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Government spending and economic activity: Regression discontinuity evidence from voting on renewals of tax levies

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Government spending and economic activity: Regression discontinuity evidence from voting on renewals of tax levies[1]

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Abstract

We estimate the impact of plausibly exogenous changes in taxes and government spending on income by utilizing regional data and a regression discontinuity design. More specifically, we identify an exogenous cut in local taxes accompanied by an equivalent reduction in local government spending by exploiting voting on renewals of tax levies of local governments in Ohio from 1991 to 2018, using a unique database that tracks city and village-level incomes and local election outcomes over time for the complete census of cities and villages in the state. We find that such "balanced budget" reductions in taxes and spending cause a large drop in local incomes that persists for two or three years before petering out. Importantly, this effect of local tax-financed government spending is present in locations with above average income inequality but not in those with low income inequality. Our results regarding the effect of locally tax-financed government spending on income are suggestive of the importance of mechanisms related to the prevalence of income heterogeneity and liquidity constrained agents in the local economy.

Keywords: Fiscal policy, balanced budget, redistribution, inequality.

JEL Codes: **E62**, **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 in this literature uses the narrative approach in order to identify exogenous changes in taxes or government spending. For example, Romer and Romer (2010) looks at presidential speeches and Congressional reports on major tax policy actions, and Ramey (2011) considers war expenditures. An alternative approach utilizes structural vector autoregressions (SVARS) and achieves identification by exploiting institutional features of fiscal systems (e.g., Blanchard and Perotti, 2002). Yet another approach to identification utilizes narrative measures of unanticipated taxes and government spending changes as instruments in a "proxy-SVAR" framework (e.g., Mertens and Ravn, 2013).

In this paper, we use an alternative approach based on a regression discontinuity (RD) design that can provide causal evidence for the effects of fiscal policy. This approach 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, et al., 2019). In our application, we identify an exogenous change in locally tax-financed government spending and estimate its direct impact on income per capita by making use of regional income data that are available for locations within the state of Ohio.[2]

[2] Regional income and output data are often unavailable to researchers studying the regional effects of government spending. Thus, e.g., Corbi, et al. (2019) estimate instead employment effects and map these into income based on an assumed production function.

More specifically, we consider an exogenous cut in local property taxes accompanied by an equivalent reduction in local government spending by exploiting voting on renewals of tax levies in the U.S. state of Ohio. Taxes and the associated government spending change abruptly at the 50% cutoff below which a tax levy is not renewed. Thus, voting percentages around the 50% cutoff serve as a source of exogenous variation allowing us to estimate the impact of a change in taxes and spending on economic activity. Moreover, 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 serves to further ensure 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 and villages that vote on current expense tax levies in the state of Ohio between 1991 and 2018. Beyond the availability of the appropriate regional tax levy and income data for a relatively long period necessary to conduct our regression discontinuity exercise, Ohio offers a number of advantages in terms of economic and political representativeness of the broader United States and also in terms of size. For instance, its 700 billion $U.S. GDP in 2020, comparable to that of Poland, would rank it 21st in the world if it were a country.

We find that such exogenous "balanced budget" reductions in property taxes and spending result in a drop in local incomes, evident in the first two or three years after the tax and spending cut. This drop in income is observed in the case of larger levies but not smaller ones, and in locations with above average income inequality but not in those with low income inequality. 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. In our context, higher local government spending is financed by higher property taxes so that redistribution effectively takes place from wealthier households and firms to poorer individuals with higher propensities to consume, which raises local incomes in a manner reminiscent of that in Farhi and Werning (2017). In terms of policy implications, this suggests that raising local government spending with relatively non-distortionary property taxes pays off in terms of incomes for the local economy.

A regression discontinuity design has recently been utilized in Corbi, et al. (2019) to study the impact of municipal expenditures on local labor markets in Brazil, in Braga, et al. (2017) to study the effect of government spending on local employment in Brazil, and 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. Becker, et al. (2010) had earlier used an RD design to study the causal effect of EU structural funds on the economic growth of treated regions, while Coelho (2018) utilizes both a panel IV approach and an RD design to study the impact of federal EU expenditures on regional output growth and employment.

Our work is closely related to the literature reviewed in Chodorow-Reich (2019) 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), and Serrato and Wingender (2016), and more generally to the literature which aims to shed light on macroeconomic questions using cross-sectional identification strategies. Similar to this body of work, we exploit some form of "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 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 economic outcomes. As argued in Serrato and Wingender (2016), "the 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" informing policy makers regarding the effectiveness of fiscal policy in smoothing regional business cycles. As explained in Nakamura and Steinson (2014), the regional approach has "important advantages" relative to the typical "closed economy" approach using aggregate U.S. data, since relative policy is precisely pinned down across regions, with the Federal Reserve unable to raise interest rates in some regions relative to others. As a result, regional estimates for the effect of government spending on economic activity are useful in distinguishing between different macroeconomic models, a point further elaborated on in Nakamura and Steinson (2018).

As noted by Chodorow-Reich (2019), "much of the pessimism regarding the informativeness of cross-sectional studies 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 could observe non-Ricardian effects at the regional level simply because locals do not fully endogenize a future hike in taxes that would be needed at the federal level 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 national level. Instead, we use locally tax-financed government spending which would be expected to influence Ricardian agents whose own spending depends on the present value of the tax burden but also those whose private spending depends on current net income, as long as they share this tax burden, so that there would be an offsetting decline in output due to the higher taxes. Our finding of large income effects of locally tax-financed government spending, especially for high inequality areas, suggests heterogeneity in income and wealth shifts the tax burden so that the resulting redistribution to less wealthy households, e.g. those less affected by the property tax, raises consumption and income despite the higher tax. Such mechanisms, reminiscent of Farhi and Werning (2017), if present at the local level, would have major implications for policy and welfare at the national level.

In the next section we describe our regression discontinuity design. Section 3 describes our data and preliminary analysis. The fourth section presents our results and is followed by robustness and falsification checks in Section 5. 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 controls 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 the natural log of per-capita income in city *i*, and let *t* index the year of the vote, so that the estimating equation is the following:

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

In Equation (1) the symbol *D* is a dummy variable that indicates whether the tax levy fails (= 1) or passes (= 0), so that τ is the treatment effect averaged over all local governments in the sample and all current expense tax levies during the timeframe. While *t* indexes the year of the vote, η indexes years before and after the vote. Positive values of η 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 η are useful as falsification tests to provide further assurance of the identification of any statistically significant treatment effects found with positive values of η. Regression discontinuity can proceed 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, ϵ 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, τ 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 τ 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 τ with a triangular kernel because it produces the MSE-optimal estimates (Cattaneo, Idrobo and Titiunik, 2019). We experiment with five different selection procedures to estimate the bandwith h, as detailed in the tables of results.

3. Data and preliminary analysis

3.1 Geography and Institutional Details

We study Ohio primarily because it has the data we need. The U.S. Bureau of the Census only tracks incomes at the city level year-by-year since 2010. Ohio has been tracking city-level income data since 1983. We require a source of income data that allows us to measure a discrete jump in response to a change in taxes. We also 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 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 1,000 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 it would rank 21st 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.

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 threeperson board of trustees. Property taxation is measured in millage, where 1 mill is one dollar collected on $1,000 of assessed property. Townships may collect up to 10 mills of property tax without a referendum, called inside millage, and it may levy taxes beyond 10 mills 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 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. 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. 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 median current expense tax levy is two mills. 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 appropriate than fuzzy RD.

Voters in Ohio must have lived in the state for at least 30 days before the election. They must be at least 18 years of age, have registered to vote, and be U.S. citizens. Residents cannot by law vote if they have been incarcerated for a felony conviction, have violated election laws, or been declared incompetent by a court.

