies for all municipal purposes from 1991 to 2021, including current expense tax levies, parks and recreation, and police funding, for example. We identify the tax levies for the cities within the effective bandwidth and compare the proportion of cities that renew and fail to renew road tax levies that have other tax levies on the ballot for different purposes at the same time as the road tax levy. The pass-levy group has a non-road tax levy on the ballot 48.1% of the time, and similarly the fail-levy group has a non-road tax levy on the ballot 47.96% of the time. The two groups are balanced, as suggested by RD theory. We next focus on the set of cities within our effective bandwidth with non-road tax levies on the ballot. The cities that barely vote to renew road taxes experience a change in other taxes, either an increase in new spending or a cut in existing spending, 12% of the time. The corresponding number for the set of cities that barely votes to cut road taxes is 15%. Again, randomization near the cutoff suggests that both observable and unobservable characteristics are balanced between pass and fail road tax levy groups. Thus, the difference in house prices can be attributed to tax cuts in road maintenance funding and decreases in road quality.

6 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 Oates (1972) and Edel and Sclar (1974). We develop a theoretical framework using Dynamic General Equilibrium (DGE) model to explain the mechanisms behind our empirical findings. The model captures the dynamic interplay between local government budget constraints, road quality, and housing prices. Specifically, cutting road-maintenance tax levies reduces local government funds for maintaining infrastructure, which in turn constrains the government's ability to provide road-upkeep services. These lower levels of maintenance funding result in a deterioration of road quality, which then directly and indirectly lowers housing values. For brevity, we summarize the key insights here and relegate the full model description to Appendix D. Below, we outline the three-step channel through which road tax cuts lead to lower house prices.

1. 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 infer the size of the drop in maintenance funds when a maintenance levy fails to renew in Table 1.

2. Decline in Road Quality. Once the road tax levy is not renewed and the budget is cut, the local government has fewer resources to maintain roads, but does it reflect in the quality of roads? We study this question and explain our method to measure road quality in Section 3.4. Results from our AI model-based road-quality measures in Section 5.1 show a marked decrease in road quality following the cut of renewal road tax levies. The resulting deterioration in road quality is primarily seen through remote sensing data, but may be experienced via 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 reduces the quality of a local public good. Residents observe these changes slowly over time, often after four to five years, when road conditions become visibly poor.

3. Capitalization into House Prices. Falling road quality imposes a disamenity on local residents via reduced local infrastructure that is capitalized into house values (Oates, 1969). A couple of mechanisms help explain the ultimate discount in property values:

- **Appearance and Neighborhood Appeal.** Rough roads, potholes, and poorly patched surfaces reduce aesthetic appeal of the houses in a neighborhood. Potential buyers, seeing visible signs of neglect, bid less, thereby reducing the sale price of houses.

- **Travel Costs.** Lower quality roads and chronic maintenance issues translate into slower commute times for residents, particularly in urban areas. This is a direct disutility for homeowners, as time lost in traffic takes time away from leisure or work.

Short-Run versus Long-Run Trade-Off. Importantly, a trade-off emerges over time. In the short run, residents in cities that voted against a road tax levy experience immediate relief in terms of lower property tax bills[20]. 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 Table 5), 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 evident, the city's real-estate market has had sufficient time to register the disamenity, capitalizing it as a discount in property values. This dynamic

[20] Table 1 reflects the relief in property tax payments for households when a road tax levy is not renewed.

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).

7 Conclusion

Most studies look at the impact of new infrastructure on housing values in developing nations (Asher and Novosad, 2020; Huang, 2016; Li et al., 2016). Recent studies recognize the difficulties in providing unbiased estimates and account for the fact that the decision on where to place the new capital infrastructure is an endogenous decision and may be correlated with other factors (Biasi, Lafortune and Schönholzer, 2025; Donaldson and Hornbeck, 2016). We avoid this identification issue by studying maintenance spending on existing infrastructure. This focus is particularly important in developed economies, where the construction of new infrastructure is infrequent compared to the large stock of existing assets and as a consequence, maintaining and preserving existing infrastructure becomes a central focus for policymakers.

By constructing a novel panel of over 3,000 renewal referendums, matching election results to local financial records and CoreLogic house-sale data, and fine-tuning a vision model on satellite imagery, we trace the full chain from a failed levy to deterioration in road quality and, ultimately, decline in property values. Our reduced-form estimates reveal that failing to renew a road-maintenance levy leads to an average funding loss of 11% for local road budgets, a 21% decline in a continuous road-quality score derived from remote sensing, and a 9% decrease in median house sale prices. The effect on home values emerges only after year four and persists through at least year nine, underscoring the gradual yet lasting consequences of underfunded maintenance. Heterogeneity analyses show that urban jurisdictions experience larger price declines than rural ones, and high-value houses are more sensitive to road-quality shocks than lower-value houses. We also find evidence of a dose-response relationship, where larger levies lead to greater declines in house prices.

Our findings speak directly to the literature on infrastructure-economics and hedonic-pricing by demonstrating how tax cuts that reduce local governments' ability to maintain roads can lead to a decline in road quality, which is then capitalized into local housing markets. From a policy perspective, our results highlight the importance of stable maintenance budget streams for local governments and considering proactive preservation strategies, especially in urban areas where property stakes are high.

Future work could extend our dynamic RD framework to other states or countries, investigate the effects of funding via other types of tax levies, and explore complementarities between road maintenance and other forms of public investments. Advances in AI vision architectures such as ConvNext, ViTs etc., open new avenues for researchers and could potentially allow real-time evaluation of road quality, and possibly enable assessment of infrastructure policies and their welfare consequences.

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Appendix

A Road Tax Renewal Elections in Ohio

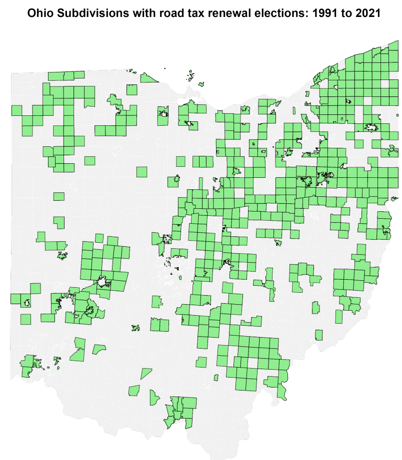


Figure 10: All Road Tax Renewal Elections in Ohio (1991-2021)

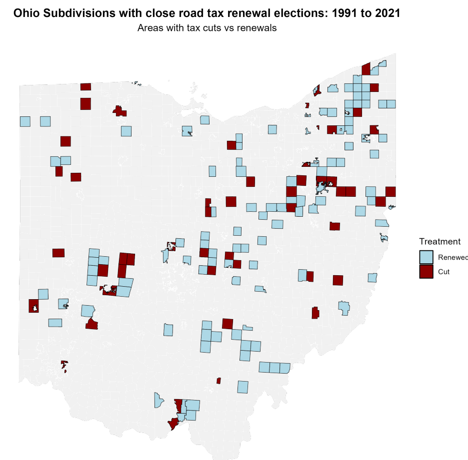


Figure 11: Close Road Tax Renewal Elections in Ohio (1991-2021)

Figure 10 displays the distribution of county subdivisions in Ohio that held at least one road tax renewal election between 1991 and 2021. The map reveals that although elections are relatively dispersed, they are more common in metropolitan areas such as Cleveland, Columbus, Cincinnati, Toledo etc. For jurisdictions that held multiple elections during this period, Figure 11 focuses on the subset of elections that were decided by narrow margins, specifically those closest to the 50% approval threshold that forms the basis of our regression discontinuity analysis. These close elections provide the quasi-experimental variation we exploit to identify the causal effects of road tax levy decisions on housing prices, and we do not observe any spatial clustering of the appearance of these close elections on the ballot, as well as the results of these close elections. The geographic distribution of close elections largely mirrors that of all elections, suggesting that narrow electoral outcomes are not systematically concentrated in particular types of communities, which supports the validity of our identification strategy.

B Additional Tables

B.1 Full set of Treatment Effects

Tables below present the full set of treatment effects for the results presented in our heterogeneity analysis and robustness checks. Table 10 presents the treatment effects after applying 1% Winsorization to the data, ensuring robustness against outliers. Table 11 breaks down the treatment effects by urban and rural categories, highlighting the differential impacts on housing prices based on the urbanization level.

Table 10: Full set of estimates – Median Housing Price (after 1% Winsorization)

Year relative to vote	**Estimate**	**Std. error**	***p*-value**	**Confidence interval**
$t-3$	258	6,217	0.967	[−11,927, 12,443]
$t-2$	1,870	6,257	0.765	[−10,393, 14,134]
$t-1$	3,489	7,202	0.628	[−10,627, 17,605]
t	−1,626	6,730	0.809	[−14,818, 11,566]
$t+1$	−4,312	8,906	0.628	[−21,767, 13,143]
$t+2$	−8,190	8,099	0.312	[−24,063, 7,683]
$t+3$	−13,275	7,588	0.080	[−28,146, 1,597]
$t+4$	−22,582	7,904	0.004	[−38,074, −7,090]
$t+5$	−20,952	9,554	0.028	[−39,678, −2,225]
$t+6$	−18,793	8,789	0.033	[−36,020, −1,566]
$t+7$	−17,918	7,180	0.013	[−31,990, −3,845]
$t+8$	−21,146	8,921	0.018	[−38,631, −3,660]
$t+9$	−17,789	7,770	0.022	[−33,018, −2,561]
$t+10$	−13,147	7,526	0.081	[−27,898, 1,604]

Notes: Supplements Figure 9 in main text. This table reports the treatment-effect estimates of cutting road tax levies (relative to renewing them) from three years before to ten years after the vote. Data are winsorized at the 1% level. All regressions include the covariates listed in Table 3. The outcome is the median house price in constant 2010 U.S. dollars (city-year observations). A coefficient of −22,582 at $t+4$ means that, four years after the vote, cities that cut their levies had median house prices $22,582 lower than those that renewed. Standard errors are in parentheses.

