

# Fiscal policy and economic activity: New Causal Evidence<sup>1</sup>

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## Abstract

Utilizing a quasi-natural experiment design, we identify an exogenous cut in local taxes accompanied by an equivalent reduction in local government spending and estimate the impact of these exogenous changes on income. We exploit a unique regional dataset that combines local income data with local voting outcomes on current expense tax levies. Taxes and the associated spending change abruptly at the 50% vote share cutoff below which a tax levy fails to pass. This cutoff determines which observations serve as controls and which receive treatment: a reduction in local taxes and government spending. Voting percentages around the cutoff are a source of exogenous variation with observations around this quasi-randomly assigned and very similar across characteristics. We find that balanced budget reductions in taxes and spending cause a large drop in local incomes in the first two years after the vote, suggesting that government expenditure effects on income are larger than fiscal revenue effects. The cumulative government spending multiplier of a balanced-budget change in spending for our baseline is a sizeable 1.5. This effect of local tax-financed government spending is prominent in low-income and high-poverty areas, suggestive of mechanisms related to the share of liquidity constrained agents.

**Keywords:** Government spending, taxes, exogenous variation, quasi-natural experiment, regression discontinuity.

**JEL Codes:** E62, H30, H72, R11.

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## 1. Introduction

A large literature in macroeconomics has utilized a variety of approaches to identify exogenous changes in taxes and government spending in order to estimate their impact on income and other economic activity. One line of research uses the narrative approach in order to identify exogenous changes in taxes or government spending as in Romer and Romer (2010) and Ramey (2011)<sup>2</sup>, while another influential approach utilizes structural vector autoregressions and achieves identification by exploiting institutional features of fiscal systems (e.g., Blanchard and Perotti, 2002).

In this paper, we use an alternative quasi-natural experiment approach based on a novel regression discontinuity (RD) design that provides causal evidence for the effects of fiscal policy. We identify an exogenous change in locally tax-financed government spending and estimate its direct impact on income by making use of a unique regional dataset that combines local income data with local election outcomes over time. More specifically, we exploit voting on renewals of tax levies for current expenses of local governments in the state of Ohio to consider an exogenous cut in local property taxes accompanied by an equivalent reduction in local government spending. Taxes and the associated government spending change abruptly at the 50% vote share cutoff below which a tax levy is not renewed. The 50% cutoff determines which observations serve as controls and which receive treatment: a reduction in property taxes and local government spending, with quasi-random variation arising around the cutoff. Thus, voting percentages around the 50% cutoff serve as a source of exogenous variation allowing us to

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<sup>2</sup> The first paper looks at presidential speeches and Congressional reports on major tax policy actions and the latter uses war expenditures.

estimate the impact of a change in taxes and spending on economic activity. As it is highly unlikely that close voting outcomes could be systematically anticipated, our identification does not suffer from the sort of anticipation effects which typically haunt such exercises where outcomes are being anticipated even before a fiscal policy has been implemented. Furthermore, as the timing of renewals is predetermined at the time these tax levies were first introduced five years earlier, considering renewals instead of new tax levies further ensures that these tax levies are not endogenous responses to prevailing economic conditions.

To implement our RD exercise, we utilize data on tax levies for the complete census of cities, villages and townships that vote on current expense tax levies in the state of Ohio between 1991 and 2018.<sup>3</sup> We thus put together a unique data set of over 4,000 votes by Ohio local governments matched to income and demographic characteristics.<sup>4</sup>

The contribution of our paper is thus threefold. First, we use the above-described novel design that exploits regional voting outcomes from a unique dataset that combines local income data with voting on renewals for local government current expense tax levies, in order to identify truly exogenous changes in fiscal policy and estimate their effects. Second, our data and research design allow a direct comparison of fiscal expenditure and fiscal revenue effects within a balanced budget framework. Third, given the specific nature of our data and context, our analysis sheds light on intermediate mechanisms behind the estimated effects.

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<sup>3</sup> Given income data availability, however, our baseline runs over the 2010-2018 period.

<sup>4</sup> Beyond the availability of the appropriate regional tax levy election outcomes and income data for a sufficiently long period, Ohio is an economically and politically representative state of the broader United States and economically important. Its 700 billion \$U.S. GDP in 2020 would rank it 21st in the world if it were a country.

We find that exogenous “balanced budget” reductions in property taxes and spending result in a drop of local incomes evident in the first two years after the tax and spending cut, indicating a larger effect of government expenditure as compared to tax revenues. The cumulative government spending multiplier of a balanced-budget change in spending for our baseline is 1.5, which is sizeable but closely resembles other regional multiplier estimates in the literature (e.g., in Chodorow-Reich et al., 2012; Nakamura and Steinsson, 2014). That the effect of government spending benefiting the broader populace exceeds that of taxes for property owners, suggests a welfare-improving redistribution mechanism is at work when both local taxes and spending increase. Since in our context higher local government spending is financed by higher property taxes, redistribution takes place from wealthier households and firms to poorer households with higher marginal propensities to consume (MPCs) on average, raising local incomes. This effect is present in spite of the fact that a portion of property owners are themselves hand-to-mouth (HtM) as shown by Kaplan et. al (2014).<sup>5</sup> Our story is about redistribution from more wealthy households to poorer ones with a higher marginal propensity to consume. This redistribution occurs because rich households tend to own more expensive property and thus the property-related taxes they pay finance local government spending that potentially benefits households who own property with lower value as well as households that

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<sup>5</sup> This is consistent with MPCs for HtM households without property being higher than the average MPC for property owners. This will be the case if the set of property owners consists of individuals with liquid wealth and low MPCs, and of HtM individuals without liquid wealth and high MPCs. While according to Kaplan et. al (2014) the latter comprise two-thirds of HtM households in the U.S. and have similar MPCs to poor HtM households without property ownership, redistribution from property owners to poor HtM households without property could increase local income as long as these poor HtM households have a higher MPC than the average MPC for property owners. In addition, the poor and other lower income individuals might also own property but of much lower value than the wealthy, thus local government spending would mostly be financed by taxes paid by more wealthy households and thus still induce redistribution towards poor and lower-income individuals.

do not own any property. The point is reinforced by the finding that the increase in income following a locally tax-financed increase in government spending is pre-eminent in areas with lower incomes or higher poverty rates where liquidity constraints and spending multipliers are plausibly higher, suggesting that a mechanism resembling that in Farhi and Werning (2017) is at work. Overall, our findings suggest that lower government spending, even if accompanied by an equivalent cut in taxes, reduces local incomes in a manner consistent with the large kind of effects on economic activity that would be predicted, e.g., by the Farhi and Werning (2017) theoretical framework with liquidity constrained consumers.<sup>6</sup>

An important ongoing debate in the literature lies in the difference between the size of the effects of revenue-versus-expenditure-side measures of fiscal policy. Our paper provides insights into this literature. Papers following the Blanchard and Perotti (2002) approach find small revenue multipliers below one (e.g., Tenhofen et al. (2010) and Gechert (2015)) while those following the narrative approach of Romer and Romer (2010) find significantly larger tax multipliers above two (e.g., Cloyne 2013 or Mertens and Ravn 2014). Our results suggest that the revenue effects are indeed small relative to the effects of government expenditure. The results are in line with Gechert et al. (2021) who find large positive balanced budget effects for changes in the social security system using narrative information to estimate the difference between changes in transfers and contribution rates. Thus, taken together with these recent estimates, our results provide a more complete picture of the overall policy effects.

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<sup>6</sup> Alternatively, Rendahl (2016) shows that fiscal multipliers may be large at the zero lower bound if the labor market is somewhat inertial, while Eggertsson (2010) and Christiano, Eichenbaum, and Rebelo (2011) show fiscal multipliers may be large at the zero lower bound if an increase in government spending sets off an inflationary spiral that lowers the real interest rate. We could not provide evidence for either of these mechanisms, as shown in appendix B.5.

The RD approach used in our paper has been underutilized in the macroeconomic literature on the impact of exogenous changes in fiscal policy on economic activity in great part due to a lack of data, and has only recently been applied to macroeconomic research to identify the effects of fiscal policy (see, e.g., Corbi, Papaioannou and Surico, 2019).<sup>7</sup> As regional income and output data are often unavailable to researchers, the latter paper estimates employment effects and maps these to income based on an assumed production function. Instead, our data allows estimating the impact of fiscal policy on income directly.

Our work is closely related to the literature which studies the impact of government spending on local economic outcomes by exploiting cross-sectional variation as in, e.g., Fishback and Cullen (2013), Acconcia, et al. (2014), Nakamura and Steinsson (2014), Fishback and Kachanovskaya (2015), Serrato and Wingender (2016), and Gabriel, Klein and Pessoa (2021)<sup>8</sup>, and more generally to the literature which aims to shed light on macroeconomic questions using cross-sectional identification. Similar to this body of work, we exploit “quasi-random” variation arising due to cross-sectional differences in order to shed light on the regional impact of government spending and taxes. While this new cross-sectional literature on fiscal multipliers differs in method and scope from the traditional empirical macroeconomics literature relying on time-series variation (e.g., Blanchard and Perotti (2002) and Ramey (2011)) and cannot identify

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<sup>7</sup> Corbi, Papaioannou and Surico (2019) study the impact of municipal expenditures on local labor markets in Brazil; An RD design had also been utilized in Litschig and Morrison (2013) to study the long-term effects of government spending on local levels of education, poverty and income per capita in Brazil, and in Becker, et al. (2010) to study the causal effect of EU structural funds on economic growth in the treated regions.

<sup>8</sup> This recent closely related paper uses regional variation in government spending in the Eurozone to trace out the impact of fiscal policy on output and employment, based on an instrument which identifies the effect of government spending on economic activity by relating the changes in regional government spending to the differential regional exposure to changes in national government spending.

nation-wide effects of policy changes (see, e.g., Nakamura and Steinson (2014)), it allows us to clearly identify the source of variation in government spending and its impact on local outcomes.<sup>9</sup>

The informativeness of cross-sectional studies has, however, been questioned. In his review of this literature, Chodorow-Reich (2019) notes that “much of the pessimism regarding *their* informativeness ... arises because in the vast majority of cases the spending used to identify cross-sectional multipliers does not require higher contemporaneous or future local taxes” as when spending is paid for by the federal government. Thus, in previous work one can observe non-Ricardian effects at the regional level simply because locals do not fully endogenize the future hike in taxes at the federal level needed to pay for the current increase in government spending at the local level, even though such effects could be counteracted by higher net taxes in other regions rendering them uninformative for the overall national level effects of an increase in government spending. Consistent with this, Farhi and Werning (2017, pp. 2460-461) show that “local multipliers estimates that involve increases in government spending that are not self-financed” can be “substantially inflated compared to the counterfactual of self-financed increases in government spending” such as the ones utilized in our application.

Our work contributes to this literature by using locally tax-financed government spending which can influence Ricardian agents whose own spending depends on the present value of the tax burden and agents whose private spending depends on current net income as long as they

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<sup>9</sup> As argued in Serrato and Wingender (2016), “estimates generated by this new literature are informative in their own right as they shed light on intermediate mechanisms and provide answers to important regional policy questions”. Nakamura and Steinson (2014) note that the regional approach has “important advantages” relative to the typical “closed economy” approach using aggregate U.S. data, as relative policy is precisely pinned down across regions with the Fed unable to raise interest rates in some regions relative to others. Thus, regional estimates of the effect of government spending on income are useful in distinguishing between different macro models, a point further elaborated on in Nakamura and Steinson (2018).

share this tax burden, so that there could be an offsetting decline in local income due to the higher taxes. Our finding of large positive income effects of locally tax-financed government spending implies that redistribution to less wealthy households, e.g. those less affected by the property tax, via government spending that benefits the broader populace, raises consumption and income despite the higher local tax. Such mechanisms, if present at the local level, would have major implications for policy and welfare at the national level. That our data allows us to shed light on such mechanisms, further distinguishes our paper from previous work.

Next, we describe our RD design. Section 3 describes our data and preliminary analysis. The fourth section presents some challenges to identification and Section 5 presents our results. The last section briefly concludes.

## 2. Methodology

### 2.1 Model of Regression Discontinuity

Regression discontinuity requires a situation in which a ‘running’ variable takes different values on either side of a cutoff which determines whether agents receive treatment or serve as controls. In its original application, Thistlethwaite and Campbell (1960) studied students who required a certain test score to receive a Certificate of Merit. The power of regression discontinuity comes from selecting the right data to identify a treatment effect estimate; as a result, the formal econometric model is relatively simple. Let the running variable be  $V$  for vote share, the proportion of votes in favor of a tax levy. Let  $c$  represent the cutoff value of  $V$  that determines which observations serve as controls and which receive treatment: a reduction in property taxes and local government spending. Because local property taxes follow a simple



majority rule, 0.50 is our cutoff. Although we will try other outcomes later, initially let outcome  $y$  be median family income in city  $i$ , and let  $t$  index the year of the vote, so that the estimating equation is the following:

$$y_{it+\eta} = \tau D_{it} + \beta V_{it} + \Phi W_{it} + \epsilon_{it}. \quad (1)$$

In Equation (1) the symbol  $D$  is a dummy variable that indicates whether the tax levy fails ( $= 1$ ) or passes ( $= 0$ ), so that  $\tau$  is the treatment effect averaged over all local governments in the sample and all current expense tax levies during the timeframe. The symbol  $t$  indexes the year of the vote, and  $\eta$  indexes years before and after the vote. Positive values of  $\eta$  test for the time it takes for treatment to first appear and for the persistence and rate of decay of an effect. Negative values of  $\eta$  are useful as falsification tests to provide further assurance of the identification of any statistically significant treatment effects found with positive values of  $\eta$ . Regression discontinuity can fully identify treatment effect estimates with only  $D$  and  $V$  as regressors, a point we expand upon in the data section, but it is often useful to add covariates  $W$  to increase the precision of the treatment effect estimates. Finally,  $\epsilon$  is the error term, with the first cumulant equal to zero and the second representing a constant variance.

## 2.2 Bandwidth and Kernel Selection

Ideally,  $\tau$  would be estimated exactly at the cutoff  $c$ , but this is not possible as there are basically no observations exactly at the cutoff, and the observations with  $c = 0.50$  are all failed tax levies. Because we need observations from tax levies that both fail and pass, and because we require sufficient statistical power to identify any treatment effect, it is necessary to estimate  $\tau$  within some bandwidth of  $c$ . The bandwidth  $h$  should be large enough to allow for precisely

estimated treatment effects, but not so wide that the observations on either side of the cutoff start to have different characteristics. Doing so would violate the randomization of agents around the cutoff, invalidating the regression discontinuity design and leading to biased treatment effect estimates.

Historically, researchers chose a bandwidth  $h$  in an ad-hoc manner and tested the sensitivity of estimates to different bandwidths. Imbens and Kalyanaraman (2012) provides an objective way to estimate a bandwidth that minimizes the mean squared error (MSE) of the treatment effect estimator, thereby balancing the need for unbiasedness and efficiency. Armed with a single, optimal bandwidth, there is no need for researchers to arbitrarily try alternative, suboptimal bandwidths. Calonico, Cattaneo, Farrell and Titiunik (2019) shows that when covariates are included, there is bias in the treatment effect estimates obtained using the method of Imbens and Kalyanaraman (2012). For this reason, we use the bias-corrected estimator of Calonico, Cattaneo, Farrell and Titiunik (2019). We estimate  $\tau$  with a triangular kernel because it produces the MSE-optimal estimates (Cattaneo, Idrobo and Titiunik, 2019). We also experiment with four alternative selection procedures to estimate the bandwidth  $h$  as detailed in section 4.5.