3.2 Independence of Observations

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. A 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. 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. Conversations with city managers suggest this would not happen. Funding for the specific purpose would be cut like voters intended. On the other hand, 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. Funds from a fire tax levy may not be shifted to repair streets. Local government spending on current expenses must be cut.

3.3 Variables Used

3.3.1 Outcome Variable

The primary outcome we study is per-capita incomes for cities and villages. We use this measure as it can capture the average level of economic welfare in a location relatively well.[3,4] The data comes from the Ohio Department of Taxation's Tax Data Series Municipal Income Tax files. These files provide the income tax revenue and tax rate data needed to calculate aggregate income earned in the city. The definition of income is somewhat complex, but it generally 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 not benefits and transfer payments like pension benefits, alimony or child support. City residents and non-residents who work in the city pay the city income tax, and consequently, city residents who work in a different city pay the income tax for the city they work in; the home city usually allows a partial or full credit for a resident who pays income taxes to another city. Finally, residents of a city who work in a township still pay the city income tax. The important thing for the current study is that city incomes vary by year, so whatever the source, a

[3] In this, we follow previous work like Litschig and Morrison (2013), Fishback and Kachanovskaya (2015) and Serrato and Wingender (2016).

[4] We complement mean income per capita with data on poverty and income inequality to get a better understanding of how government spending affects or interacts with local welfare.

cut in city taxes and services may cause a discrete change in incomes in the city. It is important to note that testing by the authors shows that city population does not change in response to a tax cut, so any change in per capita incomes does not stem from a change in residential sorting within the time frame we study. In Section 4.1 we also test for a change in the number of workers. If population and the number of workers stays constant, any change in per capita income must stem from a change in income.

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 per capita 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 per capita incomes.

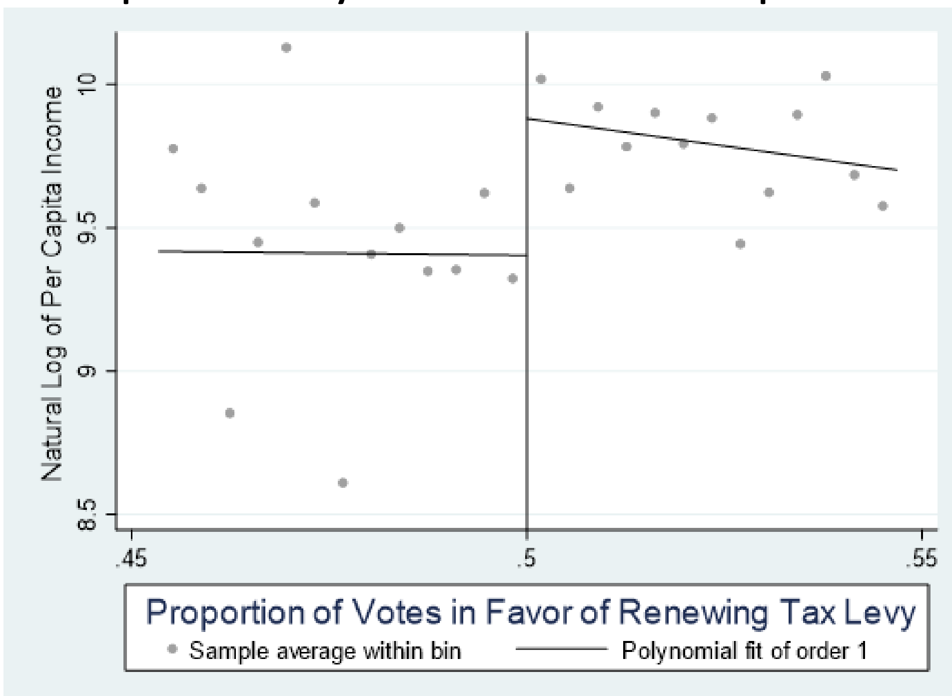
Per Capita Incomes are measured in constant 2010 U.S. dollars. The mean is $26,043. Even though they are unadjusted by the running variable, the raw means indicate a difference in incomes between the pass levy and fail levy groups one year after the vote--$26,453 vs. $18,457--hinting that voting to cut property taxes may lead to a drop in income. 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 per capita incomes for vote shares near the cutoff one year after the vote.

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. Graphs are constructed using the rdplot software in Stata (Calonico, Cattaneo and Titiunik, 2015).

One year after the vote, per-capita 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 the graph suggests.

Figure 1
Per Capita Income by Vote Share: Effective Sample Year *t*+1



3.3.2 Covariates

In ordinary least squares 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, given that the sample within the optimal bandwidth is just over 1,000 observations. The included covariates can be related to the outcome variable, as in least squares, or they can be related to the running variable instead. It is also important to note that regression discontinuity identifies its estimates differently than panel data techniques in which fixed effects are commonly used to account for omitted variable bias. When we include fixed effects as covariates, the estimates are very similar, but we notice a small loss of precision. The inclusion of irrelevant covariates in regression discontinuity decreases the precision of estimates (Calonico, et al., 2017), so we deduce that the fixed effects do not impart useful information over the covariates that we include.

We utilize a number of relevant covariates in our application. Table 1 shows covariate means split by cities that renew and cities that cut the property tax. % Minority is the proportion of non-white persons in the city. Marital status is captured by % Never Married, % Separated, and % Divorced, with married as the omitted category. The variable % With Kids measures the proportion of households with own children aged zero to 17 living with them most of the time. The next covariate we include is % Renters, the proportion of housing units in the city that are renter-occupied as opposed to owner-occupied. Furthermore, we control for education which is measured by three variables: 1) the proportion of persons 25 years and older with highest educational attainment less than a high school diploma, 2) the proportion of persons with highest educational attainment a high school diploma, and 3) the proportion with highest educational attainment some college or associates or technical degree, but less than a four-year diploma. Economic conditions are measured by the Unemployment Rate and the Labor Force Participation Rate. Finally, as an extension to the baseline results, we add the number of workers[5] as a

[5] We construct the city and village-specific number of workers by multiplying the fraction of people in the labor force by the population aged 18 to 64 years, and then multiplying this product by 1 minus the unemployment rate.

covariate to account for possible changes in a city's per capita income due to incoming workers from other regions that work and pay tax in the city but reside elsewhere.[6]

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. 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, as is the case with city or year fixed effects for instance. 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).

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

[6] Since those that work in the city pay income tax there, it is possible that observed city-level income and income per capita go up due to the number of people working in the city increasing even when population does not. We examine the robustness of our findings to controlling for the number of workers in a city in order to assess whether this mechanism is driving our results.

is that our data set begins in 1991 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 1990 for a vote in 1991, 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
Covariates		
% Minority	0.05	0.05
% Never Married	0.25	0.23
% Separated	0.02	0.02
% Divorced	0.11	0.12
% With Kids	0.41	0.40
% Renters	0.29	0.27
% Less than High School	0.22	0.20
% High School Only	0.46	0.46
% Some College	0.22	0.23
Unemployment Rate	0.07	0.06
Participation Rate	0.62	0.63
Number of Workers	1,173	1,108

Notes: Covariate means are shown at the time of the recreation tax levy vote. *p*-value shown for test of null hypothesis of equal means between failed and passed samples. Global number of observations = 1,313; local number of observations in effective bandwidth = 628.

4. Results

4.1 Main Results

Table 2 shows the main results of the paper. Covariates *W* are those listed in Table 1 except for Number of Workers. The cell in the upper left-hand corner shows a treatment effect of -0.52, with a *p*-value of 0.02 rendering it statistically significant. This means that one year after the vote, cities that fail to renew current expense tax levies have 52% lower per-capita incomes than cities that successfully renew. We note that within the effective bandwidth, the pass-levy sample has a mean median family income of $51,667 compared to $50,395 for the fail-levy sample.[7] Recall from Table 1 that these cities are nearly identical in characteristics.