Table 11: Treatment Effects on Housing Prices by Urban vs. Rural Categories

Panel A: Urban

Year	Estimate	Std. Error	p-value	Conf. Interval
$t-3$	-2,636	8,066	0.744	[-18,446, 13,173]
$t-2$	-9,607	7,310	0.189	[-23,935, 4,722]
$t-1$	1,045	6,496	0.872	[1,045, 1,045]
t	458	7,873	0.954	[-14,973, 15,889]
$t+1$	-5,087	7,617	0.504	[-20,016, 9,843]
$t+2$	-3,675	9,077	0.686	[-21,465, 14,115]
$t+3$	-11,657	7,667	0.128	[-26,684, 3,370]
$t+4$	-8,846	9,162	0.334	[-26,804, 9,112]
$t+5$	-8,967	9,311	0.336	[-27,217, 9,284]
$t+6$	-24,476	7,127	0.001	[-38,446, -10,507]
$t+7$	-14,457	7,869	0.066	[-29,880, 966]
$t+8$	-26,174	8,921	0.003	[-43,659, -8,688]
$t+9$	-19,469	8,221	0.018	[-35,582, -3,357]
$t+10$	-23,969	10,364	0.021	[-44,284, -3,655]

Panel B: Rural

Year	Estimate	Std. Error	p-value	Conf. Interval
$t-3$	6,384	9,962	0.522	[-13,142, 25,910]
$t-2$	1,609	7,758	0.836	[-13,597, 16,814]
$t-1$	-835	8,354	0.920	[-835, -835]
t	-8,727	9,351	0.351	[-27,056, 9,601]
$t+1$	5,731	7,458	0.442	[-8,886, 20,349]
$t+2$	-2,970	7,107	0.676	[-16,899, 10,960]
$t+3$	-1,866	6,538	0.775	[-14,681, 10,949]
$t+4$	-10,505	8,641	0.224	[-27,441, 6,431]
$t+5$	-16,897	7,564	0.026	[-31,722, -2,072]
$t+6$	-4,561	6,551	0.486	[-17,402, 8,280]
$t+7$	-7,632	8,936	0.393	[-25,146, 9,882]
$t+8$	-469	8,063	0.954	[-16,273, 15,335]
$t+9$	-11,492	8,383	0.170	[-27,923, 4,940]
$t+10$	1,851	7,790	0.812	[-13,417, 17,119]

Notes: Supplements Figure 6 in text. Each panel reports separate regressions of median house price on a referendum "cut vs. maintain" indicator, broken down by Urban and Rural classifications. Columns show the year relative to the referendum vote, estimated treatment effect, standard error, p-value, and 95% confidence interval. Standard errors are robust. A negative estimate indicates lower house prices in areas that cut their road taxes relative to areas that maintain them.

Table 12: Treatment Effect on Housing Prices for Top-Quartile Tax Cuts

Year relative to vote	**Estimate**	**Std. error**	***p*-value**	**Confidence interval**
$t-3$	-2,850	13,435	0.832	[−29,183, 23,482]
$t-2$	-13,984	13,290	0.293	[−40,032, 12,064]
$t-1$	447	12,148	0.971	[−23,363, 24,257]
t	-10,456	15,381	0.497	[−40,603, 19,692]
$t+1$	-6,199	16,738	0.711	[−39,006, 26,608]
$t+2$	-18,657	17,844	0.296	[−53,631, 16,317]
$t+3$	-30,307	21,968	0.168	[−73,365, 12,751]
$t+4$	-31,679	13,072	0.015	[−57,300, −6,058]
$t+5$	-37,063	26,790	0.167	[−89,571, 15,446]
$t+6$	-29,830	15,478	0.054	[−60,167, 508]
$t+7$	-47,933	13,968	0.001	[−75,310, −20,557]
$t+8$	-38,800	16,894	0.022	[−71,913, −5,687]
$t+9$	-44,279	13,410	0.001	[−70,563, −17,995]
$t+10$	-38,659	17,087	0.024	[−72,150, −5,168]

Notes: Supplements Figure 7 in the text. Estimates compare cities that failed to renew a large (top-quartile) road maintenance tax levy with similar cities that renewed. The outcome is median house price in constant 2010 U.S. dollars; the unit of observation is the city–year. Covariates from Table 3 are included in every regression. A treatment effect of −\$31,679 in year $t+4$ means that four years after the vote, treated cities' median sale prices are \$31,679 lower than those of renewing cities.

C Additional Robustness Tests

A key RD assumption is that observations just above and below the threshold are comparable in all aspects except for treatment status. Table 13 presents balance tests across community characteristics at the voting threshold. As shown, demographic and socioeconomic variables—including population size, poverty rates, educational attainment, employment status, and racial composition—exhibit no statistically significant discontinuities at the cutoff. The absence of any discontinuity suggests that our estimated effects on housing prices represent the impact of failing to renew road tax levies rather than pre-existing community differences. This balance verification via a formal RD test, using covariates as the outcome variable, strengthens our conclusion that the observed housing price effects stem from decisions regarding road tax levies, not from underlying differences in community characteristics.

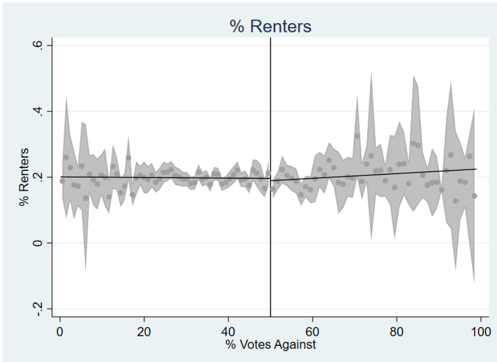
C.1 Covariate Discontinuity Table

Table 13: Covariate Discontinuity Test Results

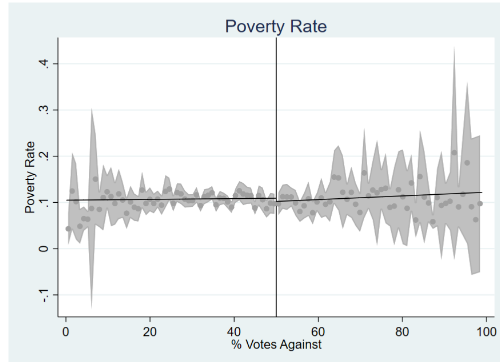
Variable	Estimate	Std. error	*p*-value	Confidence interval
Population	−388	1,094	0.722	[−2,532, 1,755]
Poverty Rate	0.017	0.014	0.234	[−0.011, 0.045]
% with Kids	−0.007	0.012	0.539	[−0.030, 0.015]
% Households with Children < 18	0.0001	0.007	0.981	[−0.014, 0.014]
% Less than High School Education	−0.004	0.020	0.834	[−0.043, 0.035]
% Some College Education	−0.012	0.011	0.274	[−0.034, 0.009]
% Renters	−0.005	0.015	0.754	[−0.035, 0.025]
Unemployment Rate	−0.002	0.006	0.733	[−0.013, 0.009]
% 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]
Income Heterogeneity Index	0.007	0.013	0.566	[−0.018, 0.033]
Median Family Income ($)	−3,147	2,963	0.288	[−8,953, 2,660]
% Under 5 Years Old	−0.006	0.004	0.126	[−0.013, 0.002]
% Aged 5 to 17	−0.007	0.007	0.284	[−0.021, 0.006]
% Aged 18 to 64	0.004	0.008	0.611	[−0.012, 0.021]
% Racial Minority	0.007	0.011	0.499	[−0.014, 0.028]

Notes: Each row reports the estimated discontinuity at the levy-approval cutoff for the specified covariate, using robust local-linear RD with the mean effective bandwidth. Confidence intervals are 95%.