### 3. Data and Preliminary Analysis

#### 3.1 Geography and Institutional Details

We study Ohio primarily because it has the data we need. We require tax referenda data at the city level. Ohio is one of the few U.S. states that holds a centralized repository of local election outcome data. More recent data is available online; data from intermediate years is available on spreadsheets; older data used in Section 4.4 is available only in pdf format; data for

1991-1994 (before the server crash of 1995) is available only in paper format. The result is a unique data set of over 4,000 votes by Ohio villages and cities matched to income and demographic characteristics.

Apart from data availability, Ohio is a worthwhile economic entity to study. Its 2020 gross domestic product was 700 billion \$U.S. If it were a nation Ohio would rank 21<sup>st</sup> in the world in terms of GDP, between Switzerland and Poland. Its 2020 population was 11.8 million, making it the seventh most populous U.S. state and larger than all but 78 nations. Ohio has three urban areas with about two million residents each, and three more with about 700,000 residents. There are numerous farming communities and small industrial cities outside of the larger urban areas. Economically, politically and geographically, it is hard to think of a state more representative of the United States.<sup>10</sup>

The Land Ordinance of 1785 established a system to organize the Northwest Territories (including Ohio) into a series of square townships with lengths of six miles on each side. There are 88 counties in Ohio, each with about 15 townships. Each township is governed by a three-person board of trustees. Townships may collect a property tax of up to 0.1 percent of assessed property value without a referendum, called inside millage, and they may levy taxes beyond this limit with voter approval. Citizens may petition to form a village, which may cross township lines. Almost all villages have a mayor and council form of government and may levy an income tax in addition to a property tax. Villages may also collect inside millage, but any income tax or property tax beyond the inside millage must be approved by voters in a simple majority vote. When a

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<sup>10</sup> We note that voters in Ohio must be U.S. citizens of at least 18 years of age who have lived in the state for at least 30 days before the election, must have registered to vote, and must have not been incarcerated for a felony conviction, violated election laws, or been declared incompetent by a court.

village exceeds a population of 5,000, it is classified as a city. There are currently about 1,000 townships, 247 cities, and 680 villages in Ohio.

Local property tax levies in Ohio must specify the purpose of the tax. The type of tax we utilize is the general levy for current expenses. It is a broad category that includes salaries and materials to support services like garbage collection, public safety, public health, air pollution, and the maintenance, operation, and the repair of parks, roads, bridges, and public buildings.<sup>11</sup> If a current expense tax levy fails to renew, funding must be cut, and tax money from specific purpose tax levies cannot by law be used to compensate for the lost funds. For example, funds from a fire tax levy may not be shifted to repair streets. Local government spending on current expenses must be cut. By contrast, if a specific purpose tax like fire services is cut, cities could conceivably shift money from a current expense tax levy to help cover the loss of funding. This is then one reason we prefer to utilize current expense levies as opposed to specific purpose taxes.

When a tax is proposed, it must specify the amount of time the tax is to be collected, the dollar amount to be collected each year, and the tax rate required to collect that amount. The mean current expense tax levy is 0.26% of assessed value. The mean current expense tax levy represents 21% of total current expense spending. By far the most common duration of a tax is five years, representing over 90% of the sample. After five years, when the tax is due to expire, the city will ask voters to renew it. If voters approve, the tax will continue in effect; if voters choose not to renew the tax, the tax is removed and local government spending declines by an equivalent amount. The deterministic nature of voting and funding means sharp RD is more

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<sup>11</sup> Capital expenditures, in contrast, include the purchase and construction of assets that are intended to last more than five years, like building a new park or purchasing a fire truck.

appropriate than fuzzy RD. Finally, it is important to note that since most votes happen in November, we would expect limited economic consequences of a change in funding in the year of the vote. At the same time, a non-trivial fraction of our sample, about a third, concerns votes that took place in May so we would not expect the income for the year to be completely unaffected, which precludes using the concurrent year as a falsification test.

### 3.2 Endogeneity of Votes

Since Cellini, Ferreira and Rothstein (2010) economists have recognized that voting data may not be independent, violating a fundamental assumption of the classical linear regression model. If voters reject a tax levy, the city may come back to voters with some version of the tax proposal until it is approved. The typical strategies for dealing with the non-independence of votes are some form of conditioning on vote history, like using only the first or the largest tax levy in the sample for each city.

Instead, we argue that the timing of *any* new tax levy is endogenously chosen by a city. The timing may be chosen to maximize the probability of passage, perhaps to coincide with favorable economic conditions or to respond to a shock to the city like a new firm relocation or a social shock to the community. We follow the precedent of Brasington (2017) by only considering renewal tax levies.<sup>12</sup> As the timing of renewals is predetermined at the time these tax levies were first introduced five years earlier, considering renewals instead of new tax levies ensures that these tax levies are not endogenous responses to prevailing economic conditions. A

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<sup>12</sup> We “stack” the data, evaluate each levy within its own window, and cluster standard errors at the unit level like Cellini, Ferreira, and Rothstein (2010).

new tax passed in 1999 with a duration of five years will expire in 2004. At that time, the city will ask voters to renew the tax. The timing of a tax vote in 2004 is exogenous to the city, having been set in 1999. If voters vote to renew the tax in 2004, funding will continue as before; but if it is rejected, funding will be cut. To examine the robustness of our approach, in section 4.6 we report results using the Cellini, Ferreira and Rothstein (2010) methodology conditioning on vote history.

### 3.3 Variables Used

#### 3.3.1 Outcome Variable

The primary outcome we study is median family income, a variable the U.S. Census Bureau has tracked since the beginning of our voting data. We use this measure as it can capture the average level of economic welfare in a location relatively well.<sup>13,14</sup> The Census defines a family as a householder and everyone living with them who is related by birth, marriage, or adoption. Its definition of income is fairly comprehensive. It excludes in-kind transfers like housing assistance and food stamps, but it includes earnings, unemployment compensation, worker's compensation, Social Security income, SSI payments, public assistance, veteran's payments, survivor benefits, disability benefits, pension or retirement income, interest and dividends, rents, royalties, estate and trust income, educational assistance, alimony, child support, and financial assistance from outside the household.

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<sup>13</sup> In this, we follow previous work like Litschig and Morrison (2013), Fishback and Kachanovskaya (2015) and Serrato and Wingender (2016).

<sup>14</sup> We complement income with data on local poverty as an outcome variable, to get a better understanding of how government spending affects or interacts with local welfare. Poverty measures the fraction of households with income less than US\$30,000.

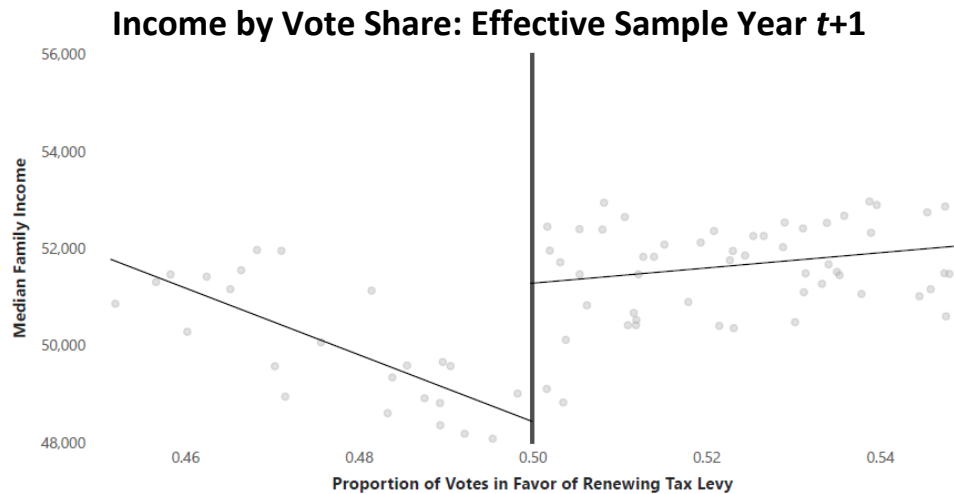
It is proper to measure the outcome variable in levels. While panel data models typically measure a change in the outcome variable between time periods, the regression discontinuity design measures a change across a threshold. Our treatment effect, then, is a change in incomes that results from cutting taxes and spending. Identification in regression discontinuity does not come from differencing out a time-invariant component but by examining otherwise comparable units that differ only in treatment status. First differencing the outcome variable would not help with identification but would instead muddle the interpretation of the treatment effect, making it something like a change between groups in the year-to-year difference in incomes.

Median family income is measured in constant 2010 U.S. dollars. The mean at the time of the vote is \$51,771. Even though they are unadjusted by the running variable, the raw means indicate a difference in incomes between the pass levy and fail levy groups one year after the vote--\$51,828 vs. \$48,480--hinting that voting to cut property taxes may lead to a drop in incomes. This difference is not driven by different economic conditions across groups of cities, because, as we show next, indicators of economic conditions like the unemployment rate are similar across groups. Outside of the effective bandwidth it is entirely likely that differences in economic conditions drive differences in incomes, so that cities that vote 70% in favor of renewing a tax would have higher incomes and cities that vote 40% in favor would have lower incomes, but cities near the 50% cutoff are nearly identical in economic conditions.

It is customary for regression discontinuity studies to graph the outcome variable relative to the running variable. Although it is not a formal analysis, just raw data unadjusted by the running variable, the raw difference in the outcome variable between treatment and control

groups can suggest a treatment effect in the regression results. Figure 1 shows incomes for vote shares near the cutoff one year after the vote.

**Figure 1**



Each dot in Figure 1 is a localized mean within a bin of the running variable. Binning helps present a visually appealing figure, because without binning there would be hundreds of dots cluttering the figure, obscuring any pattern the data might show. The bins are evenly spaced, do not overlap, and help illustrate the variability of the raw data.

One year after the vote, incomes appear to be lower for cities that do not renew current expense tax levies and thus cut taxes and spending relative to those that vote to renew these, with a discrete jump around the 50% voting share cutoff. Regression analysis in Section 4 that controls for the running variable and includes covariates will provide a formal test of what this graph suggests.



### 3.3.2 Covariates

In OLS regression, researchers isolate the independent relationship between the dependent variable and a key explanatory variable by including numerous control variables. This is not the role of covariates in regression discontinuity. Estimation can proceed with only the running variable and a treatment dummy and still be fully identified, and this is the strategy of Dykstra, et al. (2019). Although they are not necessary for identification, it is useful to include covariates to increase the precision of the estimates. The included covariates can be related to the outcome variable, as in least squares, or they can be related to the running variable instead (Lee and Lemieux 2010, p. 293). We utilize a handful of relevant covariates in our application to increase precision.

Table 1 shows covariate means split by cities that renew and cities that cut the property tax. The first covariate is the unemployment rate that measures economic conditions in a city. The next covariate measures the age distribution of a city. % Age 5 to 17 is the proportion of persons aged 5 to 17 in a city, an age group that may be particularly influenced by public services like police protection and parks and recreation so that their parents would be more likely to vote for a levy, which would then lead to correlation of this particular age group share with the running variable (the vote share). Another set of covariates captures educational attainment: % HS Grad Only is the fraction of people whose highest educational attainment is a high school diploma or equivalent, and % No HS Grad is the fraction aged 25 or older who did not graduate high school.

We would expect many of these characteristics to vary between groups of cities that vote to renew or to cut taxes, but what matters for regression discontinuity is the characteristics of the cities within an effective bandwidth of the cutoff. Table 1 shows that the differences are

small, suggesting that these characteristics are comparable near the cutoff between groups of cities. This is a crucial assumption of regression discontinuity that ensures that treatment is as good as randomized around the cutoff, and that the only thing that is different is that one set of cities renews the tax and spending while the other set cuts taxes and spending. The last column of Table 1 shows a statistical difference between some of our covariates, as is common in regression discontinuity studies (e.g., Meyersson, 2014), but the means are similar, and the covariates are shown not to jump abruptly at the cutoff.

There are more variables that could be measured, but, unlike traditional regression analysis, their inclusion would not help with identification and turns out not to help in terms of precision. What's more, the theory of regression discontinuity states that because assignment to treatment is exogenous, conditional on the running variable, both observed and unobserved variables are comparable around the cutoff (Dunning, 2012; Murnane and Willett, 2010). We make this clear in the remaining rows of Table 1 which show variable means of covariates not used in the regressions. Socially, economically, and demographically, the cities are comparable within the effective bandwidth. We show an additional check on the balance of unobserved variables in Section 4.9.

The regression results that follow have a slightly different sample size in each lead and lag period. One reason is that, from time to time, a new village incorporates or disappears from the sample as it dissolves into a surrounding township or is annexed by another local government. Another reason is that the number of tax levies is not constant from year to year. The final reason is that our yearly data set begins in 2010 and ends in 2018. We do not observe an

income value in 2020 for a tax in 2017, and we do not observe one in 2009 for a vote in 2010, so different lead and lag years will drop observations.

**Table 1**  
**Covariate Means by Tax Levy Renewal Status within Effective Bandwidth**

	Failed Levies	Passed Levies	t-test for equal means
<b>Outcome Variables</b>			
Income	47.7	48.6	0.72
Poverty Rate	0.20	0.19	0.40
<b>Baseline Covariates</b>			
Unemployment Rate	0.12	0.09	0.06
% Age 5 to 17	0.22	0.20	0.03
% HS Grad Only	0.45	0.45	0.79
% Separated	0.02	0.02	0.32
% Divorced	0.13	0.13	0.81
<b>Other Covariates Used</b>			
% Under 5	0.07	0.07	0.15
% Single Parent	0.18	0.15	0.11
% Renters	0.30	0.28	0.30
% Married	0.49	0.52	0.05
% No HS Grad	0.17	0.13	0.07
<b>Other Covariates Not Used in the Regressions</b>			
% Bachelors	0.08	0.09	0.47
% Graduate Degree	0.04	0.05	0.46
% Some College	0.26	0.26	0.66
% White	0.93	0.94	0.51
Population (1,000s)	2.9	2.8	0.34
% With Kids	0.47	0.43	0.07
Race Herfindahl	0.91	0.92	0.57
% Hispanic	0.012	0.019	0.07
Labor Force Participation Rate	0.61	0.62	0.76

Notes: Means for all variables are shown at the time of the tax levy vote. There are 1,423 current expense tax levies from 2010 to 2018, 201 of which are in the effective bandwidth for the  $t+1$  regression with values of the running variable between 0.558 and 0.442. Section 5.6 uses the sample of votes between 1991 and 2018, which has 4,509 observations, 890 of which are within the 0.064 effective bandwidth on either side of the cutoff.  $t$ -test for equal means shows  $p$ -value for null hypothesis of equal means between fail and pass levy samples, with alternative hypothesis that the differences are not zero in absolute value.

## 4. Results

### 4.1 Baseline Results

Before showing the effect of changes in government spending, it is useful to verify that our data is picking up changes in government spending. To that end, we begin by estimating Equation (1) using current operating expenditures as an outcome. We cannot do this for every city in our sample, but only for the small sample of locations which have expenditure data in the City County Data Book. We estimate that in the first year after the vote, cities that vote to cut current expense taxes spend \$16 million less on average than cities that vote to renew current expense tax levies,<sup>15</sup> suggesting that our voting data is indeed capturing changes in local government spending. For context, we find a 21% drop in current expense funding on average for cities that vote to cut taxes and current expense spending.<sup>16</sup> We note that since our estimated \$16 million government spending cut following a failed levy vote is based on a small sample of locations for which government expenditures are actually available, it is only a rough approximation and should be taken with caution.<sup>17</sup> These estimates of the change in government

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<sup>15</sup> The  $p$ -value for this estimate is less than 0.01, based on 184 observations for cities with available data. We also estimate that these cities spend 6.4 (2.1) million less in the second (third) year after the vote, with the associated  $p$ -value less than 0.01 (less than 0.05), based on 188 (166) observations. The covariates used are the unemployment rate, % Under 5, % Age 5 to 17, %Bachelors, %Graduate Degree, %Some College, and %White, using a triangular kernel with an “RD” bandwidth selection, local-linear regression, and standard errors clustered at the city level.