The -0.52 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 equivalent reductions in taxes. Our definition of city incomes includes corporate profits, so cutting taxes by definition would increase corporate income. However, the positive treatment effect we find runs counter to the effect on corporate profits, as a tax cut is found here to cause a decrease in income, not an increase. This can be rationalized given that property taxes are a small part of a corporation's expenses and a two-mill reduction is a small change relative to the average 148 mills of property taxes in Ohio (smartasset, 2021). In any case, we can surmise that the positive treatment effect is not driven by corporations. We can also surmise that the change in per-capita incomes is not driven by transfer payments since our definition of income includes wages, salaries and corporate profits, but not transfer payments. And while an income tax would factor into wages, a property tax does not.

The next columns for the t+1 row show estimates using different ways to estimate the optimal bandwidth. The average treatment effect is -0.496. The average treatment effect declines to -0.446 and -0.306 respectively two and three years after the vote, 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 more worrying for identification

[7] Median family income from the U.S. Census Bureau has so far been excluded from our analysis as it is closely related to our outcome variable and would bias the treatment effect estimates if included as a covariate. We do however consider it as an alternative outcome variable later.

if a significant treatment effect were found for every period. The results suggest a shock to the local economy that diminishes over time, with full adjustment occurring four years after the vote.[8] The final rows of Table 2 indicate that failed pass levies are not associated with lower (or higher) local incomes one and two years before the vote occurs. The importance of these rows is discussed further in Section 5.4.

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

		Bandwidth Selection Option				
Year Relative to Vote	RD	TWO	SUM	COMB1	COMB2	RD with # of workers
t+1	-0.52 (0.02)	-0.40 (0.04)	-0.52 (0.02)	-0.52 (0.02)	-0.52 (0.02)	-0.53 (0.02)
t+2	-0.47 (0.01)	-0.34 (0.02)	-0.48 (0.01)	-0.47 (0.01)	-0.47 (0.01)	-0.48 (0.01)
t+3	-0.34 (0.04)	-0.21 (0.08)	-0.32 (0.03)	-0.34 (0.04)	-0.32 (0.03)	-0.35 (0.03)
t+4	-0.13 (0.51)	-0.06 (0.67)	-0.15 (0.44)	-0.15 (0.44)	-0.15 (0.42)	-0.14 (0.44)
t+5	-0.25 (0.18)	-0.17 (0.25)	-0.27 (0.17)	-0.27 (0.17)	-0.27 (0.15)	-0.27 (0.15)
t-1	-0.13 (0.58)	-0.11 (0.48)	-0.13 (0.57)	-0.13 (0.58)	-0.13 (0.57)	-0.15 (0.54)
t-2	0.01 (0.98)	-0.07 (0.69)	-0.00 (0.99)	-0.00 (0.99)	0.01 (0.99)	0.01 (0.98)

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 from Table 1 are included in all regressions, except that Number of Workers is only added for the final column. 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; 1,292 for *t*-1; and 1,221 for *t*-2.

[8] 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.

The above results are not driven by a change in city population. Testing by the authors shows that city population does not change in response to a tax cut, so any change in per capita incomes does not stem from a change in residential sorting within the time frame we study. Nor are they driven by an increase in the number of workers that would raise aggregate income and income per capita for a city with an unchanging population level. As we can see in the last column of Table 2 where we add the number of workers as a covariate, our estimated effects are virtually unchanged, as one would expect in regression discontinuity if the covariate is not endogenous. The treatment effect is -0.53 one period after the vote, -0.48 at t+2 and -0.35 at t+3 as compared to treatment effects of -0.52, -0.47 and -0.34 at t+1, t+2 and t+3 respectively in our baseline model shown in the first column of Table 2. These results are consistent with testing by the authors which shows that the number of workers as an outcome variable (with or without covariates) is not affected by failing to renew a levy. These results are also consistent with Table 1 and with Figure A1 at the end of the appendix which show respectively that the number of workers does not differ around the fifty percent cutoff for cities that vote against relative to those who vote to renew a levy and that this variable does not jump discontinuously at the cutoff.

4.2 Size of Tax Levy Extension

The median property tax in the sample is 2 mills. We split the sample into large tax levies (>= 2 mills) and small tax levies (<= 2 mills) to get some idea of dose-response of different sizes of tax levies. The covariates are those listed in Table 1 except for the number of workers. No treatment effect estimates are significant for the small levy sample, but Table 3 summarizes the results for the large levy sample. Based on these estimates, we deduce that our findings in the previous sections are due to larger levies, with smaller levies having no impact on local incomes.

The large-levy sample loses statistical significance after the second year, but the treatment effect estimates are larger than those for the full sample: -0.674 vs. -0.496 for year *t*+1, and -0.508 vs. -0.446 for year *t*+2. The results indicate that larger balanced-budget cuts in taxes and spending cause larger drops in aggregate city income. Our estimates here suggest that higher government spending, even if accompanied by an equivalent increase in taxes, raises local incomes in a manner consistent with large fiscal effects on economic activity.

Table 3
Large Levy Sample
Effect on Per Capita Income of Failing Versus Renewing Current Expense Tax and Spending in Years after and before the Vote when Millage >= 2

	Bandwidth Selection Option				
Year Relative to Vote	RD	TWO	SUM	COMB1	COMB2
t+1	-0.73 (0.05)	-0.52 (0.11)	-0.68 (0.05)	-0.73 (0.05)	-0.71 (0.05)
t+2	-0.53 (0.05)	-0.41 (0.10)	-0.54 (0.04)	-0.53 (0.05)	-0.53 (0.05)
t+3	-0.25 (0.25)	-0.08 (0.69)	-0.23 (0.28)	-0.25 (0.25)	-0.22 (0.29)
t+4	0.02 (0.96)	0.06 (0.78)	-0.06 (0.85)	-0.06 (0.85)	-0.05 (0.87)
t+5	-0.18 (0.58)	-0.14 (0.52)	-0.24 (0.46)	-0.24 (0.46)	-0.22 (0.48)
t-1	-0.06 (0.89)	-0.17 (0.43)	-0.06 (0.89)	-0.06 (0.89)	-0.06 (0.88)
t-2	0.21 (0.62)	-0.04 (0.88)	0.23 (0.62)	0.23 (0.62)	0.17 (0.70)

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 is used. This 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. All covariates from Table 1 except Number of Workers are included in all regressions. Number of observations for each lead and lag: 731 for *t*+1; 706 for *t*+2; 694 for *t*+3; 676 for *t*+4; 655 for *t*+5; 749 for *t*-1; and 703 for *t*-2.

4.3 Income Inequality Extension

We next construct a Herfindahl index of income distribution and split the sample into cities with greater and less than average income inequality. Again, the covariates are those in Table 1 except for number of workers. We find that cuts in taxes and spending decrease incomes in areas with a lot of income inequality. As shown in Table 4 below, a significant treatment effect at the five percent level is present in years two, three and five after the vote estimated at -0.40, -0.43 and 0.61, whereas years one and four are associated with estimated effects of -0.48 and -0.39 with p-values above five percent, at 0.10 and 0.08 respectively. Areas with a lot of income equality, on the other hand, experience no drop in incomes from cutting taxes and services.

This differential effect of changes in taxes and government spending on incomes is consistent with liquidity constraints and thus spending multipliers being larger in cities where income inequality is more prevalent. This could be plausibly explained by binding liquidity constraints resulting in higher marginal propensity to consume for poorer individuals, which leads to a greater response of local consumption and local incomes following an increase in government spending. Furthermore, since the higher property taxes are unlikely to burden poorer individuals with limited property ownership, an equivalent increase in such taxes does not impact their consumption directly.