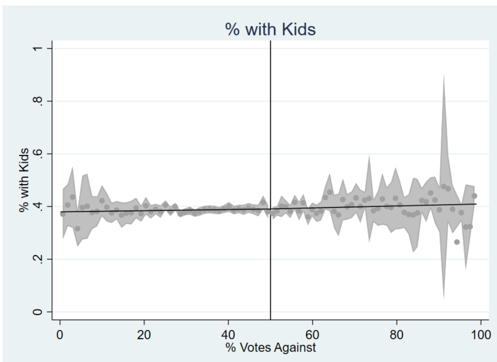
C.2 Covariate Discontinuity Plots



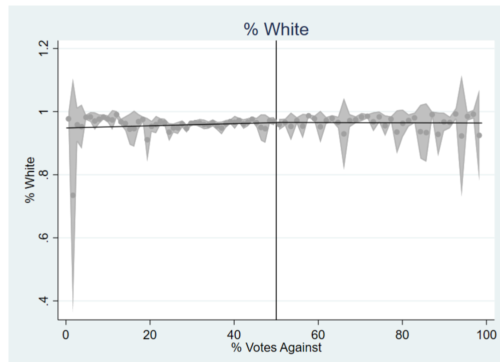
% Renters



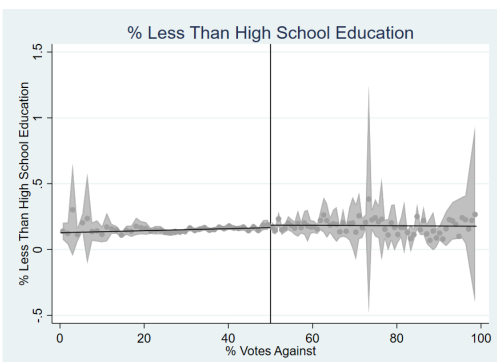
Poverty Rate



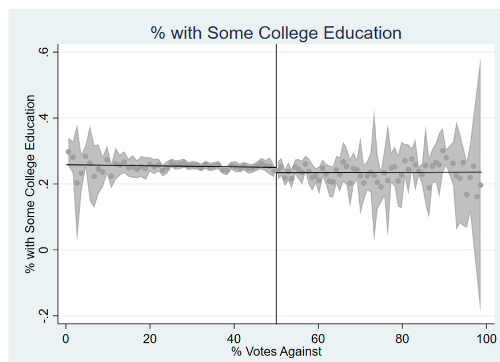
% With Kids



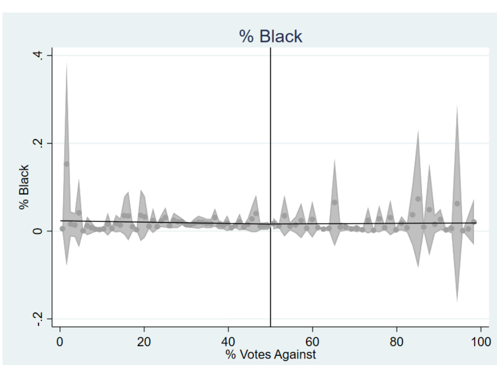
% White



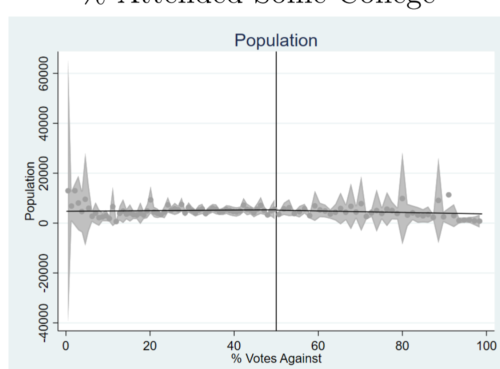
% with Less than High School education



% Attended Some College

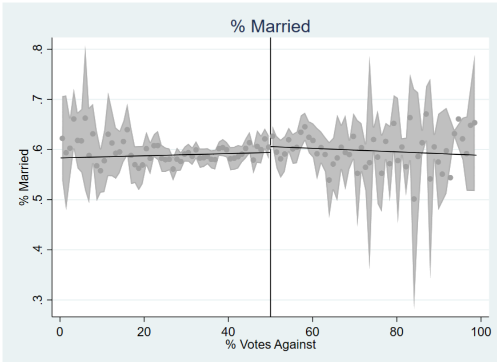


% Black

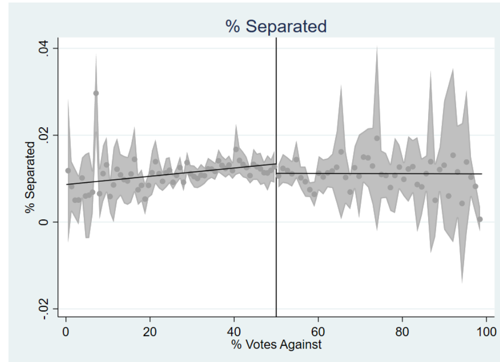


Population

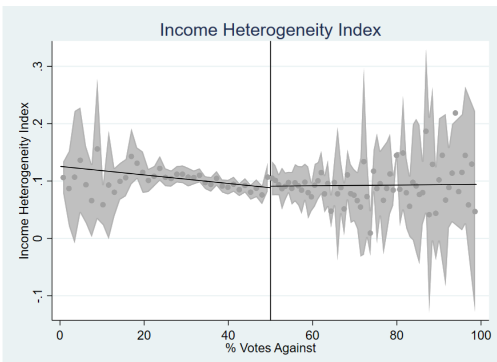
Figure 12: Covariate Discontinuity Plots - Part 1



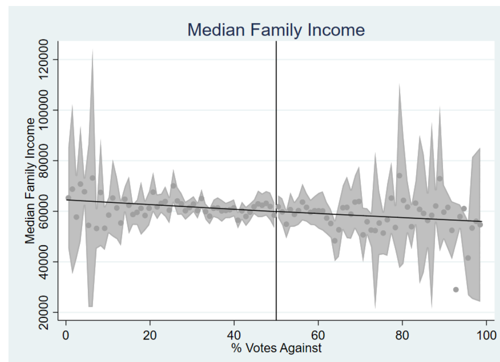
% Married



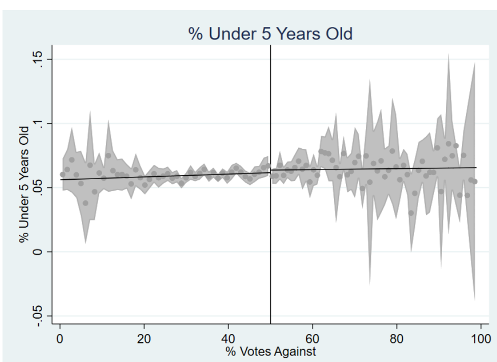
% Separated



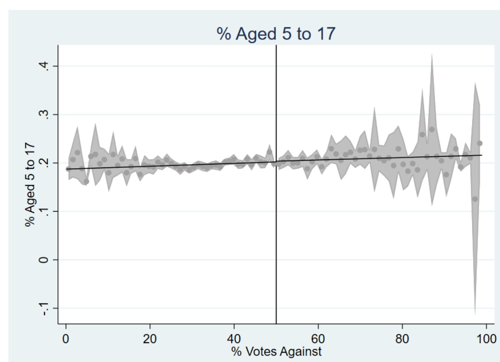
Income Heterogeneity Index



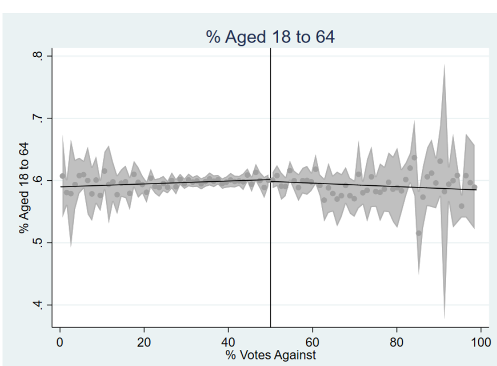
Median Family Income



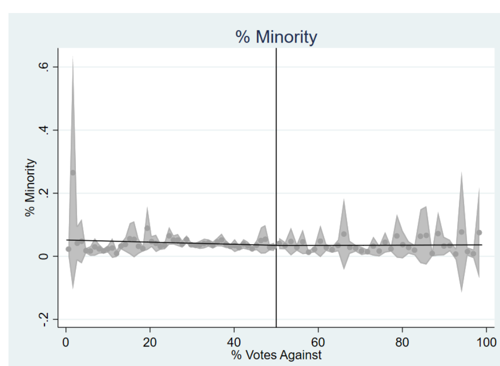
% Less than 5 years old



% between 5 to 17 years

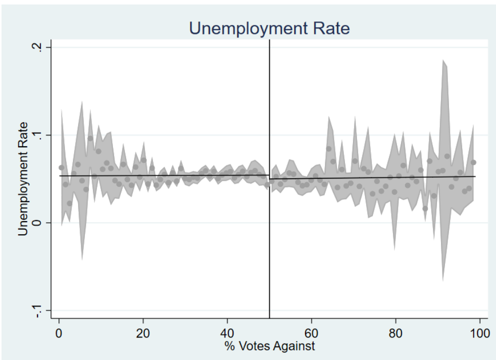


% between 18 to 64 years

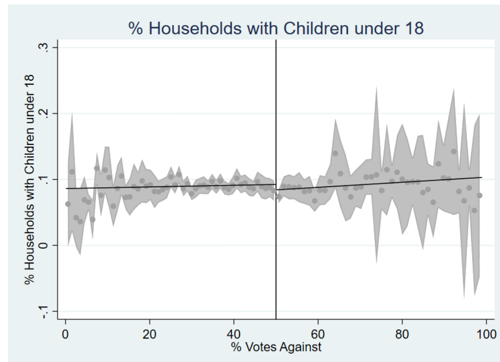


% Minority

Figure 13: Covariate Discontinuity Plots - Part 2



Unemployment Rate



% Households with Children 18 years old or less

Figure 14: Covariate Discontinuity Plots - Part 3

C.3 Bandwidth Sensitivity Analysis

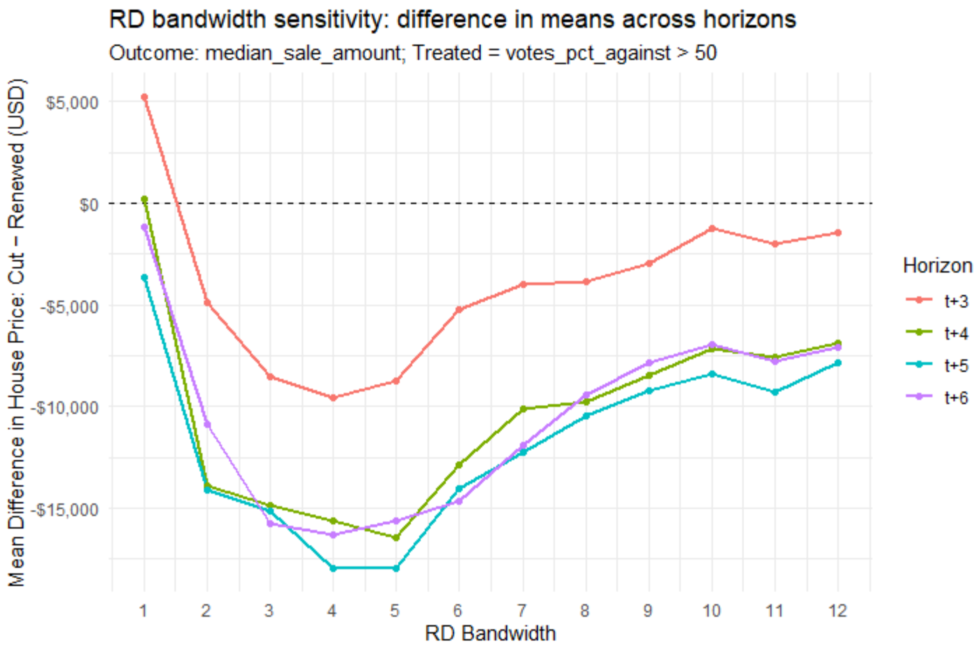


Figure 15: Bandwidth Sensitivity Analysis

Figure 15 plots the differences in mean estimate of the outcome variable, median house prices, between treated and control groups across a grid of bandwidth choices to observe how sensitive the estimates are to the choice of bandwidth. Estimates are consistently negative starting year t+4 and of comparable magnitude over a broad interval around the MSERD optimal bandwidth used in the main analysis. The relative flatness of the curve near the optimal bandwidth indicates that our main result is not driven by a finely tuned bandwidth choice.

D A Dynamic General Equilibrium Model of Roads

D.1 Representative Household

The economy is populated by a representative, infinitely–lived household that maximizes utility over consumption[21], housing services, and local amenities while disliking labor (including commuting time). In its most general form, the household's problem is to choose sequences of consumption $\{c_t\}_{t=0}^{\infty}$, housing stock $\{h_{t+1}\}_{t=0}^{\infty}$, private capital $\{K_{t+1}\}_{t=0}^{\infty}$, and labor supply $\{n_t\}_{t=0}^{\infty}$ to maximize lifetime utility:

$$\max_{\{c_t,h_t,n_t,K_{t+1},h_{t+1}\}_{t=0}^{\infty}} \sum_{t=0}^{\infty} \beta^t \tilde{u}\big(c_t, h_t, n_t, G_t\big), \qquad 0 < \beta < 1, \tag{5}$$

We consider the following utility function,

$$\tilde{u}\big(c_t, h_t, n_t, G_t\big) \;=\; c_t \, h_t^{\alpha_h A(G_t)} \, \exp\Big[-\frac{\kappa}{1+\theta} \left(n_t \left[1 + \phi(G_t)\right]\right)^{1+\theta}\Big] \tag{6}$$

where c_t denotes non–housing consumption, h_t the flow of housing services, and $A(G_t)$ the amenity derived from the public capital stock G_t, which represents the quality of local public goods.