<sup>16</sup> This figure comes from taking random samples from the Department of Taxation’s “Tax Year 2018 Property Tax Rate Abstract”.

<sup>17</sup> The mean tax levy for the locations for which government expenditure data is available is 0.34% of assessed value as compared to 0.26% for the complete sample of locations. In addition, the overall levy actually collected would also depend on the total value of local properties which may differ between locations that happen to have government spending data available and those that don’t. Related to this, the limited sample for which we have government spending data which are necessary for calculating a fiscal multiplier, has higher median house prices than the sample we use to estimate treatment effects (\$142,600 vs. \$110,400).

spending following a failed levy vote are however essential in order for us to compute the multiplier for a balanced-budget change in government spending.

Table 2 shows the main results of the paper. Covariates  $W$  are those listed in the Baseline Covariates section of Table 1. The cell in the upper left-hand corner of Table 2 shows a treatment effect of  $-7,020$ , with a  $p$ -value of 0.04 rendering it statistically significant. This means that one year after the vote, cities that fail to renew current expense tax levies have \$7,020 lower median family incomes than cities that successfully renew. Recall from Table 1 that these cities are nearly identical in characteristics. Point estimates with confidence intervals are shown in Figure 2.

**Table 2**  
**Effect on Income of Failing Versus Renewing Current Expense Tax and Spending**  
**in the Years after and before the Vote**

Time Period Relative to Year of Vote	Estimates ( $p$ -values)	Number of Observations
<b><math>t+1</math></b>	<b>-7,020*</b> (0.04)	1,275
<b><math>t+2</math></b>	<b>-7,431*</b> (0.03)	1,142
<b><math>t+3</math></b>	-4,625 (0.20)	957
<b><math>t+4</math></b>	-2,274 (0.58)	801
<b><math>t+5</math></b>	-1,944 (0.62)	640
<b><math>t</math></b>	<b>-8,080*</b> (0.02)	1,422
<b><math>t-1</math></b>	-5,085 (0.16)	1,422
<b><math>t-2</math></b>	-1,458 (0.67)	1,422

Notes: Treatment effect estimate shown with  $p$ -value in parentheses for regressions using the yearly estimates sample from 2010 to 2018. Outcome is median family income in a city in 2010 U.S. dollars, so a  $-7,020$  estimate means that voting to cut taxes for current local government expenses causes a \$7,020 drop in median family incomes the next year relative to cities that renew the tax, for example. Standard 'RD' bandwidth selection option from Calonico, Cattaneo, Farrell and Titiunik (2017) chosen that imposes a common bandwidth  $h$  on either side of the cutoff; triangular kernel used. Covariates are the Baseline Covariates described by Table 1. Estimates are mean squared error-optimal, local linear. Standard errors clustered at city level. \* = statistically significant at 0.05 level.

The  $-7,020$  estimate is a local average treatment effect valid for the set of cities and villages with vote shares close to fifty percent. It tells us that reducing local government spending reduces local incomes significantly, even if reductions in government spending are associated with an equivalent reduction in taxes. The  $-7,020$  estimate occurs on a base of  $\$51,771$ , so the effect is a nearly 14% drop during the first year, following a 21% drop in current expense funding on average for cities that vote to cut taxes and current expense spending. The treatment effect in year two after the vote is similarly high at  $\$7,431$ , as shown in the second row of Table 2. It then shrinks and fails to reach statistical significance in later periods. This is a typical pattern of results for regression discontinuity, and it is reassuring for identification. It would be worrying for identification if a significant treatment effect were found for every period. The results suggest an initial shock to the local economy, with full adjustment occurring two years after the vote.<sup>18</sup>

We calculate a government spending multiplier resulting from a balanced-budget change in local government spending, as the change in local income over the change in local government spending for the first two years after the vote during which there is a statistically significant impact on income. To calculate the change in local income, we multiply the estimated change in median income by the number of households in the average locality in our sample which is 2,343 households. The cumulative estimated impact on median family income of  $\$14,451$  in the first two years after the vote reported in the first two rows of Table 2 then implies a change in local income of  $\$33.86$  million, which divided by the respective estimated drop in government

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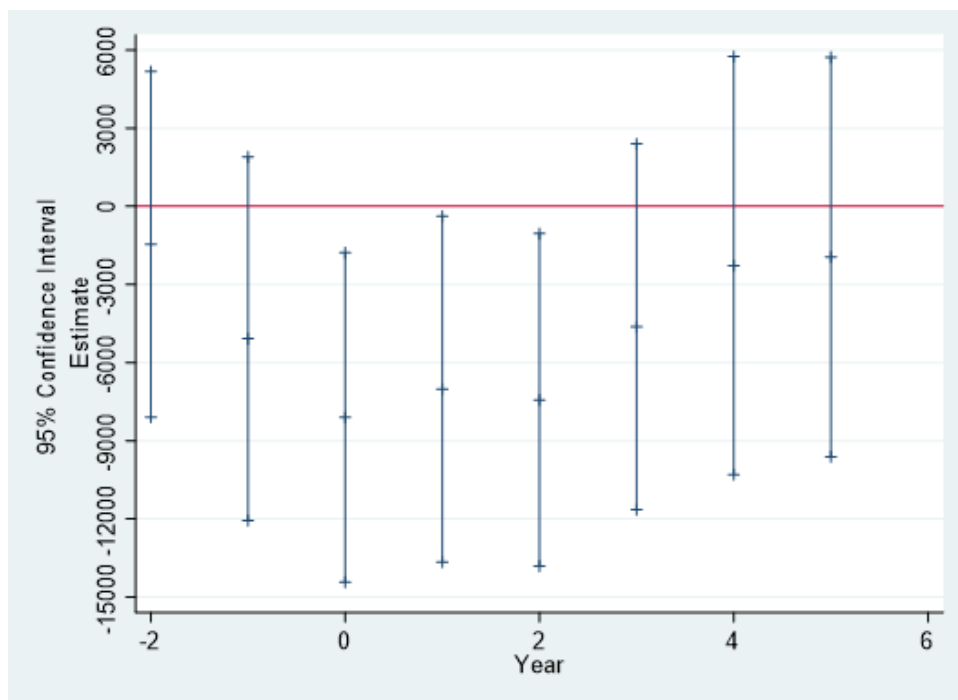
<sup>18</sup> Litschig and Morrison (2013) find no significant long-term effect of government spending on regional income per capita considering outcomes six years after the increase in spending.

spending of \$22.4 million for cities that vote not to renew the tax levy<sup>19</sup>, implies a cumulative government spending multiplier of 1.5 as a result of a balanced-budget change in spending.

The last two rows of Table 2 show that failed levies are not associated with lower (or higher) local incomes one and two years before the vote, suggesting this is not a case of reverse causation driving the results. The importance of these rows is discussed further in Section 4.9, where we also discuss the effect in year  $t$  shown in the third to last row of Table 2.

**Figure 2**

### Change in Income by Year Relative to the Vote



Our findings imply that tax cuts for property owners, which are less likely to benefit poorer individuals with limited property ownership, have a smaller effect than government

<sup>19</sup> We explain how we estimate this drop in government spending after a levy fails to be renewed at the beginning of this section, with further details in footnote 15.

spending which benefits the broader populace. Our findings here are thus suggestive of a redistribution mechanism from wealthier households and firms to poorer individuals with higher propensities to consume on average. This raises local incomes via a demand-induced channel as in Farhi and Werning (2017).

At this point we must examine whether the estimated treatment effect is simply an artifact of how income is defined. On the surface it seems not: while an income tax would factor into wages, a property tax does not, and incomes are defined by pre-tax income. On the other hand, public assistance is part of the definition of income, so the effect on income of cutting current expense funding may simply reflect decreased welfare spending. We investigate this question from many angles and conclude that decreased welfare spending is not responsible for the drop in incomes. The first factor to consider is that the median current expense tax levy is 0.26% of assessed values, which is small relative to the 14.8% average local property tax rate in Ohio (smartasset, 2021), and far lower than the 14% impact on incomes that we estimate. Next, a look at the finances of Ohio cities shows that few of them even engage in local welfare spending. This seems to be confined to the largest seven or so central cities. Even within these cities, the effects are minor. Consider Youngstown, which lists just over \$2 million in “public health and welfare” spending for 2018. Even if we assume that all of this spending was cash transfers to the poor with no health spending, it would still only represent \$34 per person, and that assumes that none of this was in-kind transfers which would not be included in the Census’ definition of income. This is an unfair figure because it assumes all of Youngstown’s population is poor, so we now consider that only the 38% of Youngstown residents below the official poverty



line receive transfers. Per capita transfers would still only be \$90 per person, so even if all public assistance were cut, it would have a minimal effect on average income.

The most generous city we find is Cleveland. Its community development department is tasked with addressing homelessness and “needed public services”, but it also handles the demolition of vacant structures, home repair, land utilization, and construction permits. If all of its \$33 million budget went to cash assistance to the poor and none to housing, and it went to the 35% of residents below the poverty line, and the small cut in current expense funding completely wiped out the community development budget, it would still represent a drop of just \$253 per person compared to the median family income of \$51,771, very far from the effect we estimate. We also have good reason to believe that the drop in income is not a matter of how income is defined because we have access to an alternative income measure for cities and villages (but not townships) from the State of Ohio, which does not count public assistance as income. Running the regressions with this alternative measure shows a significant treatment effect that is no smaller than the estimates reported in Table 2.<sup>20</sup>

## 4.2 Considering the low-income versus high-income sub-samples

In order to further understand the mechanism behind our results, we now investigate if there are differential effects in low-income as compared to high-income cities. To do so, we split the sample into below- and above-median income cities. As we can see in Table 3, the estimated effect of a failed levy is significantly negative for low-income cities but not for high-income ones.

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<sup>20</sup> We prefer the Census measure of median family income as the Ohio measure excludes townships, contains influential observations, and defines income inclusive of corporate profits, making interpretation difficult.

**Table 3**  
**High- and Low-Income Cities**  
**Effect on Income of Failing Versus Renewing Current Expense Tax and Spending**  
**in the Years after the Vote for Cities with Above- and Below-Median Income**

Time Period Relative to Year of Vote	Low-Income Cities		High-Income Cities	
	Estimates ( <i>p</i> -values)	Number of Observations	Estimates ( <i>p</i> -values)	Number of Observations
<b>t+1</b>	-10,759* (0.01)	642	-4,695 (0.35)	634
<b>t+2</b>	-8,738* (0.01)	569	-2,688 (0.62)	574
<b>t+3</b>	-8,358* (0.01)	476	-1,075 (0.86)	482
<b>t+4</b>	2,419 (0.55)	398	-4,215 (0.51)	404
<b>t+5</b>	-1,657 (0.71)	317	7,214 (0.38)	324
<b>t</b>	-3,942 (0.18)	713	-5,798 (0.18)	709
<b>t-1</b>	-3,140 (0.37)	713	-2,976 (0.62)	710
<b>t-2</b>	2,277 (0.56)	713	-3,238 (0.52)	710

Notes: Treatment effect estimate shown with *p*-value in parentheses for regressions using the yearly estimates sample from 2010 to 2018. Outcome is median family income in a city in 2010 U.S. dollars, so a –10,759 estimate means that voting to cut taxes for current local government expenses causes a \$10,759 drop in median family incomes the next year in low-income cities, relative to low-income cities that renew current expense tax levies, for example. Standard ‘RD’ bandwidth selection option from Calonico, Cattaneo, Farrell and Titiunik (2017) chosen that imposes a common bandwidth *h* on either side of the cutoff; triangular kernel used. Covariates included are Unemployment Rate, % Single Parent, % Renters, and % Married. Estimates are mean squared error-optimal, local linear. Standard errors clustered at city level. \* = statistically significant at 0.05 level.

The estimated effect of -10,759 for low-income cities in Table 3 in the first year after the vote is distinctly higher in absolute terms than the average effect for the full sample of localities which, as was shown in Table 2, stands at -7,020. The same is true for the estimated effect of -8,738 in the second year after the vote which exceeds in absolute terms the estimated effect of -7,431 from Table 2. Furthermore, the estimated effect for low-income cities persists at -8,358 even three years after the vote rather than shrinking to insignificance which was the case for the

average effect shown in Table 2. The latter can be evidently attributed to the effect for high-income cities which, as shown in Table 3, is estimated not to be significantly different from zero at one, two and three years following the vote.

We now calculate a government spending multiplier resulting from a balanced-budget change in local government spending, as the change in local income over the change in local government spending for the first three years after the vote during which there is a statistically significant impact on income. The cumulative estimated impact of \$27,855 reported in the first three rows of Table 3 adjusted for the average number of households in a locality implies a change in local income of \$65.3 million, which divided by the respective estimated drop in government spending of \$24.5 million for cities that vote not to renew the tax levy, implies a high cumulative government spending multiplier of 2.66 for low-income cities as a result of a balanced-budget change in spending.

To provide evidence in favor of the liquidity mechanism, we have so far split the sample into locations below and above the median income. It could be argued, however, that even cities below the median income may have relatively few families in the very low-income brackets, where it is more likely that families are liquidity constrained. To provide further evidence for the presence of liquidity constraints, we now consider locations that are in the lowest quartile in terms of income. We present these estimates, along with those for locations in the lowest 40 percent of income, in Table 4 below.

Indeed, the estimates of the effect of a change in government spending on income for the locations in the lowest quartile in terms of income, -12730, -10596 and -12083 in periods  $t+1$ ,  $t+2$  and  $t+3$ , are bigger than the respective estimates (-10759, -8738 and 8358) shown in Table 3 for

locations below the median income while the estimates for locations with the lowest 40% of income fall in between these. Since liquidity constraints are likely to be binding for a larger percentage of households in locations that fall in the lowest quartile of incomes, this is further evidence that the liquidity constraints mechanism is relevant.

**Table 4**  
**Effect on Income of Failing Versus Renewing Current Expense Tax and Spending**  
**in the Years after the Vote for Cities with Especially Low Income**

Time Period Relative to Year of Vote	Lowest 40% Income Cities		Lowest 25% Income Cities	
	Estimates ( <i>p</i> -values)	Number of Observations	Estimates ( <i>p</i> -values)	Number of Observations
<b>t+1</b>	-11,113 (0.01)	514	-12,730 (0.02)	323
<b>t+2</b>	-8,543 (0.04)	460	-10,596 (0.05)	289
<b>t+3</b>	-8,865 (0.03)	380	-12,083 (0.02)	236
<b>t+4</b>	5,556 (0.30)	322	6,218 (0.13)	198
<b>t+5</b>	6,668 (0.16)	254	-9,033 (0.15)	153
<b>t</b>	5,919* (0.02)	570	-719 (0.77)	355
<b>t-1</b>	-3,298 (0.34)	570	3,876 (0.41)	355
<b>t-2</b>	4,245 (0.33)	570	13,988 (0.01)	355

Notes: Treatment effect estimate shown with *p*-value in parentheses for regressions using the yearly estimates sample from 2010 to 2018. Outcome is median family income in a city in 2010 U.S. dollars, so a –12,730 estimate means that voting to cut taxes for current local government expenses causes a \$12,730 drop in median family incomes the next year in low-income cities that cut taxes and services relative to low-income cities that renew their current expense tax levies, for example. Standard ‘RD’ bandwidth selection option from Calonico et. al (2017) is chosen, which imposes a common bandwidth *h* on either side of the cutoff; triangular kernel used. Covariates included are Unemployment Rate, % Single Parent, % Renters, and % Married. Estimates are mean squared error-optimal, local linear. Standard errors clustered at city level. \* = statistically significant at 0.05 level.