These findings are consistent with models such as that of Farhi and Werning (2017) where liquidity constraints lead to larger marginal propensities to consume for poorer individuals and result in larger multiplier effects of government spending. In the latter paper, redistribution to liquidity-constrained consumers raises total consumption and 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 so that redistribution effectively takes place from wealthier households and firms to poorer individuals with higher propensities to consume, which thus raises local incomes via a demand-induced channel.

Table 4
Large Income Inequality Sample
Effect on Median Family Income of Failing Versus Renewing Current Expense Tax and Spending in Years after and before the Vote in Cities with Greater than Average Income Inequality

Regression Results		Covariate Name	Failed Levies Mean	Passed Levies Mean
Year Relative to Vote	Treatment Effect	% Minority	0.032	0.036
t+1	-0.48 (0.10)	% Never Married	0.239	0.229
t+2	-0.40 (0.05)	% Separated	0.018	0.015
t+3	-0.43 (0.04)	% Divorced	0.111	0.122
t+4	-0.39 (0.08)	% With Kids	0.392	0.373
t+5	-0.61 (0.02)	% Renters	0.282	0.270
t-1	-0.15 (0.61)	Participation rate	0.626	0.624
t-2	-0.32 (0.06)	Unemployment rate	0.066	0.054

Notes: Treatment effect estimates shown with *p*-values in parentheses below. Estimates show the effect of cutting current expense taxes and associated public services on Census measure median family income one through five years after the vote. The RD option of Calonico, et al. (2017) is used with triangular kernel and default nearest neighbor covariance structure. All covariates from Table 4 included in regressions. Number of observations for each lead and lag: 462 for *t*+1; 415 for *t*+2; 424 for *t*+3; 431 for *t*+4; 439 for *t*+5; 530 for *t*-1; and 519 for *t*-2. Means reported in the last two columns of the table are within a 0.054 bandwidth on either side of the cutoff.

4.4 Further analysis: Alternative outcome variables

4.4.1 Median family income

The effects on per-capita incomes are large, so large that one might question the Ohio income data used. We have another measure of income available. The U.S. Census Bureau tracks median family income yearly from 2010 to 2018, and in the decennial census years of 1990 and 2000. The yearly data has too few current expense tax levies to achieve the power necessary to find significant treatment effects, but adding tax levies from 1991 through 2009 helps. In addition, although linearly interpolating between census years will mute any immediate effect on incomes, it may pick up general trends.

We therefore redo our regressions using median family income as the outcome variable y from Equation (1). We use the RD bandwidth option with triangular kernel. Since the choice of covariates does not affect the magnitude of the treatment effect, we use a new set of covariates W that does a better job of increasing the precision of estimates with our new outcome variable, as is customary in regression discontinuity research. The covariates used are shown in the third column of Table 5, along with treatment effect estimates in the second column of the table.

Even when no covariates are used, we find nearly the same pattern of significance: the covariates serve to decrease the p-value of the treatment effect estimate of the t+1 regression from 0.06 without covariates to 0.05 with them as shown in the first cell of results in Table 5 above. The new set of covariates are the unemployment rate, the proportion of persons under age 5 and the proportion between the ages of 5 and 17, and three separate variables for highest educational attainment of less than a high school diploma, high school diploma only, and some college but less than obtaining a four-year bachelor's degree.

One year after voting to cut taxes and spending, median family income drops by $3,101. The second year after the vote incomes are still lower in those cities by $3,453 relative to the set of cities that renews its taxes and spending. No statistically significant effect is found for subsequent periods or for periods before the tax levy. The average drop in incomes for these two years on a base of $51,282 is 6.4%, which is far milder than the effect on the per-capita income measure from the Ohio Department of Taxation, but still sizeable. The smaller magnitude of effect for the Census measure is probably driven by the fact that extrapolating income data to the annual frequency implies that our outcome variable will not be as responsive to year-to-year changes in taxes and spending, thus tending to dampen the estimated impact on income.[9]

Table 5
Median Family Income
Effect on Median Family Income of Failing Versus Renewing Current Expense Tax and Spending in Years after and before the Vote

Regression Results		Covariate Name	Failed Levies Mean	Passed Levies Mean
Year Relative to Vote	Treatment Effect	Unemployment Rate	0.07	0.06
t+1	-3,101 (0.05)	% Under Age 5	0.07	0.07
t+2	-3,453 (0.03)	% Age 5 to 17	0.20	0.20
t+3	-2,373 (0.16)	% No High School Diploma	0.22	0.20
t+4	-2,228 (0.19)	% Only High School Diploma	0.46	0.46
t+5	-2,747 (0.13)	% Some College	0.22	0.23

Notes: Treatment effect estimates shown with *p*-values in parentheses below. Estimates show the effect of cutting current expense taxes and associated public services on Census measure median family income one through five years after the vote. The RD option of Calonico, et al. (2017) is used with triangular kernel and default nearest neighbor covariance structure. Number of observations in each year *t*+1 through *t*+5: 4,357; 4,224; 4,039; 3,882; and 3,720. Covariate means shown at time of the vote within the 0.11 effective bandwidth for the *t*+1 regression, which contains 686 observations.

[9] It might also be partly driven by the larger base which is about double that of the Ohio per capita measure.

Besides the linear interpolation, the median family income and per-capita income variables differ in what constitutes income. Median family income does not include corporate profits, but it does include additional forms of income and transfer payments. The additional forms of income are rents, estate and trust income, pension and retirement income, royalties, and interest and dividends. The transfer payments include unemployment compensation, worker's compensation, social security, SSI payments, public assistance, veteran's payments, survivor benefits, disability benefits, educational assistance, alimony, child support, and financial assistance from outside the household. Looking at this extensive list, we suggest that most forms of additional income (retirement income, etc.) are not likely to change much in the wake of a failed current expense tax levy. Most transfer payments are unlikely to change much, either, so the wages and earnings are probably the categories of income that change the most in the Census measure, just like for Ohio's measure of per-capita income.

The results for Table 5 are obtained using the RD bandwidth option, which is the most commonly used form in the literature. The appendix shows results using all bandwidth options. They largely corroborate Table 5, except when the TWO bandwidth option is used. The TWO bandwidth shows statistical significance for period *t*-1, which is problematic as discussed in Section 5.4, so the results using bandwidths other than TWO are preferable.

4.4.2 Poverty

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 the income measures.

The treatment effect estimates are precise even without the addition of covariates. We find a significant increase in the poverty rate in communities that vote to cut taxes and spending relative to ones that renew the tax. The treatment effect we find (not shown in a table here for brevity) is an increase of three percentage points in years *t*+1 and *t*+2. This is a 27% increase on a basis of 0.11. 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 also find that following an increase in government spending, local poverty rates decline by 4 percentage points from a comparison group mean poverty rate of 0.64.

However, the regression discontinuity framework may not be appropriate for this outcome variable because it also finds a treatment effect two years *before* the tax levy. On the other hand, the problem seems to be isolated to taxes in 1991 and 1992 because when these years are omitted, the three-percentage point treatment effect persists for years *t*+1 and *t*+2 and no treatment effect estimate is significant before the vote. We still only relate these estimates with caution, even if they corroborate the treatment effect estimates using the two different income measures.