After taking logs, we can write the utility function as a convenient separable specification

$$u\big(c_t, h_t, n_t, G_t\big) = \ln c_t + \alpha_h A(G_t) \ln h_t - \frac{\kappa}{1+\theta}\Big[n_t\big(1 + \phi(G_t)\big)\Big]^{1+\theta}, \tag{7}$$

with n_t hours of work[22], $\phi(G_t) \geq 0$ a commuting factor that rises as road quality falls, $\kappa > 0$, and $\theta > 0$ is the inverse Frisch elasticity of labor supply i.e. $\frac{1}{\theta}$ is the Frisch elasticity of labor supply with respect to real wages. The parameter α_h captures the household's preference for housing services, and $A(G_t)$ is a function that captures how public infrastructure quality (e.g., roads) affects the utility derived from housing. The term $\phi(G_t)$ captures the disutility of commuting, which increases as road quality deteriorates.

Budget Constraint and Capital/Housing Accumulation:

Let w_t be the real wage, r_t the rental rate on private capital K_t, $p_{h,t}$ the *pre–tax* purchase price of one unit of housing stock, τ_L the labor–income tax rate, and τ_h the property-tax rate levied on the *value of the housing stock*. The household's per-period budget can be written in the following way:

$$c_t \;+\; i_t \;+\; p_{h,t}\, x_t \;=\; (1-\tau_L) w_t n_t \;+\; r_t K_t \;-\; \underbrace{\tau_h \, p_{h,t} \, h_t}_{\text{property tax on holdings}} \tag{8}$$

[21]Since we don't have any random variables or distributions, we are not dealing with any expectations or expected utility.

[22]We model labor disutility using standard isoelastic form, as used in King, Plosser and Rebelo (1988) and Chetty et al. (2011).

where x_t is net housing investment and i_t is private–capital investment. The capital and housing asset laws of motion are

$$K_{t+1} = (1-\delta_K)K_t + i_t, \qquad \delta_K \in (0,1), \tag{9}$$

$$h_{t+1} = (1-\delta_h)h_t + x_t, \qquad \delta_h \in (0,1). \tag{10}$$

Equivalently, we can substitute out i_t and x_t to rewrite the budget constraint as,

$$c_t + K_{t+1} - (1-\delta_K)K_t + p_{h,t}[h_{t+1} - (1-\delta_h)h_t] = (1-\tau_L)w_t n_t + r_t K_t - \tau_h p_{h,t} h_t \tag{11}$$

Optimality Conditions:

Maximization of (7) subject to (8)–(11) yields:

1. **Consumption–Saving Euler Equation:** The household equalizes the marginal utility of consuming an additional unit today to the discounted utility of saving it until tomorrow. This yields:

$$U_c(c_t, h_t, A(G_t)) = \beta U_c(c_{t+1}, h_{t+1}, A(G_{t+1}))\big[(1-\tau_L)w_{t+1}\frac{\partial n_{t+1}}{\partial K_{t+1}} + r_{t+1} + 1 - \delta_K\big]. \tag{12}$$

which simplifies (under certainty and taking labor choices as separate) to

$$U_c(c_t, \cdot) = \beta U_c(c_{t+1}, \cdot)\big(1 + r_{t+1} - \delta_K\big) \tag{13}$$

In other words, the marginal rate of intertemporal substitution equals the (1+net discounted return o
factor. This is the standard Euler equation ensuring optimal capital accumulation.

Moreover, given that $U_c = \frac{1}{c_t}$ from our utility specification, we have

$$\frac{1}{c_t} = \beta\,\frac{1}{c_{t+1}}\big(1 + r_{t+1} - \delta_K\big); \tag{14}$$

2. **Labor–leisure trade–off:** The household chooses labor n_t such that the marginal disutility of working (including commuting) equals the after-tax marginal benefit of earning wages. Formally,

$$-U_n(c_t, h_t, A(G_t)) = (1-\tau_L)w_t U_c(c_t, h_t, A(G_t)) \tag{15}$$

where U_n is the partial derivative of utility with respect to labor (negative due to disutility) and U_c is the partial derivative with respect to consumption. Using our utility form, we get

$$U_n = -\kappa\big[n_t(1+\phi(G_t))\big]^{\theta}(1+\phi(G_t)), \quad U_c = \frac{1}{c_t}. \tag{16}$$

Thus, the labor optimality condition 15 becomes:

$$\kappa\big[n_t(1+\phi(G_t))\big]^{\theta}(1+\phi(G_t)) = \frac{(1-\tau_L)w_t}{c_t}. \tag{17}$$

$$\kappa\big(1+\phi(G_t)\big)^{1+\theta} n_t^{\theta} = \frac{(1-\tau_L)w_t}{c_t}; \tag{18}$$

$$n_t \;=\; \left[\frac{(1-\tau_L)\,w_t}{\kappa\,c_t\,\big(1+\phi(G_t)\big)^{1+\theta}}\right]^{1/\theta} \tag{19}$$

Intuitively, the marginal rate of substitution between leisure (or time) and consumption equals the real after-tax wage. An increase in commuting factor $\phi(G_t)$ (worse roads) raises the left-hand side (effective disutility of labor), leading the household to supply less labor for a given wage. This aligns with the idea that poor road quality reduces effective labor supply as households require higher compensation to work the same hours because commuting erodes their usable time.

3. **Euler equation for Housing:** From our utility function, we have $u_{c,t} = 1/c_t$ and $u_{h,t} = \alpha_h A(G_t)/H_t$. The FOC with respect to H_{t+1} gives

$$p_{h,t}\,u_{c,t} \;=\; \beta\Big[u_{h,t+1} \;+\; u_{c,t+1}\,(1-\delta_h-\tau_h)\,p_{h,t+1}\Big], \tag{20}$$

or, in consumption units,

$$p_{h,t} \;=\; \beta\,\frac{c_t}{c_{t+1}}\left[(1-\delta_h-\tau_h)\,p_{h,t+1} + \alpha_h\,A(G_{t+1})\,\frac{c_{t+1}}{H_{t+1}}\right] \tag{21}$$

$$p_{h,t} \;=\; \beta\left[(1-\delta_h-\tau_h)\,\frac{c_t}{c_{t+1}}\,p_{h,t+1} + \alpha_h\,A(G_{t+1}(\tau_h))\,\frac{c_t}{H_{t+1}}\right]. \tag{22}$$

The equation 22 encapsulates how the current price of housing $p_{h,t}$ reflects the discounted value of future benefits from owning housing and the benefits associated with holding it. The bracketed terms are, respectively, the *after–tax, after–depreciation resale value* next period and the *service dividend* (marginal value of one more unit of housing services) next period. The first term inside the brackets represents the discounted future resale value of the housing asset after accounting for depreciation and property taxes. The second term captures the flow of housing services (amenities) derived from owning the house, adjusted for the quality of public infrastructure $A(G_{t+1}(\tau_h))$ and scaled by consumption per unit of housing stock. Notice that the amenity value $A(G_{t+1}(\tau_h))$ depends on the future road quality, which in turn is influenced by the current property tax τ_h through its effect on maintenance spending, as explained in subsequent subsections. A cut in τ_h today leads to lower maintenance M_t, causing G_{t+1} to decline over time, which reduces $A(G_{t+1}(\tau_h))$ and thus lowers the service dividend from housing. This mechanism captures how changes in public infrastructure quality, driven by tax policy, affect housing prices through their impact on the flow of housing services.

D.2 Representative Firm

A competitive firm produces output Y_t with Cobb–Douglas technology

$$Y_t = K_t^{\alpha_k} \left(\mathcal{A}_t L_t\right)^{1-\alpha_k}, \qquad 0 < \alpha_k < 1, \tag{23}$$

enforcing factor–price equalization

$$w_t = (1-\alpha_k)\frac{Y_t}{L_t}, \qquad r_t = \alpha_k \frac{Y_t}{K_t}. \tag{24}$$

Profit Maximization. The firm operates in a competitive market for inputs and output, so it hires labor and rents capital until factor prices equal their marginal products. In each period, the firm solves

$$\max_{K_t, L_t} \big\{ F(K_t, L_t) - w_t L_t - r_t K_t \big\}, \tag{25}$$

taking wage w_t and capital rental r_t as given. The first-order conditions are:

1. **Labor demand:**

$$w_t = (1-\alpha_k) K_t^{\alpha_k} (\mathcal{A}_t L_t)^{-\alpha_k} = (1-\alpha_k)\frac{Y_t}{L_t}. \tag{26}$$

   This is the usual result that the real wage equals the marginal product of labor (MP_L).

2. **Capital demand:**

$$r_t = \alpha_k K_t^{\alpha_k - 1} (\mathcal{A}_t L_t)^{1-\alpha_k} = \alpha_k \frac{Y_t}{K_t}. \tag{27}$$

   Thus, the rental rate on capital equals the marginal product of capital (MP_K).

Under constant returns and competitive markets, the firm earns zero economic profit in equilibrium (all output is paid out to factors). This means

$$Y_t = w_t L_t + r_t K_t \tag{28}$$

in equilibrium. Because poor local roads can indirectly reduce effective labor supplied (households may choose smaller L_t) or even the productivity of labor (if $\mathcal{A}_t$ were to depend on G_t), the equilibrium wage and output will adjust accordingly. In our model, we focus on the household-side mechanism; we do not explicitly insert G_t into $F(\cdot)$ so $\mathcal{A}_t$ is treated as exogenous, but one could imagine an extension where $\mathcal{A}_t = \mathcal{A}(G_t)$ similar to household amenities function, making public infrastructure a productive externality that boosts TFP. That would reinforce the mechanism: a decline in G_t would lower productivity and thus lower wages from the firm side as well. For now, wages change endogenously mainly due to labor supply shifts in the simple model.