The above body of evidence is consistent with liquidity constraints and thus spending multipliers being present in locations with relatively low incomes and non-binding in high-income locations. Our findings could be plausibly explained by binding liquidity constraints resulting in higher marginal propensity to consume for poorer individuals, leading to a greater response of

local consumption and incomes after a fall in local government spending. Since lower property taxes are unlikely to benefit poorer individuals with limited property ownership while government spending benefits the broader populace, an equivalent fall in such taxes would not impact their consumption as much, which can explain why local incomes go down in response to a balanced-budget fall in government spending and why this fall is greater in low-income areas.

Our findings are supportive of models such as that by Farhi and Werning (2017) where liquidity constraints lead to larger marginal propensities to consume for poorer individuals that result in larger multiplier effects of government spending. In the latter paper, redistribution to liquidity-constrained consumers raises total consumption as higher current government spending increases labor income and hence consumption of “hand-to-mouth” consumers that have a higher marginal propensity to consume than unconstrained ones, even when government spending is balanced. In our context, higher local government spending is financed by higher property taxes unlikely to burden poorer individuals with limited property ownership, so that redistribution effectively takes place from wealthier households and firms to poorer individuals with higher propensities to consume on average, raising again local income.

### 4.3 Considering the high-poverty versus low-poverty subsamples

Poverty measures the fraction of households with income less than US\$30,000. Households below the poverty line are more likely to be liquidity constrained, so that we would expect the liquidity constraints mechanism to be more relevant for locations in the top quartile of poverty rates. To offer more direct evidence on the plausibility of the liquidity constraints

mechanism, we now investigate whether the effects differ between high-poverty and low-poverty locations.

**Table 5**  
**Effect on Income of Failing Versus Renewing Current Expense Tax and Spending**  
**in the Years after the Vote for Cities with high versus low poverty rates**

Time Period Relative to Year of Vote	High-Poverty Locations		Low-Poverty Locations	
	Estimates ( <i>p</i> -values)	Number of Observations	Estimates ( <i>p</i> -values)	Number of Observations
<b>t+1</b>	-10,767* (0.05)	289	-6,982 (0.30)	321
<b>t+2</b>	-9,315* (0.03)	238	- -	
<b>t+3</b>	-16,032* (0.01)	171	7,187 (0.32)	273
<b>t+4</b>	-12,340* (0.01)	146	- -	
<b>t+5</b>	-27,012* (0.01)	112	- -	
<b>t</b>	-5,478 (0.09)	349	1,026 (0.84)	342
<b>t-1</b>	-3,992 (0.39)	349	9,455 (0.10)	342
<b>t-2</b>	-6,054 (0.29)	349	10,411 (0.09)	342

Notes: Treatment effect estimate shown with *p*-value in parentheses for regressions using the yearly estimates sample from 2010 to 2018. Outcome is median family income in a city in 2010 U.S. dollars, so a -10,767 estimate means that voting to cut taxes for current local government expenses causes a \$10,767 drop in median family incomes the next year in high-poverty (top 25<sup>th</sup> percentile poverty rate) cities, relative to high-poverty cities that renew current expense taxes and funding, for example. Low-poverty is bottom 25<sup>th</sup> percentile. Blanks ('-') indicate insufficient number of observations to perform regressions. Standard 'RD' bandwidth selection option from Calonico, Cattaneo, Farrell and Titiunik (2017) chosen that imposes a common bandwidth *h* on either side of the cutoff; triangular kernel used. Covariates included are Unemployment Rate, % high school only, % aged 5 to 17, % separated, and % divorced. Estimates are mean squared error-optimal, local linear. Standard errors clustered at city level. \* = statistically significant at 0.05 level.

More specifically, we utilize the poverty rate in each locality to split our sample across localities with “large” poverty (75th percentile which amounts to poverty rates greater or equal to 0.24) versus those with “low” poverty (25th percentile with poverty rates less or equal to 0.09). As can be seen in Table 5, the effect of a balanced-budget change in government spending is

large and significant in high poverty locations but absent in low-poverty locations. This concurs with the results from the previous section on low-income locations as compared to high-income ones, and offers further evidence regarding the plausibility of the liquidity constraints mechanism in inducing such an outcome.

#### 4.4 Robustness: Using the full sample with interpolated income data

Our voting data extends from 1991 through 2018. However, the Census only started providing annual data for income in 2010. Thus, income data before 2010 must rely on the decennial censuses of 1990 and 2000, with linear interpolation in non-Census years. Using only the available annual data (excluding the decennial Census information) is more likely to pick up sharp changes in income and such variation is helpful for identification and for providing statistical power. This is why we opted to use the smaller time-series sample for our baseline reported in Table 2.

Nevertheless, using only annual data from 2010 through 2018 could result in a lack of statistical power to achieve significant treatment effect estimates, as it cuts the voting data sample to 1,276 observations and only 201 observations within the effective bandwidth. While Table 2 suggests that this is not a major concern for our application, ignoring information from the first two decades arguably renders our findings potentially less general and perhaps specific to the last decade of the sample. To investigate the generality of our results over time, in this section we utilize the complete sample of votes since 1991 by including interpolated income data. The estimates for the specifications based on the period 1991 - 2018 which includes the interpolated income data for the early part of the sample, are shown in Table 6.

Table 6 shows that the Census data seems to be picking up the impact on incomes of a vote to cut taxes and services in 1994, for example, but these estimates appear muted, plausibly due to the smoothing introduced by interpolation. The interpolation of income data implies income in  $t+1$  cannot react to a vote occurring in the intercensal years except to the extent that the vote affects income in a census year. Thus, votes occurring early in that decade are less likely to have a large estimated effect on measured income. Indeed, the estimates in Table 6 which include interpolated income data for the first two decades are about half as large as the estimates shown in Table 2 which utilize only annual income data for 2010-2018.

**Table 6**  
**Change in Income after Voting to Cut Current Expense Taxes and Spending**  
**1991 – 2018 Sample**

Time Period Relative to Year of Vote	Estimates ( $p$ -values)	Number of Observations
<b><math>t+1</math></b>	<b>-3,365*</b> (0.05)	4,357
<b><math>t+2</math></b>	<b>-3,857*</b> (0.03)	4,224
<b><math>t+3</math></b>	-2,923 (0.11)	4,039
<b><math>t+4</math></b>	-2,648 (0.15)	3,882
<b><math>t+5</math></b>	-2,990 (0.11)	3,720
<b><math>t</math></b>	<b>-3,495*</b> (0.04)	4,506
<b><math>t-1</math></b>	-2,627 (0.12)	4,506
<b><math>t-2</math></b>	-2,246 (0.15)	4,332

Notes: Treatment effect estimate shown with  $p$ -value in parentheses for regressions using the sample from 1991 to 2018. Outcome is median family income in a city in 2010 U.S. dollars, so a –3,365 estimate means that voting to cut taxes for current local government expenses causes a \$3,365 drop in median family incomes the next year relative to cities that vote to renew, for example. Standard ‘RD’ bandwidth selection option from Calonico, Cattaneo, Farrell and Titiunik (2017) chosen that imposes a common bandwidth  $h$  on either side of the cutoff; triangular kernel used. Covariates are % No HS Grad, % HS Grad Only, Unemployment Rate, % Under 5, and % Age 5 to 17. Estimates are mean squared error-optimal, local linear. Standard errors clustered at city level. \* = statistically significant at 0.05 level.



Indeed, the estimated treatment effect of  $-3,365$ , with a  $p$ -value of 0.05 rendering it statistically significant, in the upper left-hand corner of Table 6 is about half the size of that reported in Table 2 for our baseline specification. Still, this estimate tells us that reducing local government spending reduces local incomes significantly, even if reductions in government spending are associated with an equivalent reduction in taxes. One year after the vote, cities that fail to renew current expense tax levies have \$3,365 lower median family incomes than cities that successfully renew, which on a base of \$51,044, implies around a 7% drop during the first year as compared to a 14% drop in our baseline. Similarly, two years after the vote, cities that fail to renew current expense tax levies have \$3,857 lower median family incomes than cities that successfully renew based on the extended sample, as compared to \$7,431 in our baseline from Table 2. While qualitatively similar to the baseline results, these estimates for the extended sample are different quantitatively than the baseline ones, but this difference is what we would expect given the concerns outlined in the previous paragraph regarding the tendency of interpolated data to mute the effect.

The cumulative estimated impact in the first two years after the vote from Table 6, along with the estimated drop in government spending of \$22.4 million over the same period for cities that vote not to renew the tax levy, imply a cumulative government spending multiplier of 0.76 in this case. This is still sizeable, given that it results from a balanced-budget change in government spending.

## 4.5 Robustness: Alternative bandwidth selection

As  $\tau$  could not possibly be estimated exactly at the cutoff  $c$ , it is necessary to estimate  $\tau$  within some bandwidth of  $c$ , as explained in Section 2. Rather than choosing a bandwidth  $h$  in an ad-hoc manner, so far we have utilized Calonico, Cattaneo, Farrell and Titiunik (2019) to estimate a bias-corrected bandwidth using a triangular kernel that produces the MSE-optimal estimates. Here, we experiment with four additional selection procedures to estimate the bandwidth  $h$ . While the method we have used so far (“RD”, shown in the first column of Table 7) imposes a common bandwidth on either side of the cutoff, the method in the second column, “TWO” allows different bandwidths on either side of the cutoff, “SUM” selects the bandwidth for the sum of the RD and TWO estimates, “COMB1” selects the minimum bandwidth of RD and SUM, and, finally, “COMB2” selects the median bandwidth estimate of RD, TWO, and SUM for each side of the cutoff separately.

All bandwidths shown in Table 7, with the possible exception of the “TWO” bandwidth selection option which gives marginally insignificant results for  $t+1$  with a p-value of 0.08, consistently show that otherwise similar cities that barely vote to cut taxes and spending have lower incomes one and two years after the vote compared to cities that barely vote to renew it. This suggests that our findings are not sensitive to the bandwidth selection used.

**Table 7**  
**Alternative bandwidth selection options**

Year Relative to Vote	Bandwidth Selection Option				
	<u>RD</u>	<u>TWO</u>	<u>SUM</u>	<u>COMB1</u>	<u>COMB2</u>
<b>t+1</b>	-7,020* (0.04)	-5,627 (0.08)	-6,801* (0.04)	-7,033* (0.04)	-6,723* (0.05)
<b>t+2</b>	-7,431* (0.03)	-6,958* (0.03)	-7,204* (0.03)	-7,395* (0.03)	-7,200* (0.03)
<b>t+3</b>	-4,625 (0.20)	-3,053 (0.40)	-4,822 (0.20)	-4,822 (0.20)	-4,868 (0.19)
<b>t+4</b>	-2,274 (0.58)	-1,322 (0.74)	-2,548 (0.56)	-2,548 (0.56)	-2,468 (0.56)
<b>t+5</b>	-1,944 (0.62)	-1,290 (0.75)	-2,111 (0.60)	-2,111 (0.60)	-1,726 (0.67)
<b>t</b>	-8,016* (0.02)	-6,498* (0.04)	-7,437* (0.02)	-8,106* (0.02)	-7,672* (0.02)
<b>t-1</b>	-5,085 (0.16)	-4,137 (0.25)	-4,868 (0.17)	-5,091 (0.16)	-4,928 (0.17)
<b>t-2</b>	-1,458 (0.67)	-1,120 (0.75)	-1,204 (0.70)	-1,458 (0.67)	-1,126 (0.74)

**Notes:** Mean squared error-optimal bandwidths estimated with triangular kernels using the following bandwidth selection options, from Calonico et al. (2017): RD imposes a common bandwidth on either side of the cutoff; TWO allows different bandwidths on either side of the cutoff; SUM selects the bandwidth for the sum the of RD and TWO estimates; COMB1 selects the minimum bandwidth of RD and SUM; and COMB2 selects the median bandwidth estimate of RD, TWO, and SUM for each side of the cutoff separately. Standard errors clustered at city level. Estimates use local linear point estimates with a squared term for the bias correction bandwidth. The covariates used are the Baseline Covariates in Table 1. The number of observations for each lead and lag are 1,275 for t+1; 1,142 for t+2; 957 for t+3; 801 for t+4; 640 for t+5; and 1,422 for t, t-1, and t-2.

#### 4.6 Robustness: Conditioning on vote history as in Cellini et al. (2010)

As mentioned in Section 3.2, most prior studies address the endogeneity of votes by conditioning on vote history as in Cellini et al. (2010). We believe our use of renewal tax levies addresses additional sources of endogeneity, but in this section we use our renewal tax sample and additionally condition on vote history as a robustness check. Specifically, for example, if the City of Cheviot votes to cut current expense taxes and spending in 2003 but passes a tax for

additional current expense spending in 2005, we drop it from our estimates for years  $t+2$  through  $t+5$ .

**Table 8**  
**Conditioning on vote history**

Time Period Relative to Year of Vote	Estimates ( $p$ -values)	Number of Observations
$t+1$	-7,584* (0.04)	1,295
$t+2$	-8,993* (0.01)	1,159
$t+3$	-5,474 (0.13)	969
$t+4$	-637 (0.88)	812
$t+5$	-2,651 (0.55)	643
$t$	-8,006* (0.02)	1,445
$t-1$	-4,965 (0.16)	1,445
$t-2$	-1,281 (0.71)	1,443

Notes: Treatment effect estimate shown with  $p$ -value in parentheses for regressions using the sample from 2010 to 2018. Outcome is median family income in a city in 2010 U.S. dollars, so a -7,584 estimate means that voting to cut taxes for current local government expenses causes a \$7,584 drop in median family incomes one year after the vote relative to the set of cities that renews its taxes, for example. Standard 'RD' bandwidth selection option from Calonico, Cattaneo, Farrell and Titiunik (2017) chosen that imposes a common bandwidth  $h$  on either side of the cutoff; triangular kernel used. Covariates are the Baseline Covariates described by Table 1. Estimates are mean squared error-optimal, local linear. Standard errors clustered at city level. \* = statistically significant at 0.05 level.

The results shown in Table 8 are qualitatively similar to those for our baseline shown in Table 2: a balanced-budget fall in government spending has a statistically significant negative effect on income that persists for two periods. Quantitatively, this impact is somewhat larger

than in the baseline, -7584 in Table 8 as compared to -7,020 in Table 2 for  $t+1$  and -8,993 in Table 8 as compared to -7,431 in Table 2 for  $t+2$ .

#### 4.7 Alternative outcome variables: Using mean instead of median family income

In some nations home ownership is less common than renting. Using median family income may be a flawed measure because the typical family might not be directly subject to the property tax. The homeownership rate in Ohio has exceeded 66% since at least 1984, so that families with median income in a locality would typically pay property taxes. However, we still find it useful to assess the robustness of our results to using mean instead of median income. Given the fairly high Gini coefficient of the U.S., the use of mean income allows more influence from the upper tails of the income distribution. The estimates shown below in Table 9 imply once again that mean income falls as the tax levy and associated government spending fall, and are comparable to (but larger in absolute terms than) our baseline results in Table 2 (-7020 at  $t+1$  and -7431 at  $t+2$ ) but with somewhat lower statistical significance (a  $p$ -value of 0.08 instead of 0.04) for the first year after the vote.