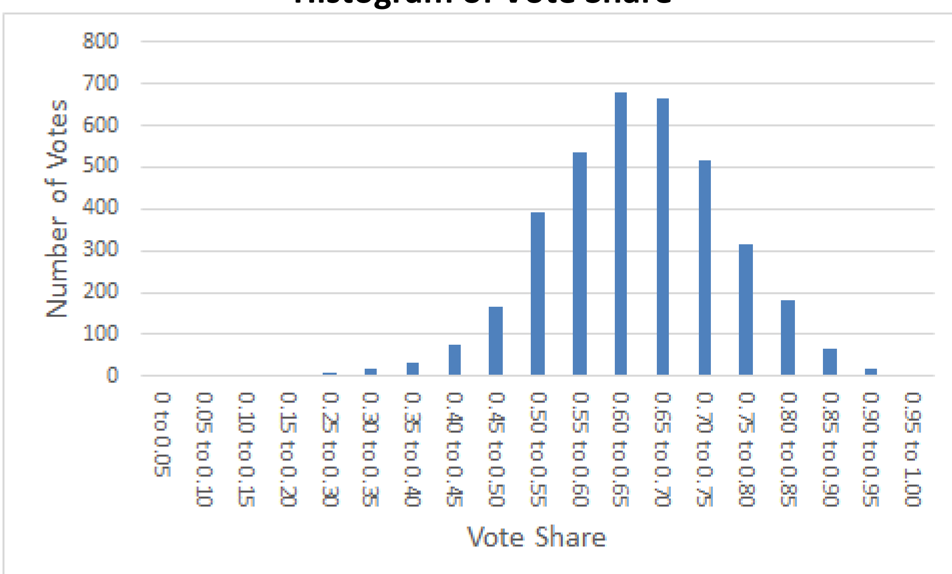
5. Challenges to Identification

5.1 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 density test is the traditional way to assess this possibility. The density test of Cattaneo, Jannson and Ma (2018) yields a *p*-value of 0.14, failing to reject the null hypothesis of no discontinuity in vote share around the 0.50 cutoff.

A histogram of vote share in Figure 2 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.

Figure 2
Histogram of Vote Share



5.2 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 is allowed to jump discontinuously is whether an agent receives treatment. If, e.g., values of % Minority have a discontinuity at the cutoff, it is possible that the treatment effect is capturing the effect of % Minority on Per Capita Incomes, 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 covariates listed in Table 1 (excluding Number of Workers), with the running variable and treatment dummy as regressors. We test whether the estimate for the treatment effect is jointly zero. The resulting chi-squared test statistic is 0.69, indicating that the covariates as a whole do not jump discontinuously at the cutoff. Running each regression individually with each covariate as regressor does not yield a significant treatment effect, either. The corresponding test for the covariates in Table 4 yields a *p*-value of 0.44, similarly failing to reject the null hypothesis of no effect, and also not showing significance in individual regressions. We augment this test with graphs of each covariate plotted against vote share, shown in Figure A1 in the Appendix. The graphs do not suggest any discontinuity, either.

5.3 Placebo Cutoff

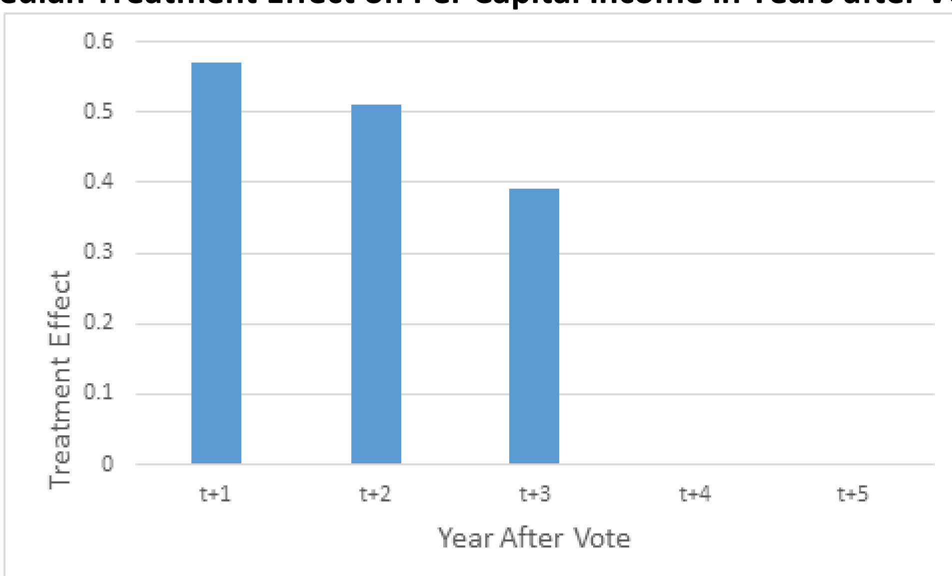
Neither the running variable nor the covariates exhibit a discontinuity at the cutoff. Nevertheless, the discontinuity of Per Capita Income at the fifty percent cutoff could be due to random chance. To guard against this possibility, we re-estimate the treatment effect using false cutoffs that are outside of the optimal bandwidth. When we pretend that the cutoff is 0.425, 0.575, or 0.6 we find no significant treatment effects, suggesting that the discontinuity at 0.50 stems from the vote and not from random chance. The same reassuring pattern of insignificance characterizes the placebo cutoff test when median family income is the outcome variable.

5.4 Prior Periods

We now change the outcome variable to capture differences in income per capita before the votes occur. For example, favorable economic conditions reflected in higher income per capita could make voters more likely to vote for tax levy renewals. In general, if a vote in 2003 is found to be related to per capita Incomes in 2002 or 2001, it would suggest that some unobserved factor was causing the treatment effect, and could be the true cause of the significant treatment effects in Tables 2, 3, 4 and 5. This unobserved factor could be an imbalance in an unobserved covariate. Whatever the source, examining the effect on prior periods is a powerful test of whether confounding factors are responsible for the treatment effects. The results are shown in the final rows of Tables 2, 3, 4, and A1.

Other than t-1 for the TWO bandwidth of Table A1, reassuringly, no estimate is statistically significant. If covariate imbalance were affecting estimates in periods t+1 and t+2, it should be affecting them in periods t-1 and t-2, too, but there is no such effect. 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 Per Capita 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*+4 and *t*+5 producing significant treatment effects, but there is no statistical significance for these years, either. The evidence thus suggests that an imbalance in omitted factors is not causing the treatment effects we observe.

Figure 3
Median Treatment Effect on Per Capital Income in Years after Vote



One might worry about the effect of confounding factors if a large treatment effect is found every year, but Figure 3 shows a treatment effect that declines in magnitude and dissipates four years after the vote. The corresponding test using the median family income measure similarly shows no statistically significant treatment effect one or two years before the vote, except when using the TWO bandwidth option, which is a reason why we rely instead on the estimates from the RD option.

6. Conclusion

We consider plausibly exogenous variation arising from voting on the renewal of tax levies within an RD design framework. This allows us to clearly identify the source of variation in government spending and 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 or three years following the outcome of the vote. This drop in income is observed for larger levies but not smaller ones, and in locations with above average income inequality but not in those with low inequality. Furthermore, we find that poverty goes up after a balanced budget cut in government spending and taxes. 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 large effects of local government spending on economic activity. In terms of policy implications, this evidence suggests that raising local government spending by raising relatively non-distortionary property taxes pays off for the local economy in terms of incomes and overall welfare.

The large impact of levy renewals in our baseline and the large “open economy” regional multiplier estimated in Nakamura and Steinson (2014), are analogous to closed economy aggregate multipliers for a more accommodative monetary policy than has typically been in place for the United States. 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 about 2 in Chodorow-Reich et al. (2012), Serrato and Wingender (2016), Shoag (2016), and Fishback and Kachanovskaya (2015),[10] are consistent with the much lower existing estimates of the closed economy aggregate multiplier in previous work and comparable to the similarly large estimates in Ilzetzki et al. (2013) for countries with fixed exchange rate regimes.[11]

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 Steinson (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) and Farhi and Werning (2017).

In particular, our findings here 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

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[10] The latter authors estimate "an added dollar of federal spending in a state increased state per capita income by between 40 and 96 cents".