D.3 Government and Public Infrastructure

Taxation and Spending. The government collects taxes and uses the revenue to finance road maintenance. Taxes have two components: an exogenous property tax τ_h (subject to voter approval) and an endogenous tax (such as τ_L on labor income, or potentially a lump-sum tax) that provides additional revenue. Denote by T_t^{exo} the exogenous tax revenue from the voted levy, and T_t^{endo} the endogenous tax revenue from other sources. For example, if τ_L is a labor tax, then $T_t^{\text{endo}} = \tau_L w_t n_t$, which varies with the economy and is therefore "endogenous". The property tax revenue is $T_t^{\text{exo}} = \tau_h p_{h,t} h_t$ each period, directly tied to the housing market outcome.

The government budget constraint is:

$$T_t^{\text{endo}} + T_t^{\text{exo}} = M_t, \tag{29}$$

assuming all tax revenue is spent on road maintenance M_t. We abstract from any other government consumption or transfers.

In a scenario where the road tax is cut (e.g., τ_h drops to zero after a failed levy vote), T_t^{exo} falls exogenously. For a balanced budget, maintenance spending M_t must be reduced by an equal amount. This captures the immediate fiscal impact of the tax cut, such as an observed 11% loss in road maintenance funding empirically.

Public Capital (Roads) Accumulation. The stock of public road infrastructure G_t evolves over time depending on maintenance. We adopt a law of motion in line with Rioja (2003) to formalize how insufficient maintenance leads to infrastructure depreciation, whereas sufficient maintenance can maintain road quality[23]. Let δ_G be the "normal" depreciation rate of roads (wear-and-tear if adequately maintained). Actual effective depreciation $\delta_{G,t}$ can exceed δ_G if maintenance is below the requirement. A convenient formulation is:

$$G_{t+1} = G_t(1 - \delta_{G,t}), \tag{30}$$

where

$$\delta_{G,t} = \varphi \cdot \max\left\{0, 1 - \frac{M_t}{\delta_G G_t^{\psi}}\right\}. \tag{31}$$

In words, if maintenance spending M_t falls short of the amount needed to cover normal depreciation ($\delta_G G_t$), the depreciation rate increases proportionally to the shortfall. The parameter $\varphi \geq 0$ governs how sensitive the infrastructure is to under-maintenance. For example, if M_t is only half of $\delta_G G_t$, then $1 - \frac{M_t}{\delta_G G_t} = 0.5$, and $\delta_{G,t} = 0.5\varphi$; this means roads wear out. If M_t is zero (no upkeep), depreciation could jump substantially (if φ is large, G_t may deteriorate very quickly). On the other hand, if M_t is at least $\delta_G G_t$ (maintenance fully covers yearly wear), then $\delta_{G,t} = 0$ (no deterioration). We also assume $\delta_{G,t}$ cannot go below 0 even if M_t exceeds $\delta_G G_t$ (any extra maintenance beyond replacing depreciation might marginally improve roads but with diminishing returns). This Riojas-style law of motion captures the intuition that lack of maintenance shortens the life of existing public capi-

[23]In this setup, roads can only be maintained, not improved.

tal. Maintenance spending effectively slows down depreciation, preserving the infrastructure stock; cutting maintenance causes G_t to decline faster over time. Importantly, a reduction in the road tax τ_h translates to lower M_t and thus a gradual decline in G_t over several periods. The deterioration in road quality will typically become noticeable after a few periods of under-maintenance, consistent with evidence that the negative effects on road conditions (and subsequently house prices) emerge with a lag. In our model, that lag is endogenously determined by the above law of motion – if M_t drops at $t = 0$, G_t will depreciate a bit faster each period, compounding into a significant drop in infrastructure quality after some years.

D.4 Housing Market

Housing is a tradable asset in *fixed* aggregate supply $\bar{h}$[24]. With a representative household, market clearing is

$$h_t \;=\; \bar{H} \quad \text{for all } t. \tag{32}$$

Given (32), the price $p_{h,t}$ is determined by the housing Euler (21) and the paths of $\{c_t, G_t\}$. In steady state, substituting $c_{t+1} = c_t = c$, $h_{t+1} = h_t = h = \bar{H}$, $G_{t+1} = G_t = G$, $p_{h,t+1} = p_{h,t} = p_h$ into (21) yields the user–cost formula:

$$p_h \;=\; \frac{\beta}{1 - \beta(1 - \delta_h - \tau_h)} \alpha_h \, A(G) \, \frac{c}{h}. \tag{33}$$

Better roads i.e. higher $A(G)$ higher raise p_h. Larger h reduces marginal service value (log utility), lowering p_h ceteris paribus.

The steady-state housing price formula in equation (33) reveals how the discount factor β interacts with depreciation δ_h and property taxes τ_h to determine asset valuation. The term $\frac{\beta}{1-\beta(1-\delta_h-\tau_h)}$ represents the present value multiplier that converts the flow of housing services into an asset price.

To understand this multiplier, consider that $1-\delta_h-\tau_h$ represents the net retention rate of housing value after accounting for physical depreciation and tax obligations. The household effectively loses fraction $\delta_h + \tau_h$ of the housing asset's value each period through wear and tax payments. The remaining fraction $1 - \delta_h - \tau_h$ carries forward to the next period, where it generates both service flows and continuation value.

The economic intuition is clear: higher property taxes τ_h reduce the net retention rate, making housing a less attractive store of value and lowering equilibrium prices. A patient household (high β) values future service flows more highly, increasing the multiplier and thus housing prices. Conversely, rapid depreciation δ_h erodes the asset's continuation value, reducing prices through the same channel.

This formulation also shows why property tax cuts can have ambiguous effects on housing prices in our model. While lower τ_h directly increases the multiplier and boosts prices, it also reduces maintenance funding, leading to infrastructure decay that lowers the service flow $\alpha_h A(G)$. The net effect depends on the relative magnitudes of these opposing forces and their timing.

[24]This means that the total quantity of housing is constant over time, and any changes in demand will only affect prices. We can also allow extensions to the model that incorporate changes in housing supply.

D.5 Competitive Equilibrium

We now define a recursive competitive equilibrium for this economy. Given an initial private capital stock K_0 and initial public capital (road quality) G_0, and a sequence of exogenous tax policy $\{\tau_{h,t}\}$ (with a drop in τ_h at the time of the tax cut shock), a competitive equilibrium is a sequence of allocations $\{c_t, h_{t+1}, n_t, K_{t+1}, M_t, G_t\}_{t\geq 0}$ and prices $\{w_t, r_t, p_{h,t}\}_{t\geq 0}$ such that:

1. **Household Optimization:** Given prices and taxes, the representative household chooses $\{c_t, h_{t+1}, n_t, K_{t+1}\}$ to maximize its utility subject to its budget and accumulation constraints. The household's choice satisfies the first-order optimality conditions described above (Consumption and housing Euler equations, labor supply condition, and housing demand condition, etc.), and the transversality condition on capital holding.

2. **Firm Optimization:** The representative firm chooses inputs $\{K_t, L_t\}$ each period to maximize profit. In equilibrium, $L_t = n_t$ (labor market clears) and $K_t = K_t$ held by the household (capital market clears), and the wage and rental rates satisfy $w_t = F_L(K_t, L_t)$ and $r_t = F_K(K_t, L_t)$ as given by the Cobb-Douglas marginal products. This ensures the firm's first-order conditions are met and profit is zero.

3. **Government Budget and Public Capital:** The government budget constraint holds each period: $T_t^{\text{endo}} + T_t^{\text{exo}} = M_t$, with $T_t^{\text{exo}} = \tau_{h,t} p_{h,t} h_t$ and $T_t^{\text{endo}} = \tau_L w_t n_t$. The road maintenance M_t feeds into the law of motion for G_t: given G_t at the start of period t, the next period's public capital G_{t+1} is determined by $G_{t+1} = G_t(1 - \delta_{G,t})$ with $\delta_{G,t}$ increasing if M_t is insufficient (as specified above). The government sets M_t according to the available tax revenue each period.

4. **Market Clearing:** All markets clear in equilibrium:

   - **Goods market:** Output is used for private consumption, private investment, and public maintenance. The resource constraint each period is:

     $$Y_t = c_t + i_t + M_t. \tag{34}$$

     Total output Y_t produced by the firm equals the sum of household consumption c_t, private investment i_t, and government demand for road maintenance M_t.

   - **Labor market:** $L_t = n_t$. The labor supplied by the household equals labor demanded by the firm. The real wage adjusts such that this holds.

   - **Capital market:** The firm's capital input equals the household's capital stock: K_t (used in production) $= K_t$ (owned by household). The rental rate r_t adjusts to clear this market.

   - **Housing market:** $h_t = \bar{H}$. The total housing demanded by the household equals the fixed housing stock. The house price $p_{h,t}$ adjusts each period to clear the housing market.

5. **Consistency:** The expectations of the household are consistent with realized outcomes. In a deterministic steady-state scenario, this means the household correctly

anticipates the path of prices and G_t. In a perfect-foresight or rational expectations equilibrium, the household foresees that a cut in τ_h implies lower future M_t and thus a gradually declining G_t, factoring this into its decisions. The sequence $\{p_{h,t}\}$ reflects the present value of housing services given the expected decline in amenities.

D.6 Solving the model

The steady-state equilibrium can be characterized as a system of 9 equations in 9 unknowns. We solve for the following endogenous variables: $(c, n, K, Y, w, r, q, M, G)$, where $q \equiv (1 + \tau_h)p_h$ is the effective housing price and housing quantity is fixed at $h = \bar{H}$.

D.6.1 System of Equilibrium Equations

The steady-state equilibrium is determined by the following 9 independent equations:

1. **Euler equation (steady-state):** From the intertemporal optimality condition:

$$1 = \beta(1 + r - \delta_K) \quad \Rightarrow \quad r = \delta_K + \beta^{-1} - 1 \tag{35}$$

   This pins down the rental rate r directly from parameters.

2. **Firm capital pricing:** From profit maximization:

$$r = \alpha_k \frac{Y}{K} \tag{36}$$

   This relates the output-capital ratio to the rental rate.

3. **Firm wage pricing:** With labor market clearing $L = n$:

$$w = (1 - \alpha_k)\frac{Y}{n} \tag{37}$$

   This relates the wage to output and labor supply.

4. **Production technology:** The Cobb-Douglas production function:

$$Y = K^{\alpha_k}(A \cdot n)^{1-\alpha_k} \tag{38}$$

   This ties together output, capital, and labor.