**Table 9**  
**Effect of cutting current expense spending and taxes on mean income**

Time Period Relative to Year of Vote	Estimates ( <i>p</i> -values)	Number of Observations
<b>t+1</b>	-7,350 (0.08)	1,274
<b>t+2</b>	<b>-8,939*</b> <b>(0.03)</b>	1,141
<b>t+3</b>	-2,688 (0.58)	955
<b>t+4</b>	4,603 (0.44)	800
<b>t+5</b>	1,916 (0.72)	639
<b>t</b>	-6,448 (0.11)	1,421
<b>t-1</b>	-4,840 (0.39)	1,249
<b>t-2</b>	980 (0.87)	1,079
Notes: Treatment effect estimate shown with <i>p</i> -value in parentheses for regressions using the sample from 2010 to 2018. Outcome is mean family income in a city in 2010 U.S. dollars, so a –8,939 estimate means that voting to cut taxes for current local government expenses causes a \$8,939 drop in mean family incomes two years after the vote relative to renewing the taxes, for example. Standard ‘RD’ bandwidth selection option from Calonico, Cattaneo, Farrell and Titiunik (2017) chosen that imposes a common bandwidth <i>h</i> on either side of the cutoff; triangular kernel used. Covariates are % HS Grad Only, % Some College, % Age 65+, % Other Race, % Divorced, % Separated, Income Heterogeneity, and Tax Levy Size. Estimates are mean squared error-optimal, local linear. Standard errors clustered at city level. * = statistically significant at 0.05 level.		

#### 4.8 Alternative outcome variables: Using poverty instead of median family income

Next, we discuss experimentation with the Census’ poverty rate measure as the outcome variable. As poverty is the opposite of higher income, finding a positive treatment effect on poverty would corroborate the negative treatment effects found for income. Because covariates are only used to increase the precision of estimates, it is fair to consider a different set of covariates for a different outcome variable, as often done in the RD literature. We find the use

of % Separated and % Divorced—both found in Table 1—provides the best improvement in terms of precision. The results are reported in Table 10.

**Table 10**  
**Effect on Poverty Rate of Failing Versus Renewing Current Expense Tax and**  
**Spending in the Years after and before the Vote**

Time Period Relative to Year of Vote	Estimates ( <i>p</i> -values)	Number of Observations
<b>t+1</b>	0.08* (0.02)	1,276
<b>t+2</b>	0.06* (0.05)	1,143
<b>t+3</b>	-0.02 (0.74)	958
<b>t+4</b>	-0.04 (0.30)	802
<b>t+5</b>	0.001 (0.98)	641
<b>t</b>	0.05 (0.08)	1,423
<b>t-1</b>	0.03 (0.19)	1,423
<b>t-2</b>	0.03 (0.20)	1,423

Notes: Treatment effect estimate shown with *p*-value in parentheses for regressions using the yearly estimates sample from 2010 to 2018. Outcome is poverty rate in percentage points, so a 0.08 estimate means that voting to cut taxes for current local government expenses causes an eight-percentage point increase in the poverty rate relative to renewing the taxes, for example. Standard 'RD' bandwidth selection option from Calonico, Cattaneo, Farrell and Titiunik (2017) chosen that imposes a common bandwidth *h* on either side of the cutoff; triangular kernel used. Covariates used are %Separated and %Divorced. Estimates are mean squared error-optimal, local linear. Standard errors clustered at city level. \* = statistically significant at 0.05 level.

For the first and second years after the vote, we find significant treatment effect estimates at the five percent level. These estimates suggest that cities that barely vote to cut taxes and local government spending experience an eight-percentage point rise in poverty rates in the year after the vote and six percent rise in the second year after the vote, as compared to otherwise similar cities that barely vote to renew spending. Our estimates here resemble

qualitatively but appear bigger than those in Litschig and Morrison (2013) who, using regional data for Brazil and a RD design, find that after an increase in government spending local poverty rates decline by 4 percentage points. Overall, our results clearly suggest a rise in poverty in addition to a drop in incomes in the first two years following a failed levy.

## 4.9 Falsification Tests

We now examine the same outcome variable before the votes occur instead of after the vote. In general, if a vote in 2003 is found to be related to incomes in 2002 or 2001, it would suggest that an imbalance in some unobserved factor between pass and fail groups was causing the treatment effect, and could be the true cause of the significant treatment effects in subsequent years. This unobserved factor could be an imbalance in an unobserved covariate. Whatever the source, testing the data for divergent pre-trends between groups is a powerful test of whether confounding factors are responsible for the treatment effects. The results are shown in the final two rows of Tables 2 through 10. They show no statistical significance for years  $t-1$  or  $t-2$  (except in a single case in Table 4 for locations in the lowest quartile of income that shows an effect at  $t-2$  of the opposite sign than our treatment effect, which is thus less of a concern) suggesting that our finding of an effect on income for the years after the vote is not driven by confounding factors or reverse causality.

If the assumptions of regression discontinuity hold, treatment for current expense tax levies should be randomized around the cutoff. If for some reason treatment for current expense tax levies is randomized but there is systematic passage of, say, police tax levies for the pass or fail group, it may be police tax levy passage that drives the difference in income in periods  $t+1$



and  $t+2$ . But in this case the imbalance in police tax levy passage should also exist in periods  $t-1$  and  $t-2$  and cause a significant treatment effect. It is in fact not present. It would also exist in periods  $t+3$  through  $t+5$  producing significant treatment effects in Table 2, but there is no statistical significance for these years, either.

Finally, it is useful at this point to discuss the treatment effect estimates for year  $t$ , the year of the vote, as yet another potential falsification test. About one-third of the votes occur in May and tax collections sometimes start the same year so we would not expect the income for the year to be unaffected, which precludes using the concurrent year as a falsification test. It is also possible that the effect for year  $t$  once the vote has failed could contain anticipation effects regarding the negative future impact of a current drop in government spending, which would then affect current decisions and outcomes.<sup>21</sup> These considerations can explain the significance and size of the effects shown in some of the tables for year  $t$ . At the same time, given that most votes are in November, year  $t$  cannot serve as an ideal outcome year as reflected in Tables 2 to 10 which sometimes show significant treatment effects for year  $t$  and sometimes do not.

#### 4.10 Do localities with larger homeowner shares exhibit a negative effect on income following a tax levy failure?

Our story, based on our findings so far, is about redistribution from more wealthy households to poorer ones with a higher marginal propensity to consume, which occurs because

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<sup>21</sup> Such anticipation effects would be the greatest in size for year  $t$  as after a levy vote fails, individuals might anticipate large negative effects in years  $t+1$  and  $t+2$  which could affect their actions and outcomes in year  $t$ , but would be absent for year  $t+2$  as the effects in years  $t+3$  onwards are zero and would be rational for forward looking individuals to anticipate zero future effects in our baseline shown in Table 2.

rich households own more expensive property and thus the property-related taxes they pay finance local government spending that may potentially benefit households who own lower value property as well as those that do not own any property. To explore the possibility that redistribution occurs solely from property owners to those that do not own a home, we acquired the share of owner-occupied housing in each locality for each year in order to investigate whether localities with a larger share of homeowners still exhibit a negative effect on income following a tax levy failure.

We investigate the effect of renewing versus cutting current expense tax levies on median incomes while adding an interaction term between the fraction of homeowners in a locality and the treatment effect dummy  $D$  that indicates a failed tax levy. Using the votes within the 0.064 bandwidth around the cutoff, the effective bandwidth for the  $t+1$  baseline regressions, we find that the direct effect of a fall in taxes and government spending on income is negative, with  $p$ -values less than 0.01 for year  $t+1$  after the vote, less than 0.05 at  $t+2$ , and equal to 0.068 at  $t+3$ . The effect of the interaction with the share of homeowners is positive and statistically significant with  $p$ -values less than 0.01 for year  $t+1$ , and less than 0.05 for years  $t+2$  and  $t+3$  after the vote. The total effect of a fall in taxes and spending on income, evaluated at the mean share of homeowners is negative, with median family income falling after a fall in taxes and local government spending, but turns positive above homeowner shares of 0.82 for  $t+1$  and 0.88 for  $t+2$ . The total effect on median family incomes is insignificant at the 5% level for year  $t+3$ . The interaction effect we estimate implies that while localities with a higher share of homeowners still exhibit a negative effect on income following a tax levy failure, this estimated effect falls in absolute size as the share of homeowners rises. As cutting taxes and government spending

reduces incomes (but less so) for localities with higher homeowner shares, we conclude that the answer to the question posed in the title of this subsection is affirmative, as long as the homeowner share is not too high.

#### 4.11 Spillovers

To examine the presence of spillovers we examine the effect of a city's votes to cut taxes and current expense funding on median family incomes in the county. As shown in Table 11 below, there is no evidence of spillovers from city level votes to median family income at the county level.

Furthermore, since it is possible that production factors relocate in response to a localized spending shock, e.g. moving across the borders of the cities where tax levies are renewed, we consider population as an outcome variable following a levy vote. If present, this could lead to another source of spillovers across localities. However, as shown in Table 12, population size is not affected by the outcome of the levy vote one to five years after voting occurs. This suggests that this potential source of spillovers is absent in our data.

**Table 11**  
**The effect of cutting taxes & spending on county-level median family incomes**

Time Period Relative to Year of Vote	2010-2018 Sample		1991-2018 Sample	
	Estimates ( <i>p</i> -values)	Number of Observations	Estimates ( <i>p</i> -values)	Number of Observations
<b>t+1</b>	-744 (0.80)	1,298	-414 (0.85)	4,409
<b>t+2</b>	-2,999 (0.30)	1,163	-863 (0.69)	4,274
<b>t+3</b>	752 (0.82)	975	-292 (0.89)	4,086
<b>t+4</b>	537 (0.87)	820	-205 (0.93)	3,931
<b>t+5</b>	1,142 (0.77)	654	-189 (0.93)	3,764
<b>t</b>	256 (0.94)	1,445	-69 (0.98)	4,558
<b>t-1</b>	574 (0.85)	1,445	-191 (0.93)	4,558
<b>t-2</b>	119 (0.97)	1,445	-274 (0.90)	4,383

Notes: Treatment effect estimate shown with *p*-value in parentheses for regressions using the yearly estimates sample from 2010 to 2018 and for the 1991 to 2018 sample. Outcome is median family income in the county in which a city holds a vote in 2010 U.S. dollars, so a -744 estimate would mean that voting to cut taxes for current local government expenses causes a \$744 drop in median family incomes in the county the next year (if it were statistically significant). Standard 'RD' bandwidth selection option from Calonico, Cattaneo, Farrell and Titiunik (2017) chosen that imposes a common bandwidth *h* on either side of the cutoff; triangular kernel used. Covariates included are the Baseline Covariates from Table 1. Estimates are mean squared error-optimal, local linear. Standard errors clustered at city level.

**Table 12**  
**Population as outcome variable: examining relocation effects**

Time Period Relative to Year of Vote	Estimates ( <i>p</i> -values)	Number of Observations
<b>t+1</b>	-2,234 (0.21)	1,275
<b>t+2</b>	-2,353 (0.22)	1,142
<b>t+3</b>	-1,462 (0.47)	957
<b>t+4</b>	-1,144 (0.51)	801
<b>t+5</b>	-116 (0.91)	640
<b>t</b>	-1,345 (0.39)	1,422
<b>t-1</b>	-671 (0.64)	1,422
<b>t-2</b>	-463 (0.75)	1,422

Notes: Treatment effect estimate shown with *p*-value in parentheses for regressions using the sample from 2010 to 2018. Outcome is population in a city, so that a -2,234 estimate would indicate 2,234 fewer people in the city one year after voting to cut current expense taxes and funding (if it were statistically significant), relative to cities that successfully renew their tax levies. Standard 'RD' bandwidth selection option from Calonico, Cattaneo, Farrell and Titiunik (2017) chosen that imposes a common bandwidth *h* on either side of the cutoff; triangular kernel used. Covariates are the Baseline Covariates described by Table 1. Estimates are mean squared error-optimal, local linear. Standard errors clustered at city level.

## 5. Conclusion

Using a unique dataset that combines local income data with local election outcomes on renewals of current expense tax levies, we identify exogenous variation in government spending and estimate its impact on local economic outcomes. We find that a fall in local government spending associated with an equivalent cut in local taxes, reduces incomes at the city level during the first two years following the vote. This drop in incomes is prominent in low-income locations where the effect is distinctly larger in absolute terms and lasts longer, up until the third year following the vote, but absent in high-income locations where liquidity constraints are less likely

to be binding. The effect is also absent in low-poverty locations but prominent and persistent in high-poverty areas where liquidity constraints are most likely to bind.

Overall, our findings imply that lower government spending, even if accompanied by an equivalent cut in taxes, reduces local incomes in a manner consistent with large effects of local government spending on economic activity. In terms of policy implications, this suggests that financing local government spending with relatively non-distortionary property taxes pays off in terms of incomes for the local economy.

The large impact of levy renewals in our baseline and the large “open economy” regional multiplier estimated in Nakamura and Steinsson (2014) are analogous to closed economy aggregate multipliers for a more accommodative monetary policy than has typically been in place for the US. In particular, Nakamura and Steinsson (2014) show that “the open economy relative multiplier is exactly the same as the aggregate multiplier in a small open economy with a fixed exchange rate”, implying that the large estimate of 1.5 for the open economy multiplier in the latter paper and in Acconcia, et al. (2014) and similarly large regional multiplier estimates of 1.5 in our baseline and of about 2 in Chodorow-Reich et al. (2012), Serrato and Wingender (2016), Shoag (2016), and Fishback and Kachanovskaya (2015)<sup>22</sup>, are consistent with the much lower existing estimates of the closed economy aggregate multiplier in previous work and comparable to the large estimates in Ilzetki et al. (2013) for countries with fixed exchange rate regimes.<sup>23</sup> Our results are also in line with the meta-analysis in Gechert and Rannenberg (2018), who find

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<sup>22</sup> The latter authors estimate “an added dollar of federal spending in a state increased state per capita income by between 40 and 96 cents”.

<sup>23</sup> They estimate, based on data from 44 countries, a multiplier of 1.5 for countries in a fixed exchange rate regime and a much lower multiplier for those in a flexible regime.

that tax multipliers are smaller than spending multipliers and that the size of the multiplier difference is 1 to 1.5.

Our approach and the resulting estimates are, in the spirit of Serrato and Wingender (2016), “informative as they shed light on intermediate mechanisms” and can be useful in distinguishing between different macroeconomic models, as argued in Nakamura and Steinsson (2018). More specifically, our estimates of a large impact of government spending on regional incomes even when this spending is balanced, are supportive of models where demand shocks can have large effects on economic activity and where trade openness or liquidity constraints affect the transmission of government spending within a New-Keynesian framework, such as Nakamura and Steinsson (2014), Farhi and Werning (2017) or Kara and Sin (2018).<sup>24</sup>

In particular, our findings are consistent with models where the presence of liquidity constraints is associated with a larger marginal propensity to consume and larger multiplier effects of government spending, as in the Farhi and Werning (2017) New-Keynesian theoretical setting with liquidity constraints. There, higher current government spending raises consumption of liquidity constrained consumers who have a relatively high marginal propensity to consume, even when government spending is balanced.<sup>25</sup> Indeed, in our data, higher government spending raises local incomes even when it is funded by a hike in local taxes within a balanced budget framework, presumably via such a consumption-related channel. In our context, higher local government spending is financed by higher property taxes unlikely to burden poorer individuals

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<sup>24</sup> The New-Keynesian model with liquidity constraints in Kara and Sin (2018) implies a cumulative spending multiplier of 1.6.