[11] 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.

setting with liquidity constraints. In that setting, 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.[12] Indeed, in our data, higher government spending raises local incomes even when government spending is funded by an equivalent 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 so that redistribution effectively takes place from wealthier households and firms to poorer individuals with higher propensities to consume, which raises 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 occurs only in areas with a lot of income inequality 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 such areas, the last finding could be plausibly explained by binding liquidity constraints resulting in higher marginal propensity to consume for poorer individuals that are more prevalent in high inequality areas, which leads to a greater response of local consumption and local incomes following an increase in government spending in those areas, along the lines of New-Keynesian macroeconomic models with liquidity-constrained agents and heterogeneity in income and wealth.

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[12] See their “Hand-to-Mouth in a Liquidity Trap” setup in pages 2451-2454.

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Appendix

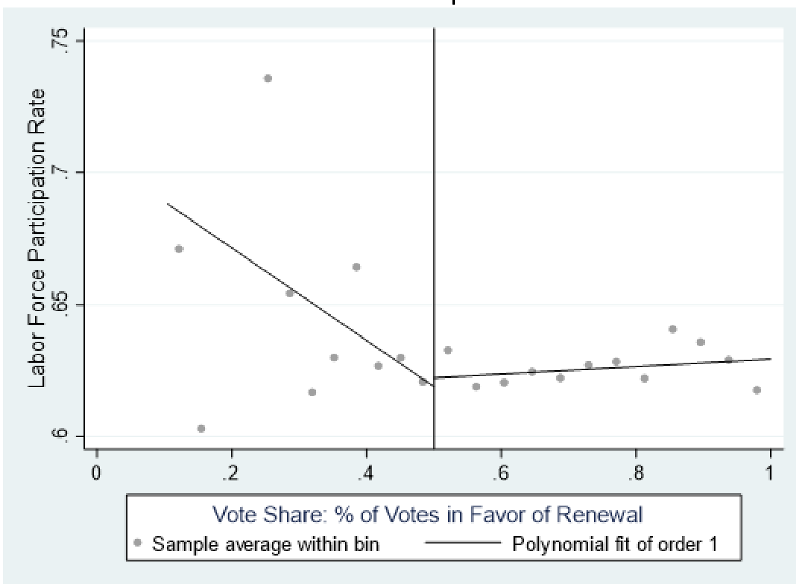
Table A1
Effect on Median Family Income of Failing Versus Renewing Current Expense Tax and Spending in Years after and before the Vote: All Bandwidth Options

	Bandwidth Selection Option				
Year Relative to Vote	RD	TWO	SUM	COMB1	COMB2
t+1	-3,101 (0.05)	-4,287 (0.01)	-3,017 (0.07)	-3,007 (0.07)	-3,237 (0.05)
t+2	-3,453 (0.03)	-5,143 (0.01)	-3,245 (0.05)	-3,245 (0.05)	-3,524 (0.03)
t+3	-2,373 (0.16)	-4,412 (0.01)	-2,366 (0.20)	-2,366 (0.20)	-2,777 (0.12)
t+4	-2,228 (0.19)	-4,176 (0.01)	-2,068 (0.27)	-2,068 (0.27)	-2,481 (0.17)
t+5	-2,747 (0.13)	-4,389 (0.01)	-2,703 (0.16)	-2,703 (0.16)	-3,003 (0.11)
t-1	-2,213 (0.14)	-2,916 (0.04)	-2,136 (0.16)	-2,079 (0.18)	-2,323 (0.13)
t-2	-2,189 (0.14)	-2,053 (0.13)	-2,212 (0.11)	-2,189 (0.14)	-2,327 (0.11)

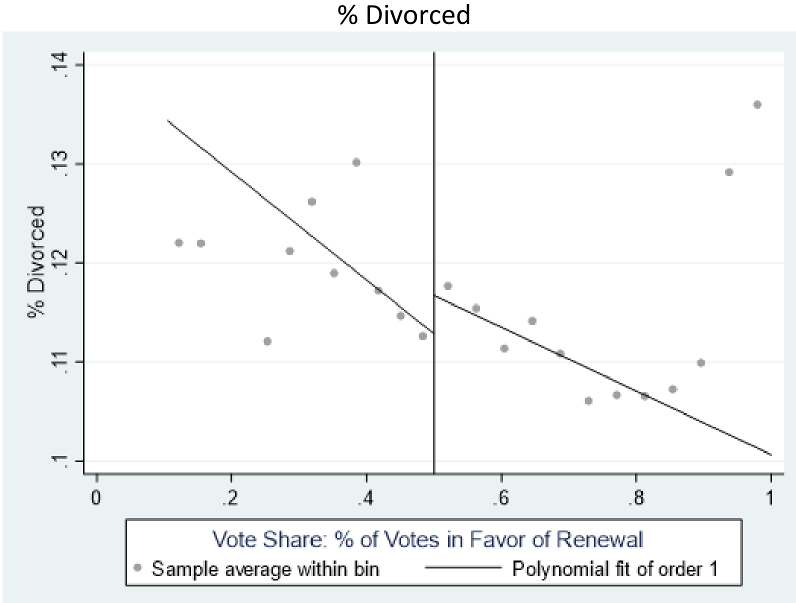
Notes: Treatment effect estimates shown with *p*-values in parentheses below. Local average treatment effect is the effect of not renewing a current expense tax levy on the natural log of Per Capita Income in a city in years after the vote, relative to voting to renew tax funding. Mean squared error-optimal 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 is used. This 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 from Table 4 are included in all regressions. Number of observations for each lead and lag: 731 for *t*+1; 706 for *t*+2; 694 for *t*+3; 676 for *t*+4; 655 for *t*+5; 749 for *t*-1; and 703 for *t*-2.