5. **Labor-leisure trade-off:** From household optimization:

$$\kappa[1 + \phi(G)]^{1+\theta} n^{\theta} = \frac{(1 - \tau_L)w}{c} \tag{39}$$

   This links labor supply to wages, consumption, and road quality through commuting costs.

6. **Housing price determination:** From the housing Euler equation in steady state:

$$q = \frac{\beta}{1 - \beta(1 - \delta_h - \tau_h)} \cdot \alpha_h A(G) \frac{c}{\bar{H}} \tag{40}$$

This determines the housing price from the present value of housing services, incorporating the discount factor and net retention rate.

7. **Government budget constraint:** Balanced budget condition:

$$M = \tau_L w n + \tau_h p_h \bar{H} \tag{41}$$

This defines maintenance spending from tax revenues.

8. **Road quality steady-state:** From the law of motion $G_{t+1} = G_t(1 - \delta_{G,t})$:

$$G = G(1 - \delta_G) \quad \text{where} \quad \delta_G = \varphi \cdot \max\left\{0, 1 - \frac{M}{\delta_G^{\text{norm}} G^{\psi}}\right\} \tag{42}$$

In the minimal-upkeep steady state, this simplifies to $M = \delta_G^{\text{norm}} G^{\psi}$.

9. **Resource constraint:** Goods market clearing with capital accumulation:

$$c = Y - \delta_K K - M \tag{43}$$

Note that $i = K - (1 - \delta_K)K = \delta_K K$ in steady state. This accounts for the allocation of output between consumption, investment, and maintenance.

D.7 Transitional Dynamics and Welfare Analysis

To analyze the transitional dynamics following a tax cut, we solve the model numerically two different ways: under perfect foresight[25] and partial adjustment[26]. The key steps are:

1. **Calibration:** Assign parameter values based on empirical estimates or literature benchmarks. For example, β, α_h, η, κ, θ, δ_K, δ_G, and φ are calibrated to match observed household behavior, infrastructure depreciation rates, and labor supply elasticities.

2. **Initial Steady State:** Compute the pre-tax-cut steady state by solving the equilibrium conditions under the initial τ_h. This provides baseline values for $\{M_t, G_t, p_{h,t}, c_t, n_t, Y_t\}$.

3. **Shock Implementation:** Introduce the tax cut by reducing τ_h at $t = 0$. Solve the model forward to trace the path of endogenous variables $\{M_t, G_t, p_{h,t}, c_t, n_t, Y_t\}$ as the economy transitions to the new steady state.

[25] Households are forward-looking and adjust their behavior immediately to changes in policy and economic conditions.

[26] Households adjust their behavior gradually in response to changes in policy and economic conditions.

4. **Dynamic Path:** Use one of the methods stated above to compute the full transition path of $\{M_t, G_t, p_{h,t}, c_t, n_t, Y_t\}$. Households anticipate the future decline in G_t due to lower maintenance and adjust their consumption, labor supply, and housing demand accordingly. The path of G_t is updated each period using the law of motion in equation 30.

5. **Welfare Analysis:** Compute the household's lifetime utility before and after the tax cut. Decompose the welfare change into short-run gains (higher disposable income) and long-run losses (lower amenities and higher commuting costs).

6. **Sensitivity Analysis:** Vary key parameters (e.g., φ, α_h, θ) to test the robustness of the results. Examine how the magnitude of the tax cut or alternative government policies affect outcomes.

The numerical solution involves iterating on the household's Euler equations, firm conditions, and government budget constraint while updating G_t using its law of motion. This allows us to simulate the gradual decline in road quality and its impact on housing prices, labor supply, and overall welfare.

D.8 Discussion of Policy Shock

A permanent cut in the voted levy lowers τ_h exogenously. In the short run, disposable income rises as tax burden decreases, but G_t is initially unchanged. This leads to a sharp increase in consumption but also a sharp decrease in maintenance budget. Over time, reduced maintenance spending accelerates depreciation in equation 30, lowering $A(G_t)$ and raising $\phi(G_t)$; this depresses labor supply, housing demand, and therefore equilibrium house prices, as demonstrated in Section D.10.

D.9 Model Calibration

The model parameters are calibrated based on empirical evidence and standard values from the literature. Table 14 summarizes the calibrated values used in the analysis. These values are chosen to reflect realistic economic conditions. For example, $\beta = 0.96$ corresponds to a 4% annual discount rate, while $\delta_K = 0.05$ reflects typical depreciation rates for private capital. Other parameters such as labor disutility parameters κ and θ are calibrated to match observed labor supply elasticities, and φ captures the empirical sensitivity of road quality to maintenance shortfalls.

D.10 Model Results

The model is solved numerically using both a perfect foresight and a partial adjustment algorithm to simulate the economy's response to a permanent cut in the road maintenance tax τ_h. Below, we present the key results from partial adjustment simulations[27].

[27] We focus on partial adjustment results here as they are better equipped for illiquid housing markets and gradual behavioral responses. Perfect foresight results are qualitatively similar but exhibit sharper short-run dynamics.

Table 14: Baseline calibration and sensitivity ranges

Parameter	Model block	Baseline	Sensitivity range	Key empirical source
β	Household intertemporal utility	0.96	0.94–0.99	Cooley (1995)
α_h	utility weight on housing services	0.35	0.25–0.40	(U.S. Bureau of Labor Statistics, 2024)
η	Amenity elasticity	0.88		†
θ	Inverse Frisch elasticity labor supply	2	1.5–4.0	Chetty et al. (2011)
κ	Labor-disutility scale	2.9	2.9	Chatterjee, Gibson and Rioja (2017)
α_k	Private-capital share	0.30	0.25–0.40	Chatterjee, Gibson and Rioja (2017)
δ_K	Private Capital depreciation (annual)	0.06	0.04–0.08	Cooley (1995)
δ_h	Housing depreciation (annual)	0.05	0.04–0.08	
δ_G^{norm}	Road stock depreciation	0.25		†
φ	Under-maintenance sensitivity	0.64	0.25–0.75	Rioja (2003)
γ	commuting disutility sensitivity	0.47		†
ψ	maintenance requirement elasticity	0.47		†
τ_L	Labor income tax share for roads	0.35		†

Note: † Calibrated to match empirical moments

- A reduction in τ_h from 1% to 0% leads to an decrease in maintenance budget M_t and road quality G_t, which declines gradually over time due to the law of motion for public capital.
- The house price $p_{h,t}$ first increases and then falls significantly as the amenity value $A(G_t)$ declines, reflecting the reduced desirability of housing in areas with deteriorating roads

The figures 16 illustrates the dynamics of key variables over time following the tax cut. The model's transitional dynamics following a permanent reduction in the property tax from 1 mills to 0 mills.

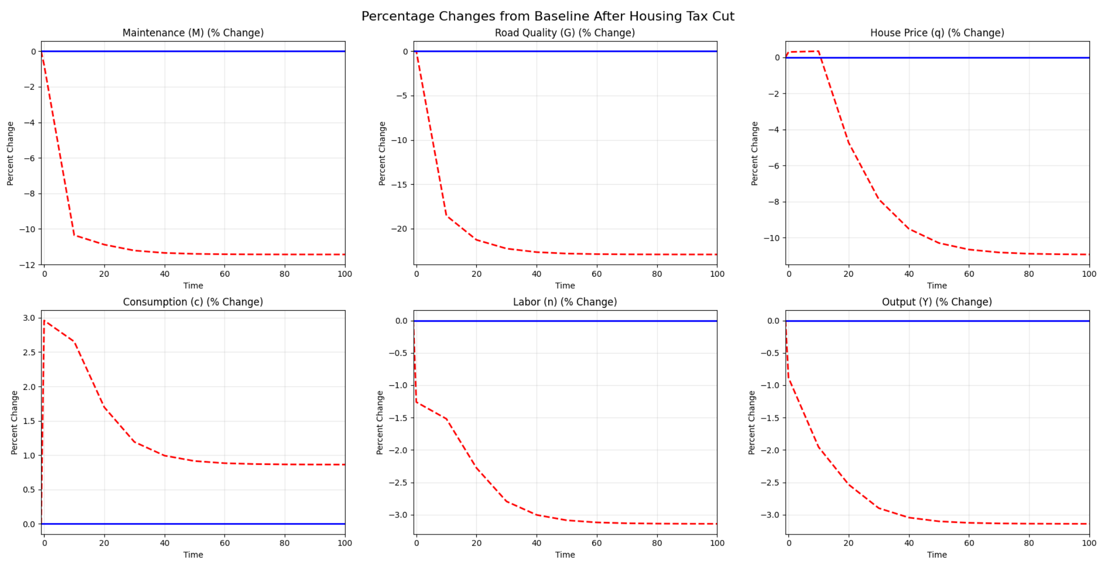


Figure 16: Dynamics of variables following the tax cut.

Figure 16 plots the full transition paths (percentage deviations from baseline). Most of the adjustment in M_t and q_t occurs early, while G_t converges more gradually as under-maintenance compounds over time.Findings from Figure 16 are summarized below:

- **Roads and maintenance.** Following the road tax cut, the road maintenance budget drops by about 10% and then drifts to roughly -11% (top-left panel of Figure 16). Given the law of motion for roads, G_t declines gradually and monotonically, approaching a long-run decline to 21% by the end of the simulated horizon.
- **House prices.** The housing price q_t initially shows a small blip (about $+0.3\%$) and then declines smoothly toward a long-run drop of an average of 9% over the simulated horizon as the amenity value $A(G_t)$ deteriorates.
- **Aggregates.** The macro aggregates move modestly: labor n_t falls by about -3.0%, output also Y_t by about -3%, and consumption c_t increases by about 3% in the long run. Consumption exhibits a slight short-run overshoot (of $+3\%$) as households initially respond to higher disposable income from the tax cut, before settling slightly

above baseline (bottom row). This reflects the household's initial response to higher disposable income from the tax cut, followed by adjustments and substitution effect as road quality declines.