<sup>25</sup> The Farhi and Werning (2017) setup in the section “Hand-to-Mouth in a Liquidity Trap” in pages 2451-54 includes two concurrent terms regarding the effect of a reduction of current taxes or an increase in government spending on income where “the sum of the concurrent terms is not exactly zero ... even when government spending is balanced.”

with limited property ownership so that redistribution effectively takes place from wealthier households and firms to poorer individuals with higher propensities to consume on average, raising local incomes in a manner reminiscent of that in Farhi and Werning (2017).

That this increase in income following a locally tax-financed increase in government spending is prominent in localities with lower incomes or higher poverty rates, further suggests that a mechanism similar to that in the latter theoretical framework is at work. That is, given that liquidity constraints and spending multipliers are expected to be higher in low-income or high-poverty areas, the last finding is plausibly explained by binding liquidity constraints for poorer individuals resulting in higher marginal propensity to consume, which would then lead to a greater response of local consumption and local incomes after an increase in government spending in these areas. Our findings are thus very much in line with New-Keynesian macroeconomic models that incorporate liquidity-constrained agents and heterogeneity in income and wealth.



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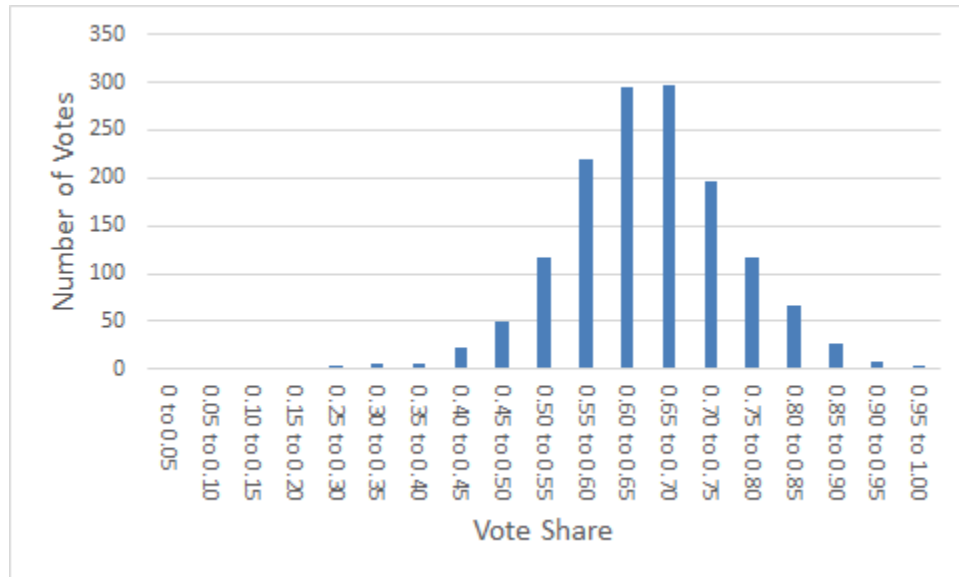
Thistlethwaite, D. L., & Campbell, D. T. (1960). "Regression-discontinuity analysis: An alternative to the ex post facto experiment." *Journal of Educational Psychology*, 51(6), 309.

## Appendix

### A. Challenges to Identification

#### A.1 No Precise Control

In the context of the Thistlethwaite and Campbell (1960) paper, one might be concerned that a teacher might give an extra point or two to his or her favorite students whose test scores fall just shy of the cutoff. In this case, assignment to treatment would not be fully randomized so that any treatment effect might be biased. For our purposes, one might be concerned that some agent with access to voting ballots like the county board of elections might be able to change a few votes so that a community that was going to cut taxes instead renews. This precise control of the running variable would result in a discrete jump in vote share at the cutoff. A McCrary (2008) density test is the traditional way to assess this possibility, but we employ the density test of Cattaneo, Jansson and Ma (2018) because it has better size and power properties, and it requires fewer tuning parameters. It yields a  $p$ -value of 0.94, failing to reject the null hypothesis of no discontinuity in vote share around the 0.50 cutoff.

**Figure A1: Histogram of Vote Share**

The histogram of vote shares in Figure A1 allows readers to visualize if there is an unusual pattern to voting. Vote share seems to follow a fairly normal distribution. The relative paucity of data around the 0.50 cutoff could have implications for statistical power, and it could affect the generalizability of the results, but it does not appear that the distribution of vote share is being manipulated.

## A.2 Covariate Discontinuity

The table of covariate means across groups helps verify that the cities within a narrow bandwidth of the cutoff are comparable. Still, a covariate can have similar means but jump discontinuously at the cutoff. Regression discontinuity requires that the only variable that jumps discontinuously is whether an agent receives treatment. If covariate values exhibit discontinuities at the cutoff, it is possible that the treatment effect is capturing the effect of covariate discontinuities and not exclusively a difference in tax renewal and spending cuts. To guard against

this possibility, we first perform a seemingly unrelated regression as suggested by Lee and Lemieux (2010). The dependent variables in this system of equations are the Baseline Covariates listed in Table 1, with the running variable and the treatment dummy as regressors. We test whether the estimate for the treatment effect is jointly zero. The resulting chi-squared test statistic of 7.8 has a  $p$ -value of 0.17, indicating the covariates do not jump discontinuously at the cutoff. We next graph each covariate as a function of the running variable, shown in Figure A2 shown below. The graphs do not suggest any discontinuity.

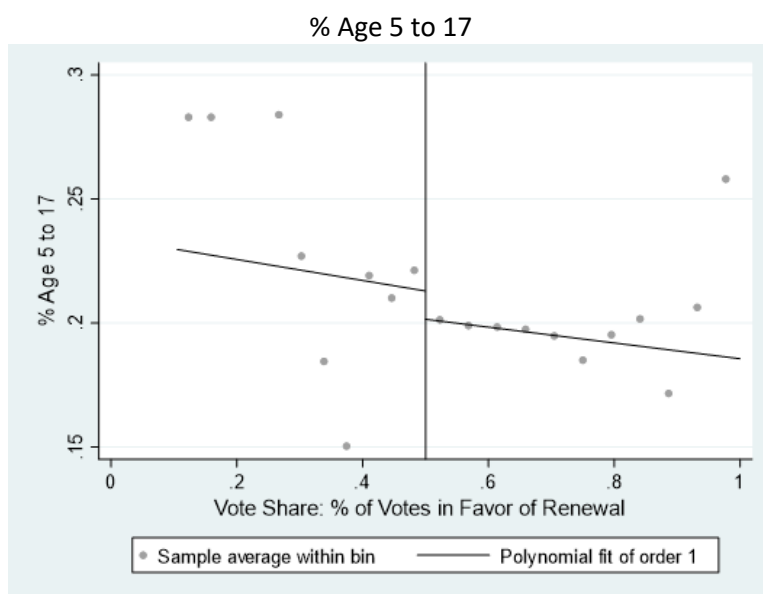
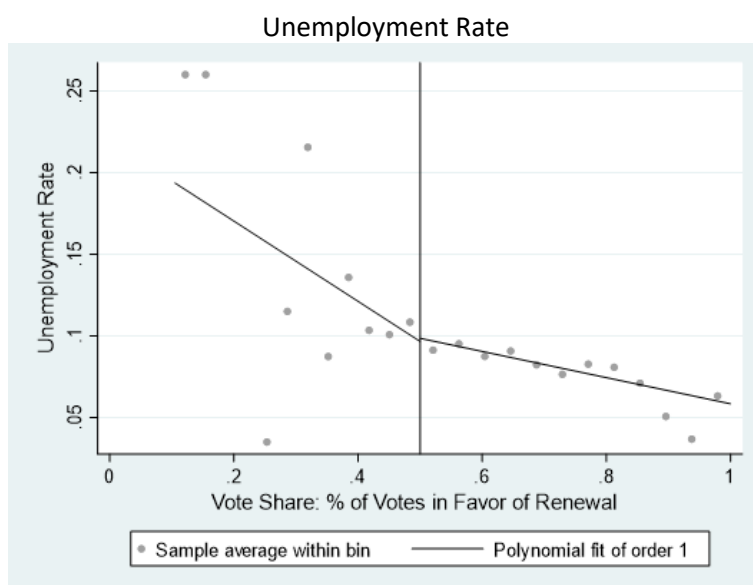
### A.3 Placebo Cutoff

Neither the running variable nor the covariates exhibit a discontinuity at the cutoff. Nevertheless, any discontinuity of incomes at the fifty percent cutoff could be due to random chance. To guard against this possibility, we re-estimate the treatment effect using false cutoffs that are outside of the optimal bandwidth. When we pretend that the cutoff is 0.45, 0.575, 0.6, 0.65, or 0.7 we find no significant treatment effects, suggesting that the discontinuity at 0.50 stems from the vote and not from randomness in the data.<sup>26</sup>

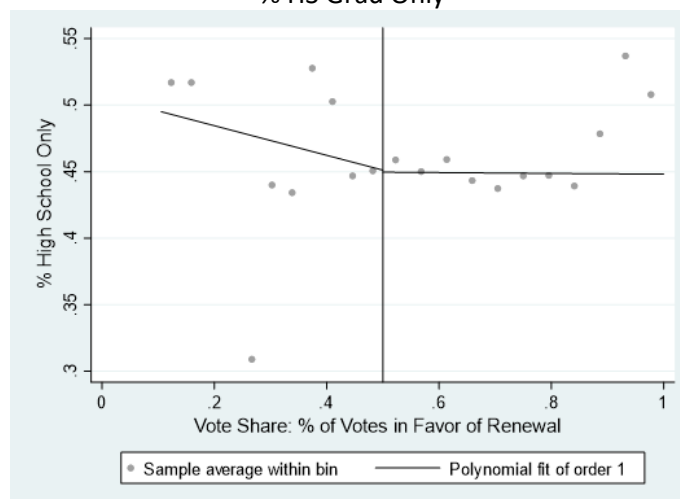
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<sup>26</sup> The  $p$ -values for the placebo cutoff tests are as follows for years 1 through 5 after the vote. 0.45 cutoff: 0.54, 0.37, 0.27, 0.78, and 0.14. 0.575 cutoff: 0.48, 0.68, 0.71, 0.50, and 0.23. 0.60 cutoff: 0.94, 0.61, 0.25, 0.92, and 0.60. 0.65 cutoff: 0.55, 0.35, 0.27, 0.09, and 0.77. 0.70 cutoff: 0.35, 0.28, 0.60, 0.32, and 0.50.

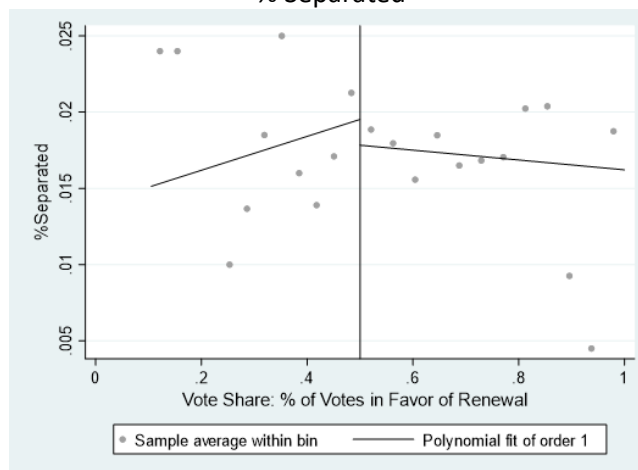
**Figure A2**  
**Graphs of Covariate Smoothness at the Cutoff**