Figure A1
Graphs of Covariate Smoothness at the Cutoff

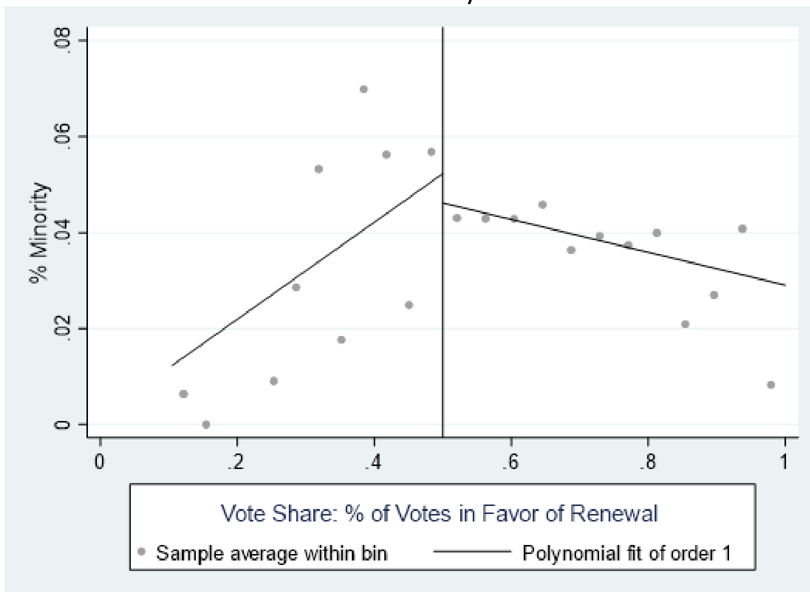
Labor Force Participation Rate



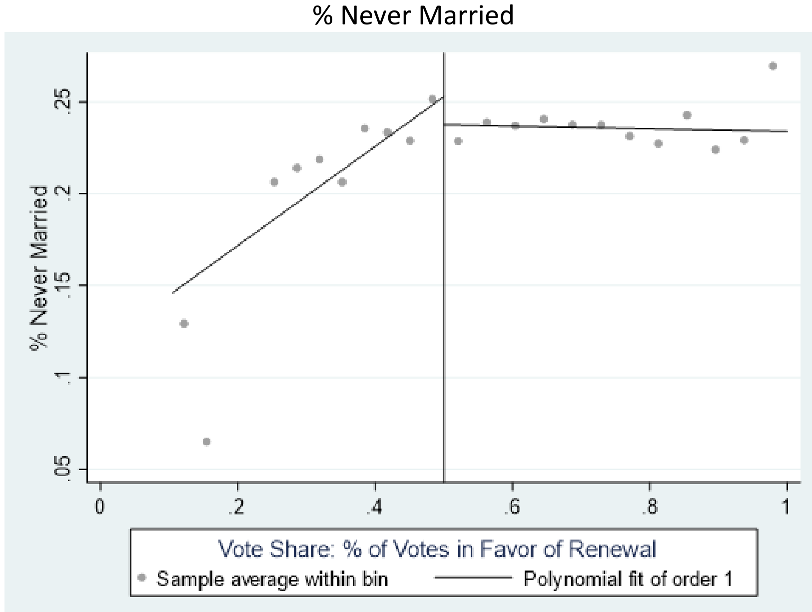
% Divorced



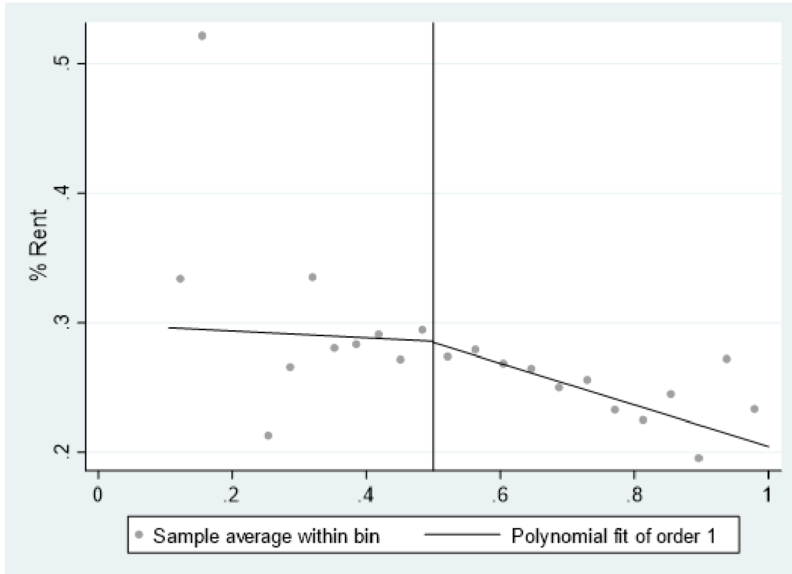
% Minority



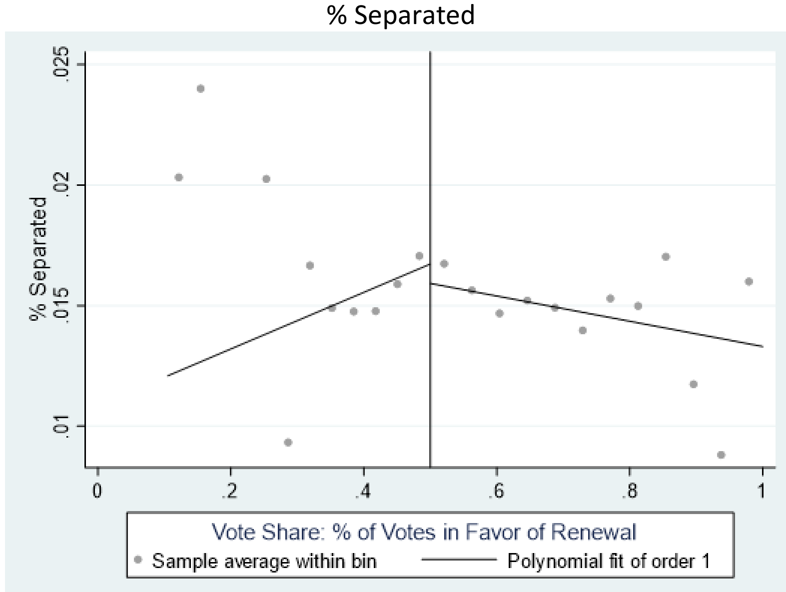
% Never Married



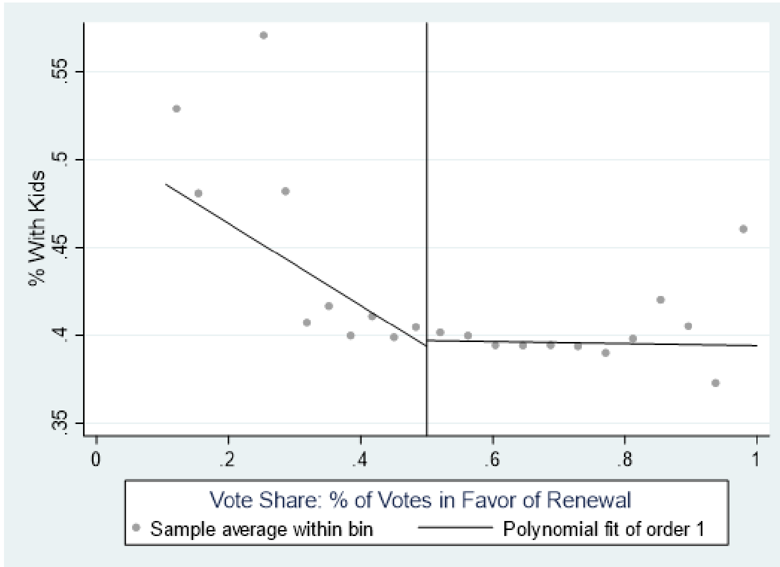
% Rent



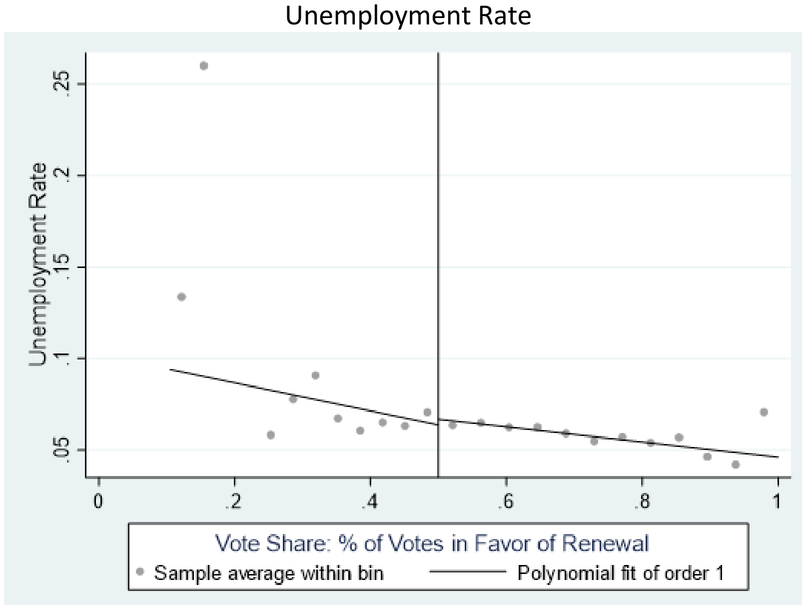