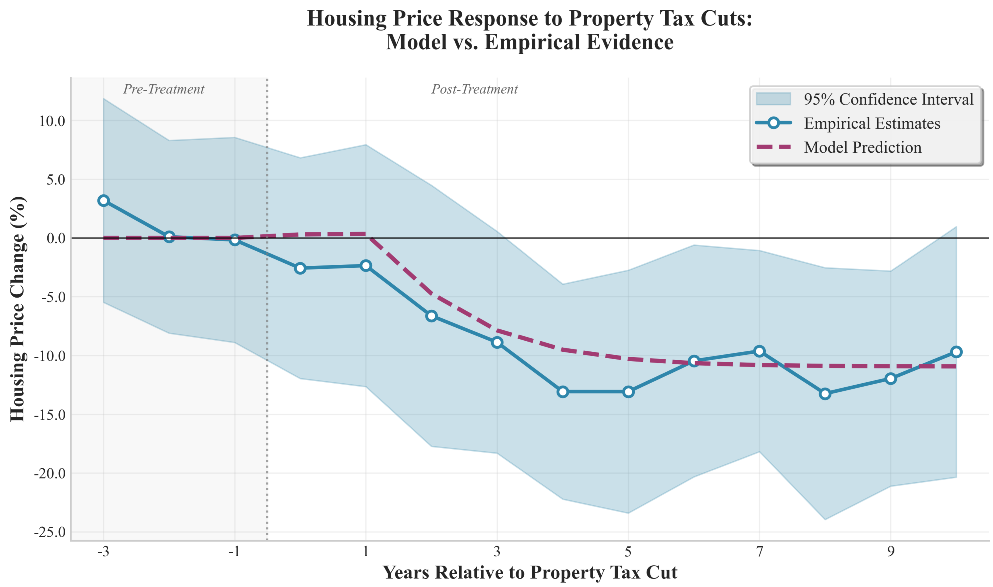


Figure 17: Model vs Empirical Estimates of House Prices Following the Tax Cut.

Model vs. empirical house-price path. Figure 17 compares the model-implied house-price response to the event-study estimates in Figure 5. After aligning the time units, the model tracks the medium- and long-run decline well, squarely within the empirical confidence band. The model is slightly conservative initially as the drop in housing prices occurs more gradually than the empirical estimates. Nevertheless, the overall magnitude and timing of the decline remain consistent with the data as the model starts to deviate from the baseline around year 2 and approaches a long-run average drop of about 9% by year 10. This pattern is consistent with the mechanism presented in the paper: eliminating the tax reduces maintenance revenues, which gradually lowers road quality, depresses amenity $A(G_t)$, and in turn pulls down housing valuations.

Overall, the calibrated model reproduces the targeted steady-state effects (about -21% in roads, -9% in house prices, and -11% in maintenance) and generates transitional dynamics for house prices that closely follow the empirical path while remaining slightly less sharp at the short-run trough.

D.11 Model Limitations

1. **Representative agent assumption.** Heterogeneity in income or commuting preferences could attenuate price effects. Sensitivity analyses with quasi-linear preferences or idiosyncratic $\phi(G_t)$ show qualitative robustness.

2. **No spatial heterogeneity.** We currently ignore spatial heterogeneity. The model intentionally abstracts from space for tractability. Future work can embed G_t in a Rosen–Roback framework to capture cross-location sorting.

3. **Fixed housing stock.** Allowing new construction would introduce a supply elasticity that dampens price responses. Empirical evidence suggests housing supply in many U.S. jurisdictions is quite inelastic.

4. **No direct productivity role for roads.** Incorporating G_t into firm TFP $\mathbb{A}_t$ would further enrich the model. However, this would require additional assumptions about how road quality affects firm location and productivity, which is beyond our current scope.

The DGE framework formalizes the intuition that road–maintenance tax cuts provide immediate relief but, by undermining infrastructure quality, erode local amenities and raise commuting costs, culminating in lower house values and welfare.

D.12 Derivations of Key Equations

D.12.1 Derivation of the Euler Equation for consumption 14

Below $U_c \equiv \partial u/\partial c$ and λ_t is the Lagrange multiplier on the period-t budget constraint.

Set up the Lagrangian:

$$L = \sum_{t=0}^{\infty} \beta^t \Big[u(c_t, h_t, A(G_t)) + \lambda_t \Big((1-\tau_L) w_t n_t + r_t K_t - \tau_h p_{h,t} h_t - c_t - p_{h,t} h_t - K_{t+1} + (1-\delta_K) K_t \Big) \Big].$$

FOC w.r.t. consumption c_t:

$$\frac{\partial L}{\partial c_t} = 0 \quad \Longrightarrow \quad \beta^t U_c(c_t, h_t, A(G_t)) - \lambda_t = 0 \quad \Longrightarrow \quad \lambda_t = \beta^t U_c(c_t, h_t, A(G_t)). \tag{A1}$$

FOC w.r.t. next-period capital K_{t+1}:

$$\frac{\partial L}{\partial K_{t+1}} = 0 \quad \Longrightarrow \quad -\lambda_t + \lambda_{t+1} \Big[(1-\tau_L) w_{t+1} \frac{\partial n_{t+1}}{\partial K_{t+1}} + r_{t+1} + 1 - \delta_K \Big] = 0. \tag{A2}$$

Combine (A1) and (A2):

Insert λ_t and λ_{t+1} from (A1) into (A2):

$$\beta^t U_c(c_t, h_t, A(G_t)) = \beta^{t+1} U_c(c_{t+1}, h_{t+1}, A(G_{t+1})) \Big[(1-\tau_L) w_{t+1} \frac{\partial n_{t+1}}{\partial K_{t+1}} + r_{t+1} + 1 - \delta_K \Big].$$

Divide both sides by β^t to obtain:

$$U_c(c_t, h_t, A(G_t)) = \beta U_c(c_{t+1}, h_{t+1}, A(G_{t+1}))\Big[(1-\tau_L)w_{t+1}\frac{\partial n_{t+1}}{\partial K_{t+1}} + r_{t+1} + 1 - \delta_K\Big].$$

Why the derivative $\frac{\partial n_{t+1}}{\partial K_{t+1}}$? Capital chosen today affects tomorrow's labor choice only through the household's optimal plan. If, as we assume, labor in each period is chosen independently of the inherited capital stock, this derivative is zero and the term drops out, giving the familiar

$$U_c(c_t, \cdot) = \beta U_c(c_{t+1}, \cdot)\big(1 + r_{t+1} - \delta_K\big),$$

which is the standard Euler equation for intertemporal consumption smoothing.

Equation 10 still contains the marginal utility of consumption $U_c(\cdot)$. Because the one-period utility function chosen is:

$$u(c_t, h_t, A(G_t)) = \ln c_t + \alpha_h \ln h_t + \eta \ln A(G_t) - \frac{\kappa}{1+\theta}\big[n_t(1+\phi(G_t))\big]^{1+\theta}, \tag{6}$$

its partial derivative with respect to consumption is:

$$U_c(c_t, h_t, A(G_t)) = \frac{\partial u}{\partial c_t} = \frac{1}{c_t}. \quad \text{(log utility)}$$

Substituting U_c into the Euler Equation:

Start from Eq. 10:

$$U_c(c_t, \cdot) = \beta\, U_c(c_{t+1}, \cdot)\big(1 + r_{t+1} - \delta_K\big). \tag{10}$$

Replace U_c with $\frac{1}{c}$ (log utility):

$$\frac{1}{c_t} = \beta\,\frac{1}{c_{t+1}}\big(1 + r_{t+1} - \delta_K\big).$$

This is exactly Eq. 11:

$$\boxed{\frac{1}{c_t} = \beta\,\frac{1}{c_{t+1}}\big(1 + r_{t+1} - \delta_K\big)}. \tag{11}$$

D.12.2 Labor–leisure trade–off

To derive the labor-leisure trade-off condition, we start by computing the partial derivatives of the utility function with respect to consumption and labor:

For consumption:

$$u_c(c_t, h_t, n_t, G_t) = \frac{1}{c_t} \tag{B1}$$

For labor, we have:

$$u_n(c_t, h_t, n_t, G_t) = -\,\kappa\big[n_t\big(1+\phi(G_t)\big)\big]^{\theta}\,\big(1+\phi(G_t)\big). \tag{B2}$$

Optimal labour supply equates the marginal disutility of work to the after-tax marginal benefit of wages

$$-\,u_n(c_t, h_t, n_t, G_t) \;=\; (1-\tau_L)\, w_t\, u_c(c_t, h_t, n_t, G_t)$$

Using B1 and B2, we can rewrite this as:

$$\kappa\big[n_t\big(1+\phi(G_t)\big)\big]^{\theta}\big(1+\phi(G_t)\big) \;=\; \frac{(1-\tau_L)\, w_t}{c_t}$$

This is exactly the condition we wanted to derive:

$$\kappa\big(1+\phi(G_t)\big)^{1+\theta}\, n_t^{\theta} \;=\; \frac{(1-\tau_L)\, w_t}{c_t}$$

$$n_t \;=\; \left[\frac{(1-\tau_L)\, w_t}{\kappa\, c_t\, \big(1+\phi(G_t)\big)^{1+\theta}}\right]^{1/\theta}$$

D.12.3 Euler equation for Housing

We derive the housing Euler starting from the household's problem.

1. Primitives. Per–period utility:

$$u(c_t, h_t, n_t, G_t) = \ln c_t + \alpha_h A(G_t) \ln h_t - \frac{\kappa}{1+\theta}\big[n_t(1+\phi(G_t))\big]^{1+\theta}.$$

Marginal utilities (holding G_t fixed):

$$u_{c,t} = \frac{1}{c_t}, \qquad u_{h,t} = \frac{\partial u}{\partial h_t} = \alpha_h \frac{A(G_t)}{h_t}.$$

2. Budget constraint in stock form. Using $H_{t+1} = (1-\delta_h)H_t + x_t$ and substituting $x_t = H_{t+1} - (1-\delta_h)H_t$, the period-$t$ budget is

$$c_t + i_t + p_{h,t}\big(H_{t+1} - (1-\delta_h)H_t\big) = (1-\tau_L)w_t n_t + r_t K_t - \tau_h p_{h,t} H_t.$$

Rearrange the housing terms:

$$-p_{h,t}H_{t+1} + p_{h,t}(1-\delta_h)H_t - \tau_h p_{h,t} H_t = -p_{h,t}H_{t+1} + p_{h,t}(1-\delta_h-\tau_h)H_t.$$

3. Lagrangian. Let λ_t be the multiplier on the budget constraint:

$$
\begin{aligned}
\mathcal{L} = \sum_{t=0}^{\infty} \beta^t \Big\{ & u(c_t, h_t, n_t, G_t) \\
& + \lambda_t \big[(1-\tau_L) w_t n_t + r_t K_t - c_t - i_t - p_{h,t}(H_{t+1} - (1-\delta_h) H_t) \\
& - \tau_h p_{h,t} H_t - (K_{t+1} - (1-\delta_K) K_t) \big] \Big\}
\end{aligned} \tag{44}
$$

4. FOCs for c_t and H_{t+1}. Consumption:

$$
\frac{\partial \mathcal{L}}{\partial c_t} = 0 \;\Rightarrow\; \lambda_t = u_{c,t}.
$$

For H_{t+1} (differentiating with respect to the housing stock chosen for next period):

$$
\frac{\partial \mathcal{L}}{\partial H_{t+1}} = 0 : \quad \beta^{t+1} u_{h,t+1} + \beta^{t+1} \lambda_{t+1} p_{h,t+1} (1 - \delta_h - \tau_h) - \beta^t \lambda_t p_{h,t} = 0.
$$

Divide by β^t and rearrange:

$$
p_{h,t} \lambda_t = \beta \big[u_{h,t+1} + \lambda_{t+1} p_{h,t+1} (1 - \delta_h - \tau_h) \big].
$$

5. Replace multipliers with marginal utility of consumption. Since $\lambda_t = u_{c,t}$:

$$
p_{h,t} u_{c,t} = \beta \Big[u_{h,t+1} + u_{c,t+1} (1 - \delta_h - \tau_h) p_{h,t+1} \Big],
$$

which is equation (20).