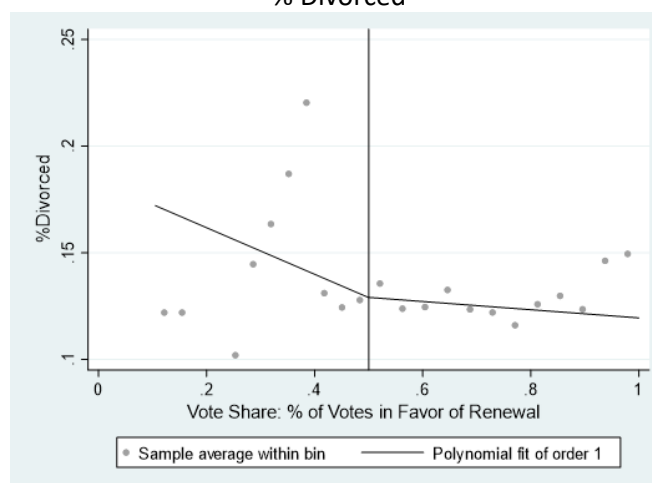
% HS Grad Only



% Separated

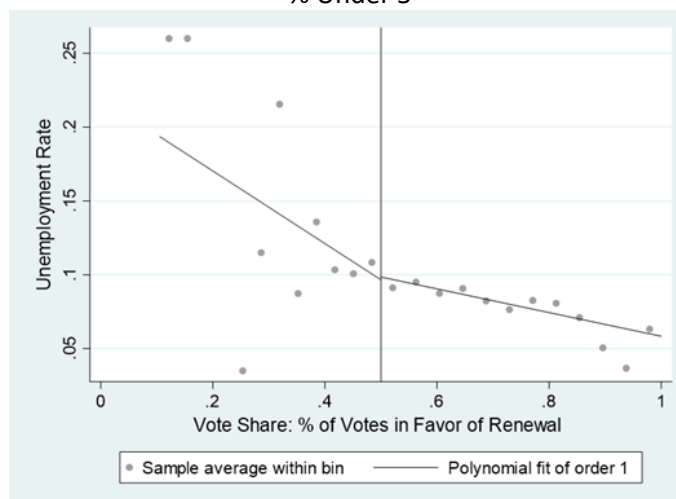


% Divorced

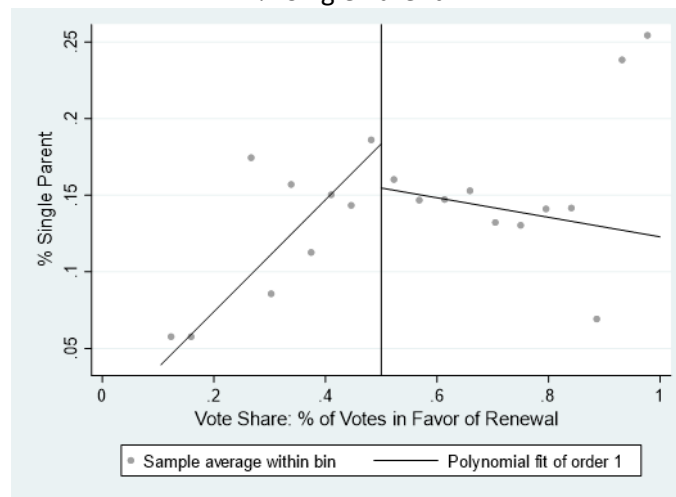




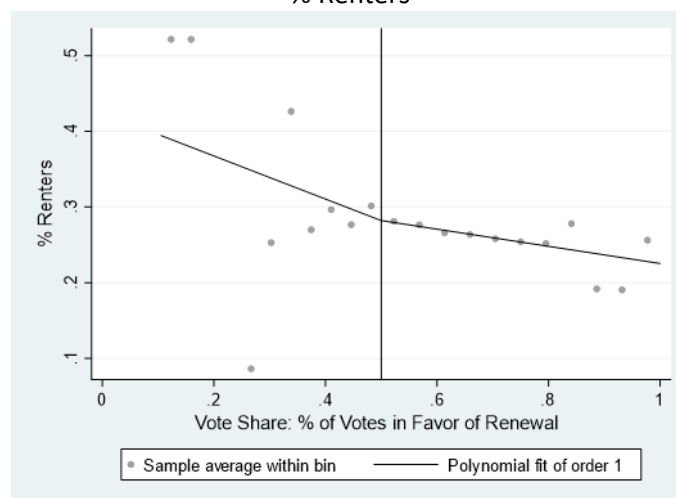
% Under 5



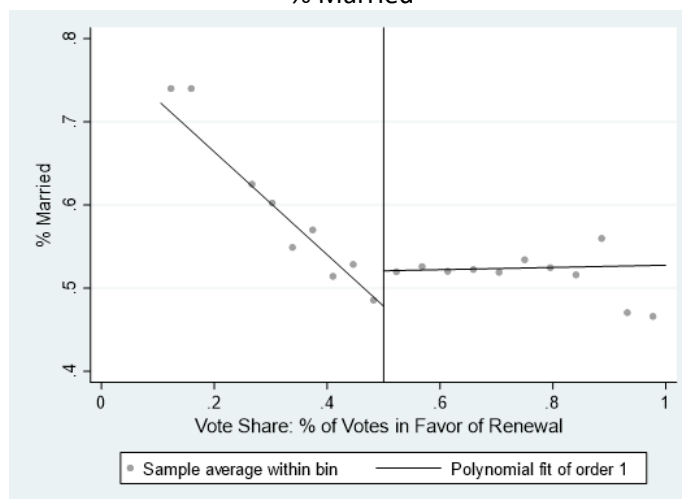
% Single Parent



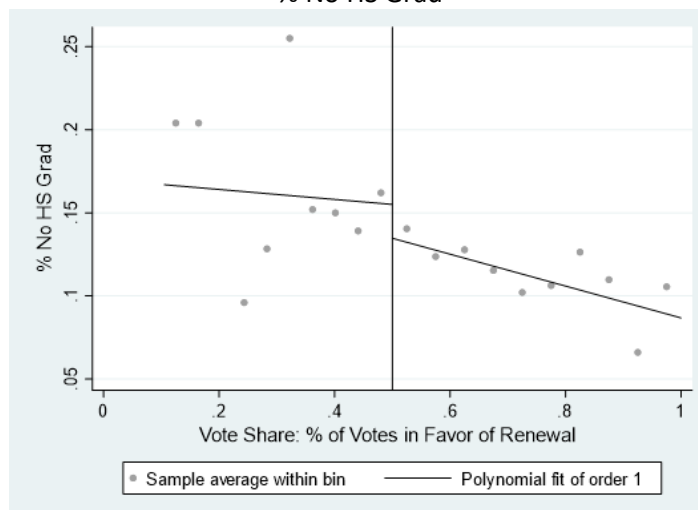
% Renters



% Married



% No HS Grad



## B. Additional Estimates

### B.1 Omitted Variables

To further test whether other types of tax levies are affecting incomes, we collect the set of fire, police, and recreation specific purpose tax levies and rerun the regressions responsible for Tables 2 and 5. Once again, we find no link between voting on such tax types with income levels or poverty rates. The evidence thus suggests that an imbalance in omitted factors is not causing the treatment effects we observe.

Year Relative to Vote	Fire Tax Levies		Recreation Tax Levies		Police Tax Levies	
	Effect on Poverty Rate	Effect on Median Income	Effect on Poverty Rate	Effect on Median Income	Effect on Poverty Rate	Effect on Median Income
$t+1$	-0.008 (0.56)	-1,393 (0.56)	0.016 (0.31)	-1,444 (0.62)	-0.023 (0.17)	-5,063 (0.11)
$t+2$	-0.006 (0.66)	-2,104 (0.42)	0.016 (0.25)	-1,319 (0.62)	-0.021 (0.20)	-4,497 (0.15)
$t+3$	-0.018 (0.26)	-334 (0.92)	0.026 (0.08)	-3,990 (0.13)	-0.022 (0.23)	-3,155 (0.27)
$t+4$	0.001 (0.94)	-2,300 (0.47)	0.020 (0.20)	-4,745 (0.11)	-0.013 (0.40)	-2,425 (0.40)
$t+5$	0.0002 (0.99)	-490 (0.89)	0.020 (0.19)	-1,796 (0.44)	-0.011 (0.47)	1,455 (0.68)
$t$	-0.0005 (0.98)	-2,564 (0.37)	0.007 (0.65)	-859 (0.76)	-0.015 (0.42)	-5,112 (0.11)
$t-1$	-0.006 (0.63)	-1,929 (0.52)	0.020 (0.13)	-1,257 (0.66)	-0.010 (0.59)	-4,177 (0.19)
$t-2$	-0.003 (0.84)	-2,624 (0.38)	0.017 (0.21)	-237 (0.94)	-0.013 (0.48)	-4,257 (0.18)

**Notes:** Treatment effects of voting to cut taxes and services on Poverty Rate and median family income in a city in 2010 U.S. dollars, with  $p$ -values in parentheses below. For example, the -0.008 estimate means cutting fire taxes and services decreases the poverty rate by almost one percentage point one year after the vote (if it were statistically significant), relative to cities the renew the taxes. Standard 'RD' bandwidth selection option from Calonico, Cattaneo, Farrell and Titiunik (2017) chosen that imposes a common bandwidth  $h$  on either side of the cutoff; triangular kernel used. Estimates are mean square error-optimal, local linear. Covariates for fire and recreation tax levies are % not high school graduates and % high school graduates; police tax levies add the unemployment rate, % less than five years old, and % age 5 to 17. Default three nearest neighbors used to adjust standard errors. Number of observations for year  $t+1$  through  $t-2$ : Fire tax levies 8312, 7987, 7760, 7345, 7021, 8638, 8638, 8438; recreation tax levies 830, 787, 760, 721, 690, 854, 854, 831; police tax levies 689, 657, 620, 585, 545, 719, 719, 706.

## B.2 Using logged dependent variable as robustness check

There is extensive heterogeneity across time and cities in our dataset, which might affect the precision of the estimates. In this section we use the natural log of median family income as our outcome variable in order to alleviate this potential concern. The results support our main finding of a large fall in income following a fall in taxes and the associated government spending.

Time Period Relative to Year of Vote	Estimates ( $p$ -values)	Number of Observations
<b>t+1</b>	<b>-0.14*</b> (0.04)	1,275
<b>t+2</b>	-0.12 (0.06)	1,142
<b>t+3</b>	-0.08 (0.22)	957
<b>t+4</b>	-0.02 (0.77)	801
<b>t+5</b>	-0.02 (0.75)	640
<b>t</b>	<b>-0.15*</b> (0.02)	1,422
<b>t-1</b>	-0.08 (0.20)	1,422
<b>t-2</b>	-0.03 (0.63)	1,422

Notes: Treatment effect estimate shown with  $p$ -value in parentheses for regressions using the sample from 2010 to 2018. Outcome is mean family income in a city in 2010 U.S. dollars, so a -8,939 estimate means that voting to cut taxes for current local government expenses causes a \$8,939 drop in mean family incomes two years after the vote relative to cities that renew the tax, for example. Standard 'RD' bandwidth selection option from Calonico, Cattaneo, Farrell and Titiunik (2017) chosen that imposes a common bandwidth  $h$  on either side of the cutoff; triangular kernel used. Covariates are % HS Grad Only, % Some College, % Age 65+, % Other Race, % Divorced, % Separated, Income Heterogeneity, and Tax Levy Size. Estimates are mean squared error-optimal, local linear. Standard errors clustered at city level. \* = statistically significant at 0.05 level.

### B.3 Using Ohio's income measure instead of U.S. Census measure

The analysis in the paper uses median family income from the U.S. Census Bureau as its outcome variable. This measure only has yearly estimates starting in 2010, although it includes the township form of local government. We have an alternative measure of incomes at the municipality level from the State of Ohio Department of Taxation's Tax Data Series Municipal Income Tax files. This alternative measure includes per capita income for villages and cities, but excludes townships. On the other hand, it gives yearly estimates from 1991 through 2018, the first and last years for which we have voting data.

<u>Bandwidth Selection Option</u>					
Year Relative to Vote	<u>RD</u>	<u>TWO</u>	<u>SUM</u>	<u>COMB1</u>	<u>COMB2</u>
$t+1$	-0.52 (0.02)	-0.40 (0.04)	-0.52 (0.02)	-0.52 (0.02)	-0.52 (0.02)
$t+2$	-0.47 (0.01)	-0.34 (0.02)	-0.48 (0.01)	-0.47 (0.01)	-0.47 (0.01)
$t+3$	-0.34 (0.04)	-0.21 (0.08)	-0.32 (0.03)	-0.34 (0.04)	-0.32 (0.03)
$t+4$	-0.13 (0.51)	-0.06 (0.67)	-0.15 (0.44)	-0.15 (0.44)	-0.15 (0.42)
$t+5$	-0.25 (0.18)	-0.17 (0.25)	-0.27 (0.17)	-0.27 (0.17)	-0.27 (0.15)
$t$	-0.44 (0.03)	-0.35 (0.04)	-0.42 (0.03)	-0.44 (0.03)	-0.42 (0.03)
$t-1$	-0.13 (0.58)	-0.11 (0.48)	-0.13 (0.57)	-0.13 (0.58)	-0.13 (0.57)
$t-2$	0.01 (0.98)	-0.07 (0.69)	-0.00 (0.99)	-0.00 (0.99)	0.01 (0.99)

**Notes:** Treatment effect estimates shown with  $p$ -values in parentheses below. Local average treatment effect is the effect of not renewing a current expense tax levy on the natural log of Per Capita Income in a city in years after the vote, relative to voting to renew tax funding. Mean squared error-optimal bandwidths estimated with triangular kernels using the following bandwidth selection options, from Stata's `rdrobust` command of Calonico et al. (2017): RD imposes a common bandwidth on either side of the cutoff; TWO allows different bandwidths on either side of the cutoff; SUM selects the bandwidth for the sum the of RD and TWO estimates; COMB1 selects the minimum bandwidth of RD and SUM; and COMB2 selects the median bandwidth estimate of RD, TWO, and SUM for each side of the cutoff separately. Default covariance structure used which uses at least three nearest neighbors to construct the variance-covariance matrix. Estimates use local linear point estimates with a squared term for the bias correction bandwidth. Covariates used are % no high school diploma, % high school diploma only, % some college but no degree, % non-White population, % never married, % separated, % divorced, labor force participation rate, % with children, unemployment rate, and % renters. Number of observations for each lead and lag: 1,263 for  $t+1$ ; 1,230 for  $t+2$ ; 1,208 for  $t+3$ ; 1,170 for  $t+4$ ; 1,143 for  $t+5$ ; 1,259 for  $t$ ; 1,292 for  $t-1$ ; and 1,221 for  $t-2$ .

The Ohio income measure is different than that of the U.S. Census Bureau. The Ohio measure reflects wages from residents, non-residents who work in the city, and the net profits of corporations domiciled in the city attributable to activities within the city. Income includes wages, salaries, and lottery winnings but, unlike the Census measure, it excludes benefits and transfer payments like pensions, alimony, and child support. Our regressions use the natural log of this measure of income.

Upon request, we can supply interested readers with additional details about the testing of the validity of the regression discontinuity approach for this outcome variable, but suffice it to say that it passes the same tests performed in the main body of this paper. On the other hand, as mentioned in Footnote 18, the data contains influential observations.

Above are the results of cutting taxes and spending for current expenses on the natural log of per capita income. Additional testing shows a significant dose-response: the results below are driven by large (high rate) tax levies.

#### B.4 Examining spillovers related to the prevalence of renters

Our results could also be sensitive to the prevalence of renters in the city, if homeowners are not residents of the respective locality so that the tax incidence partly shifts away from the locality and potential multiplier effects of the tax cut materialize in other jurisdictions. We note that if the above channel was important in our data, to the extent that renters do not benefit directly from a drop in the tax levy we would expect a higher share of renters in a locality to be associated with a more negative effect on income following a reduction in tax rates and an associated fall in government spending, as renters do not directly benefit from the former but stand to lose from the latter.

We thus consider an interaction term between the percentage of renters in a city and the treatment effect dummy  $D$  (indicating a failed tax levy). We find that the direct effect of a fall in taxes and government spending is negative and that accounting for the interaction with the share of renters, the total effect evaluated at the mean share of renters is also negative for  $t+1$  and  $t+2$ . We do not find evidence that the drop in localities with a higher share of renters is greater in absolute terms, which suggests that the channel discussed in the previous paragraph is not dominant in our data.

## B.5 Potential mechanisms

Effect of Cutting Taxes and Spending on Unemployment Rate

Time Period Relative to Year of Vote	All Years 1991-2018		Zero Lower Bound 2009-2015	
	Estimates ( <i>p</i> -values)	Number of Observations	Estimates ( <i>p</i> -values)	Number of Observations
<b><i>t</i>+1</b>	0.008 (0.21)	4357	0.021 (0.12)	1120
<b><i>t</i>+2</b>	0.001 (0.85)	4224	-0.009 (0.67)	1120
<b><i>t</i>+3</b>	0.003 (0.68)	4039	0.002 (0.92)	1118
<b><i>t</i>+4</b>	0.005 (0.49)	3882	-0.00003 (0.99)	962
<b><i>t</i>+5</b>	0.006 (0.49)	3720	-0.004 (0.87)	801
<b><i>t</i></b>	0.006 (0.43)	4506	0.004 (0.79)	1120
<b><i>t</i>-1</b>	0.0004 (0.95)	4506	-0.010 (0.54)	1120
<b><i>t</i>-2</b>	-0.003 (0.71)	4332	-0.004 (0.74)	1120

Notes: Treatment effect estimate shown with *p*-value in parentheses for regressions using two different timeframes: all the years we have available and the 2009-2015 zero lower bound years. Outcome is unemployment rate in a city, so a 0.021 estimate means that voting to cut taxes and current local government expenses causes a two percentage points increase in the unemployment rate the next year relative to cities that renew taxes and spending (if it were significant). Standard 'RD' bandwidth selection option from Calonico, Cattaneo, Farrell and Titiunik (2017) chosen that imposes a common bandwidth *h* on either side of the cutoff; triangular kernel used. Covariates are included as elsewhere in order to improve estimation efficiency. Estimates are mean squared error-optimal, local linear. Standard errors clustered at city level. \* = statistically significant at 0.05 level.

Cutting taxes and current local government expenses during the zero lower bound period causes a marginally insignificant 2.1 percentage points increase in the unemployment rate the next year relative to cities that renew taxes and spending, as compared to a statistically insignificant 0.8 percentage points increase for the 1991-2018 period. It should be noted that the marginal insignificance in period *t*+1 during the zero-lower bound (a *p*-values of 0.12) might be related to the relatively smaller number of observations available in this case. At the same time, there is clearly no evidence in these data regarding any persisting impact on the unemployment rate in periods *t*+2 to *t*+5 following a vote to cut local taxes and local government spending.