% Separated



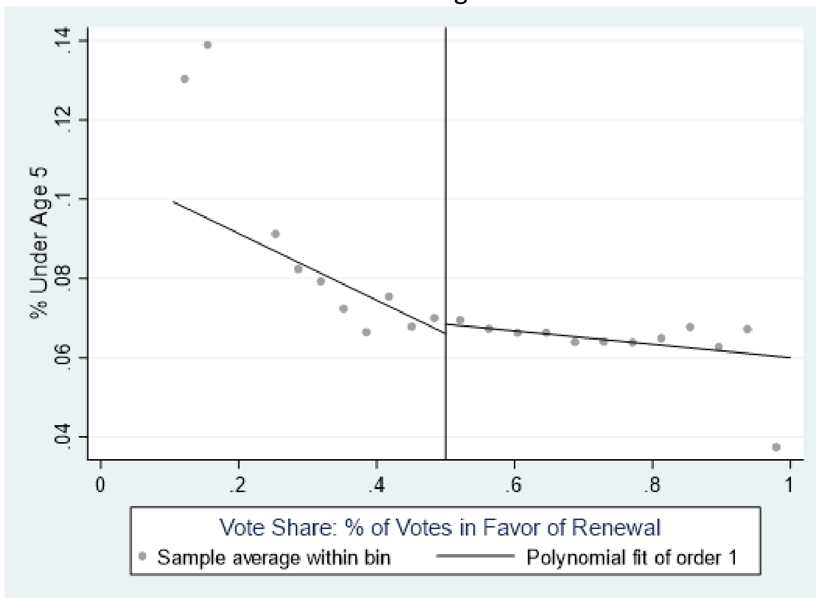
% With Kids



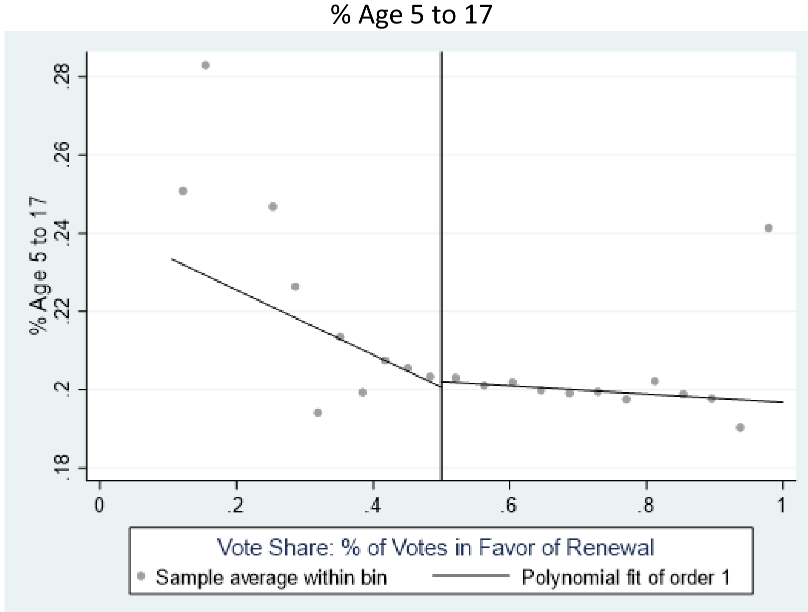
Unemployment Rate



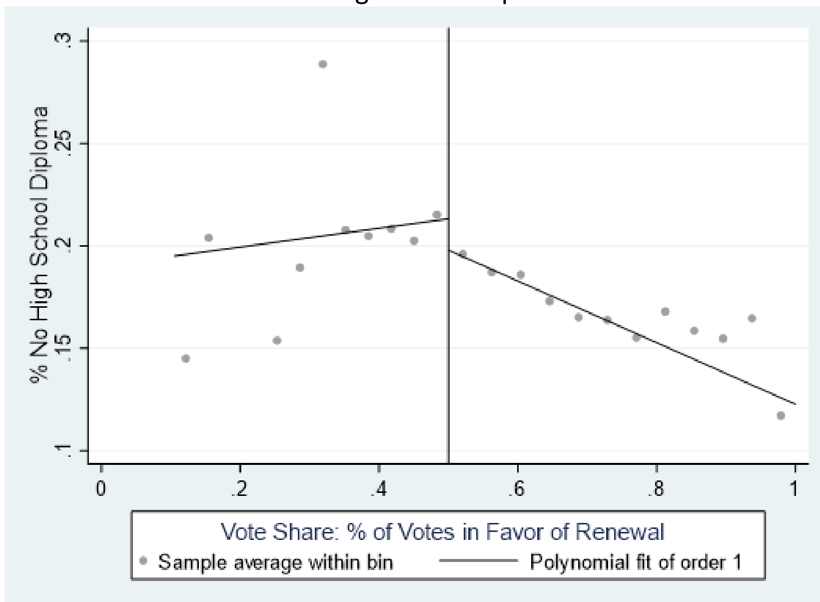
% Under Age 5



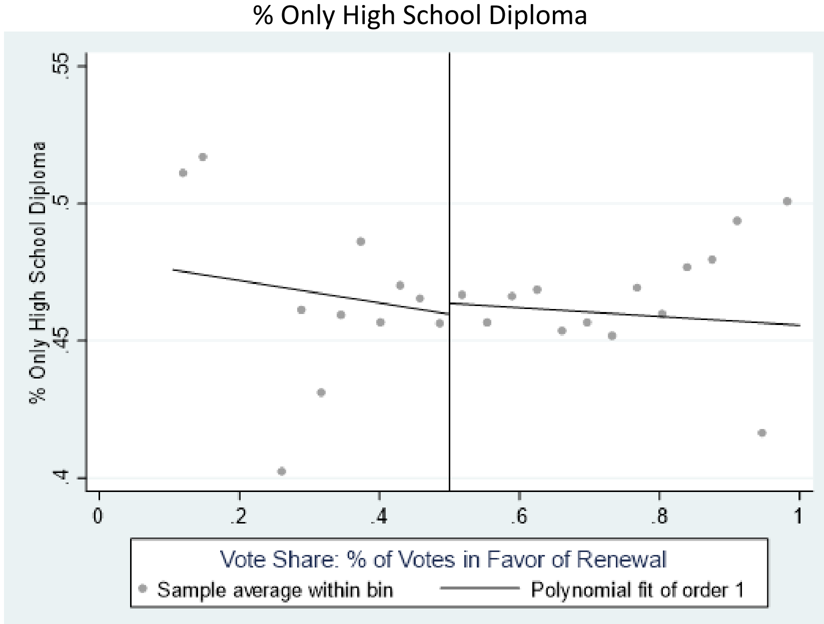
% Age 5 to 17



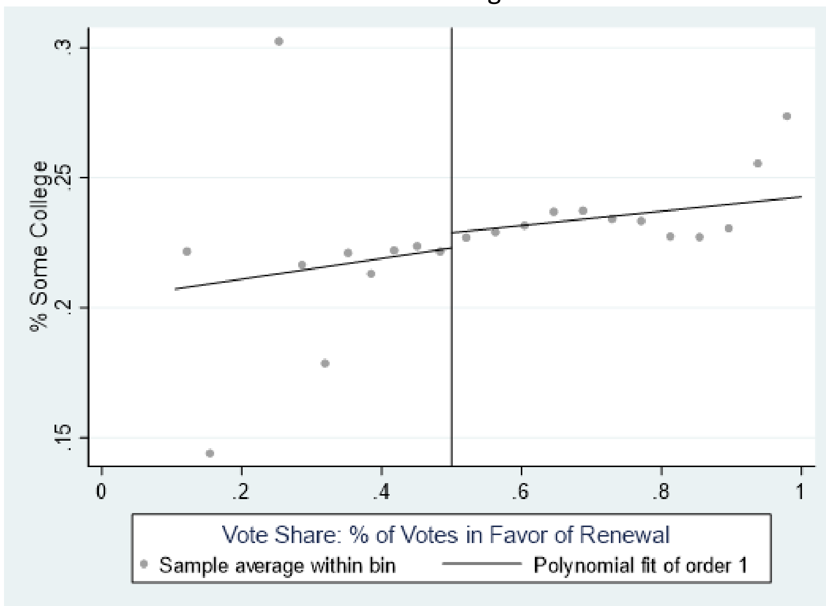
% No High School Diploma



% Only High School Diploma



% Some College



Number of Workers