6. Convert to consumption units. Divide both sides by $u_{c,t+1}$:

$$
p_{h,t} \frac{u_{c,t}}{u_{c,t+1}} = \beta \Big[(1 - \delta_h - \tau_h) p_{h,t+1} + \frac{u_{h,t+1}}{u_{c,t+1}} \Big]
$$

With $u_{c,t} = 1/c_t$ we have $\dfrac{u_{c,t}}{u_{c,t+1}} = \dfrac{c_{t+1}}{c_t}$, so

$$
p_{h,t} = \beta \, \frac{c_t}{c_{t+1}} \Big[(1 - \delta_h - \tau_h) p_{h,t+1} + \frac{u_{h,t+1}}{u_{c,t+1}} \Big].
$$

Substitute $u_{h,t+1}/u_{c,t+1} = \big(\alpha_h A(G_{t+1}) / H_{t+1} \big) \cdot c_{t+1}$:

$$
p_{h,t} = \beta \, \frac{c_t}{c_{t+1}} \Big[(1 - \delta_h - \tau_h) p_{h,t+1} + \alpha_h A(G_{t+1}) \frac{c_{t+1}}{H_{t+1}} \Big]
$$

which is equation (21).

E Road-Quality Vision Model: Fine-Tuning and Evaluation

In this section, we document our pipeline to classify road-surface quality from satellite imagery into three classes: poor (0), medium (1), and high (2). We begin from an ImageNet-pretrained vision model called *ConvNeXt V2*, available on Hugging Face and supported by Meta AI (Woo et al., 2023). We use the ConvNeXt V2 backbone and fine-tune the base version of the model on labeled road imagery from Brewer et al. (2021). The resulting model produces categorical ratings which can be converted into a continuous Road Quality Score (RQS), used in Section 5.1.

Fine-tuning Workflow. Figure 18 summarizes the end-to-end workflow. We begin with a corpus of 53,677 satellite tiles (224x224 pixels) labeled for road quality from Brewer et al. (2021). We feed these images into a ConvNeXt V2 model, replacing the original classification head with a 3-way softmax layer. We then fine-tune the entire model end-to-end using cross-entropy loss and the AdamW optimizer. The training process is monitored via a validation set, and we select the best-performing checkpoint based on minimum validation loss. Finally, we evaluate the fine-tuned model on a held-out test set to report accuracy, F1, and class-wise precision and recall.

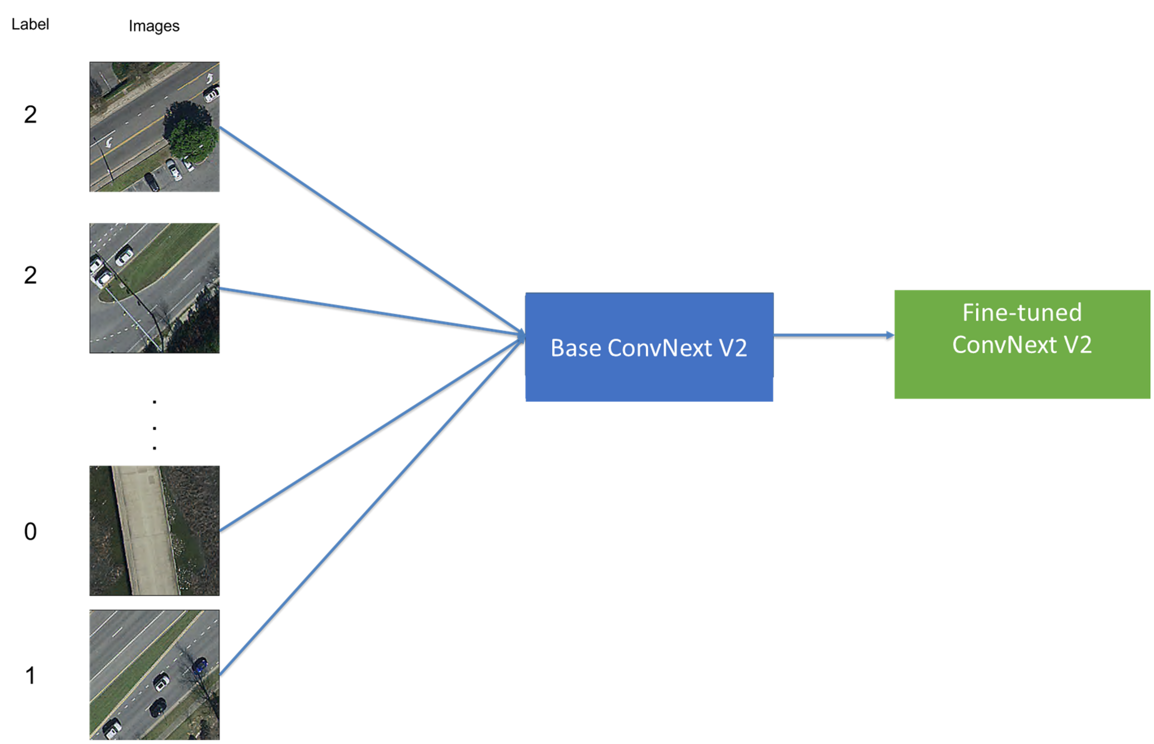


Figure 18: Fine-tuning workflow for road-quality classification

Data and Training. We fine-tune ConvNeXt V2 trained on ImageNet-1k dataset. The classification head is replaced by a 3-way softmax. We fine-tune the model for 3 epochs and use cross-entropy loss, AdamW optimizer with learning rate of 5×10^{-5} and weight decay of 0.01, and a batch size of 16 for training and 32 for evaluation. Inputs use the model's ImageNet normalization; no explicit augmentation. We use a weighted, stratified 70/15/15 split and select the checkpoint with the lowest validation loss.

Split	**Share**	**Stratified**	**Weights**	**Rationale**
Train	70%	By label	3/2/1	Mitigates class imbalance by allocating more minority-class samples to training, improving gradient signal and stability.
Validation	15%	By label	3/2/1	Class-aware monitoring consistent with train; used for model selection (min. val. loss).
Test	15%	By label	3/2/1	Hold-out evaluation with comparable class mix for fair assessment across splits.

Table 15: Split design and class-aware allocation. Weights are used only to allocate per-class counts into splits, *not* as loss weights.

Model Evaluation Metrics. We report accuracy, macro F1, and class-wise precision/recall to account for any residual imbalance. We also provide a confusion matrix to diagnose systematic confusions between adjacent quality classes.

Table 16: Overall accuracy by split (fine-tuned ConvNeXt V2).

Split	Overall Accuracy
Train	0.9115
Validation	0.8525
Test	0.8337

Table 17: Confusion matrix on the held-out Test set (N=1,539). Low/Medium/High correspond to classes 0/1/2.

		Predicted		
		Low	Medium	High
	Low	33	11	9
Actual	Medium	12	78	181
	High	2	41	1172

Table 16 presents model accuracy results on training, validation and test datasets. Accuracy is 0.9115 on train, 0.8525 on validation, and 0.8337 on test, indicating a good fit without overfitting on training data. As expected, accuracy declines slightly from train to

Table 18: Classification metrics on the Test set derived from Table 17.

Class	Precision	Recall	F1	Support
0	0.702	0.623	0.660	53
1	0.600	0.288	0.389	271
2	0.861	0.965	0.910	1,215
Macro avg	0.721	0.625	0.653	1,539
Weighted avg	0.810	0.834	0.810	1,539
Accuracy		0.8337		1,539

validation to test. However, the gap between validation and test is less than 10 percentage points, suggesting good generalization across unseen data.

Table 17 shows the confusion matrix on the held-out test set and is useful to understand the types of errors the model makes when classifying road quality. As presented in the table, most errors are between adjacent classes: medium (1) is often predicted as high (2), and poor (0) occasionally as medium (1). Confusions between poor (0) and high (2) are rare, while high (2) is predominantly classified correctly.

Table 18 presents per-class metrics. Class 2 attains high precision and recall, driving weighted averages near overall accuracy, which suggests the model is very reliable at identifying high-quality roads. Class 1 shows low recall, lowering macro F1 and reflecting difficulty on borderline surfaces. This means the model often misses medium-quality roads, misclassifying them as high quality. Class 0 performs moderately, with balanced precision/recall relative to its smaller support, indicating reasonable reliability on poor-quality roads despite fewer examples. Overall, the model excels at identifying high-quality roads but nevertheless struggles more with medium and poor classes, especially in recall for medium quality.

Base vs. Fine-Tuned ConvNeXt V2. Table 19 summarizes the main gains from end-to-end adaptation. We compare our fine-tuned ConvNeXt V2 against a baseline where the pretrained backbone is frozen and only the classification head is trained. This linear-probe baseline on frozen ImageNet features approach is common in transfer learning.

Table 19: Fine-tuned vs. baseline ConvNeXt V2.

Model	Overall Accuracy
Fine-tuned ConvNeXt V2	0.8337
Baseline ConvNeXt V2	0.2586

Fine-tuning improves test accuracy by 57.51 (from 0.2586 to 0.8337) percentage points, a more than 220% relative gain. This indicates that adapting the pretrained backbone to road imagery is crucial for learning task-specific features and achieving reliable generalization, justifying end-to-end fine-tuning over a frozen baseline.