### Effect of Cutting Taxes and Spending on the Rate of change of House Prices

Time Period Relative to Year of Vote	All Years 1991-2018		Zero Lower Bound 2009-2015	
	Estimates ( <i>p</i> -values)	Number of Observations	Estimates ( <i>p</i> -values)	Number of Observations
<b><i>t</i>+1</b>	-0.072 (0.65)	488	0.03 (0.90)	158
<b><i>t</i>+2</b>	-0.037 (0.84)	493	-0.32 (0.25)	139
<b><i>t</i>+3</b>	0.006 (0.97)	490	- -	- -
<b><i>t</i>+4</b>	0.123 (0.43)	492	- -	- -
<b><i>t</i>+5</b>	0.156 (0.11)	466	- -	- -
<b><i>t</i></b>	0.040 (0.82)	479	0.23 (0.06)	161
<b><i>t</i>-1</b>	0.073 (0.64)	473	0.14 (0.07)	156
<b><i>t</i>-2</b>	-0.040 (0.80)	467	-0.63 (0.01)	162

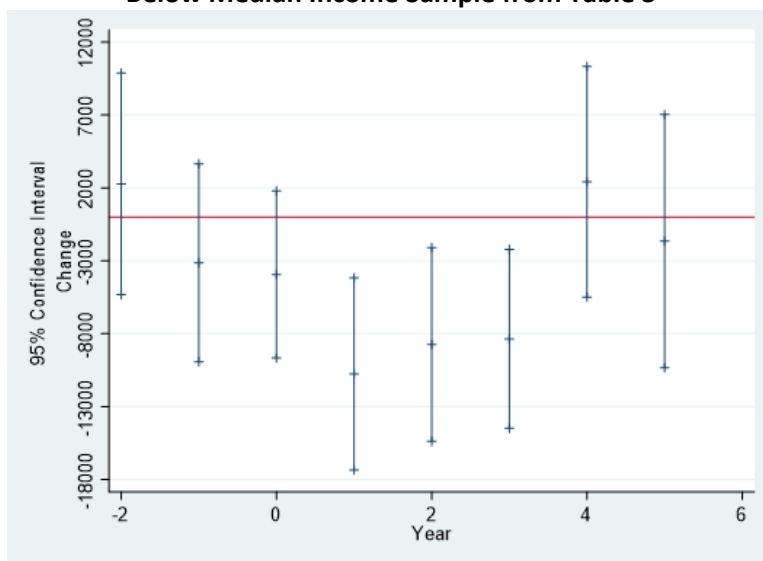
Notes: Treatment effect estimate shown with *p*-value in parentheses for regressions using two different timeframes: all the years we have available and the 2009-2015 zero lower bound years. Outcome is change in the natural log of median house prices in a city, so a -0.072 estimate means that voting to cut taxes for current local government expenses would cause a 7.2 percent drop in nominal house prices the next year if it were statistically significant, relative to cities that renew taxes and spending. The “-” symbols indicate that there was an insufficient number of observations to compute treatment effect estimates, given that we had to take the median sale price per city-year, and some cities are small enough not to have housing sales in each year, and oftentimes a city does not have a vote in a given year, and the votes it does have often pass, leaving few city-years with failed renewals with which to compare with successful renewals. Standard ‘RD’ bandwidth selection option from Calonico, Cattaneo, Farrell and Titiunik (2017) chosen that imposes a common bandwidth *h* on either side of the cutoff; triangular kernel used. Covariates included are the baseline covariates from Table 1. Estimates are mean squared error-optimal, local linear. Standard errors clustered at city level. \* = statistically significant at 0.05 level.

There is no significant impact on the rate of change of house prices following a vote to cut local taxes and spending in periods *t*+1 and *t*+2. It should be noted, however, that the number of observations available in this case is extremely small for the reasons explained in the notes in the Table above, especially so during the zero lower bound period. This renders estimation impossible for periods *t*+3 to *t*+5 after the vote during the zero lower bound period. Furthermore, the significant impact on our outcome variable here for period *t*-2 before the vote, casts some doubt upon the appropriateness of the regression discontinuity estimation approach in this particular case.

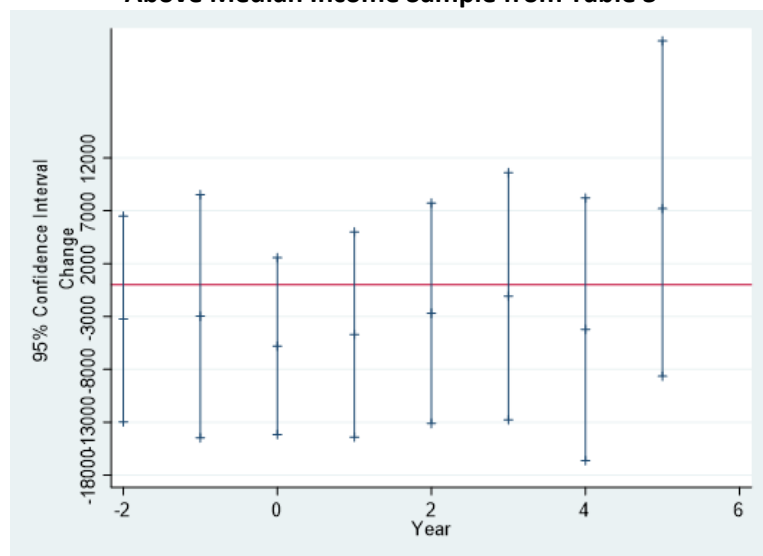


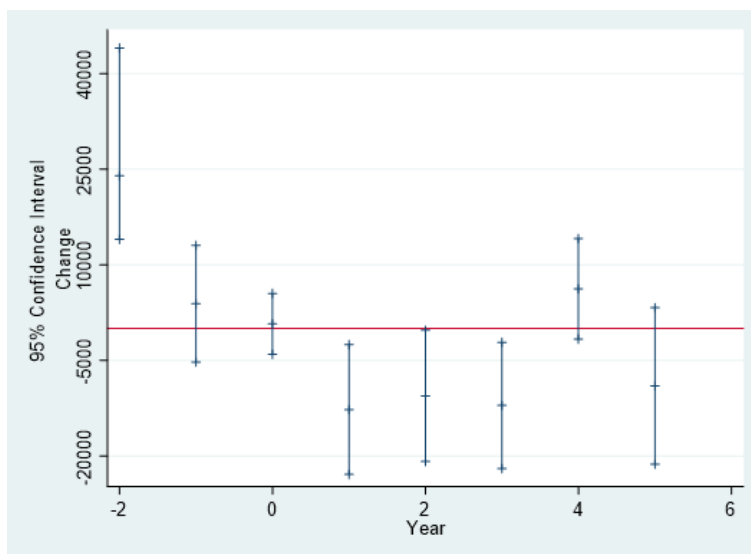
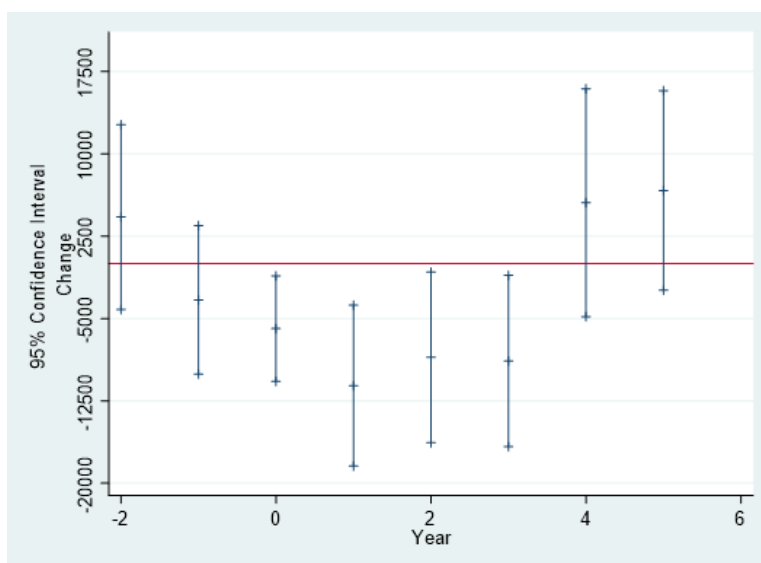
## B.6 Confidence Interval Figures for Ancillary Regressions

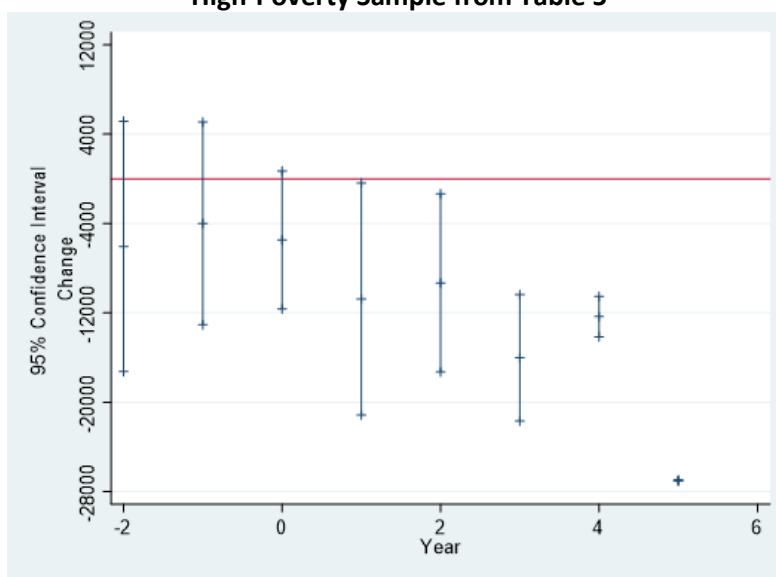
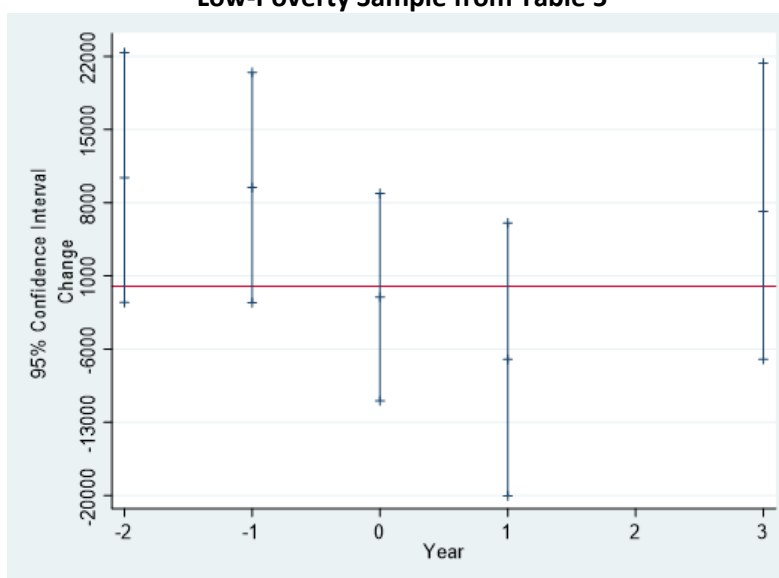
**Below Median Income Sample from Table 3**

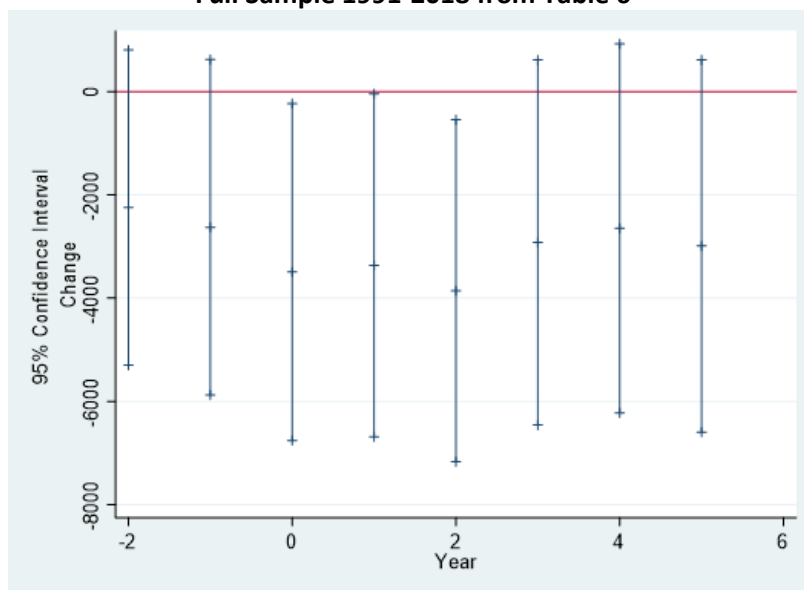
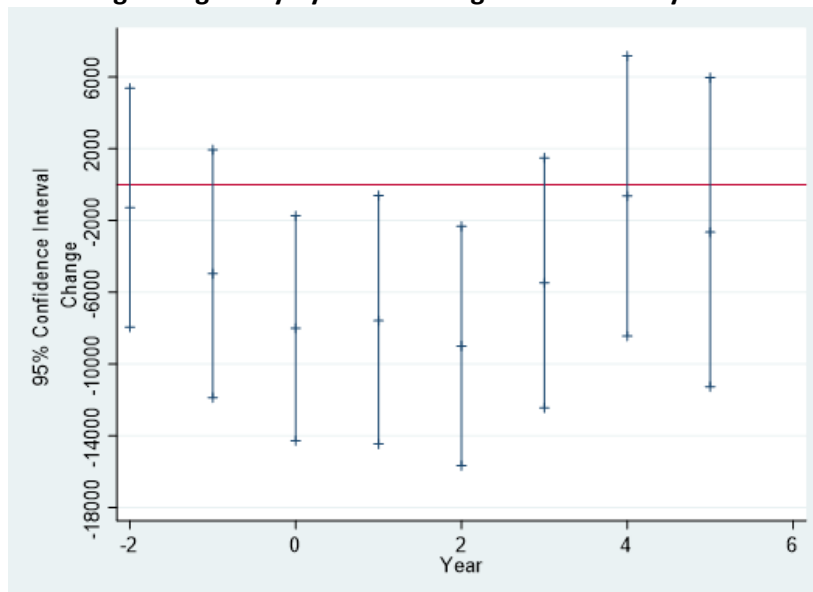


**Above Median Income Sample from Table 3**

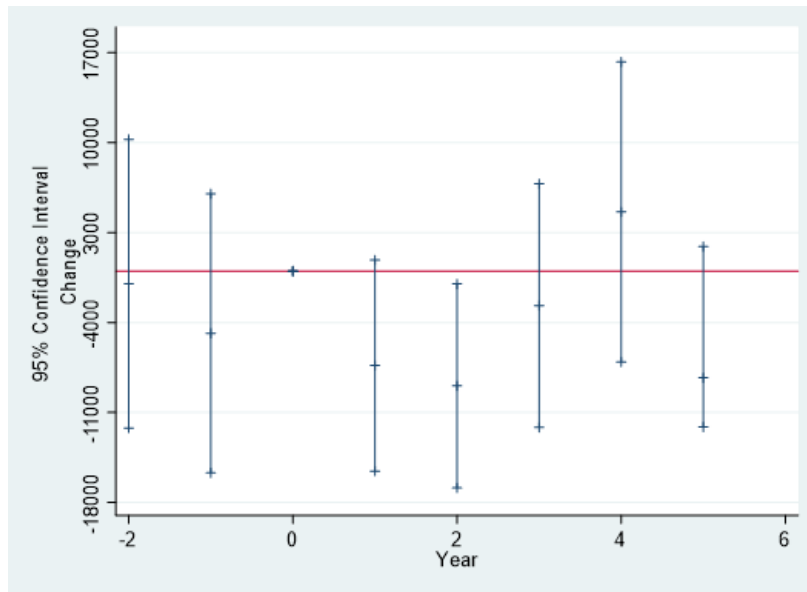


**Lowest 25% of income Sample from Table 4****Lowest 40% of income Sample from Table 4**

**High-Poverty Sample from Table 5****Low-Poverty Sample from Table 5**

**Full Sample 1991-2018 from Table 6****Addressing Endogeneity by Conditioning on Vote History from Table 8**

**Mean Income instead of Median Income from Table 9**



**Poverty as an Outcome from Table 10**

