Fiscal policy and economic activity: New Causal Evidence

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Motivation

- A major question for policy and academia alike: What is the economic impact of fiscal policy?
- A large macro literature attempts to identify exogenous changes in taxes & government spending and estimate their impact on economic activity.
- One line of research uses the narrative approach to identify exogenous changes in taxes or govt spending (Romer&Romer, 2010, or Ramey, 2011).
- Alternative approaches utilize structural VARs (Blanchard&Perotti, 2002) or narrative measures of unanticipated taxes&government spending as instruments in proxy-SVARs (Mertens&Ravn, 2013).

What we do

- We apply a quasi-natural experiment design that provides causal evidence for the effects of fiscal policy. Regression discontinuity (RD) only recently applied in macro to identify fiscal policy effects (e.g., Corbi, et al., 2019).

- Unique dataset combining local incomes with local voting outcomes on current expense tax levies for the complete census of cities and villages in Ohio over 1991-2018.

- Taxes and the associated spending change abruptly at the 50% vote share cutoff below which a tax levy is not renewed. This cutoff determines which observations serve as controls and which receive treatment: a reduction in taxes & local government spending.

- Voting percentages around 50% cutoff are a source of exogenous variation, with observations around this quasi-randomly assigned and very similar across characteristics.

- By exploiting voting on renewals of tax levies of local governments within this design, we identify a truly exogenous cut in local taxes and an equivalent reduction in local spending.

What we do

- Timing of renewals predetermined at time the tax levy was introduced 5 years ago, so using renewals instead of new tax levies ensures these are not endogenous responses to prevailing economic conditions.

- Use local tax-financed government spending that can influence agents whose spending depends on the present value of the tax burden. Allows for the possibility of an offsetting decline in output due to higher taxes.

- Our data and research design allow comparison of fiscal expenditure and fiscal revenue effects.

- Given the nature of our data, our analysis can shed light on intermediate mechanisms behind these effects.

What we find

- Balanced budget cuts in taxes&spending cause large drops in local incomes, implying that government spending effects on income are larger than fiscal revenue ones: a 14% income drop in the first year after the cut.
- Effects persist for 2 years: the cumulative balanced budget spending multiplier is about 1.5 in our baseline.
- This effect of tax-financed govt spending prominent in low-income areas and in high-poverty areas, absent in high-income or low-poverty areas.
- Results are in line with mechanisms related to the prevalence of liquidity-constrained agents in the local economy (e.g., Farhi & Werning, 2017.)

Data

- Ohio one of few states with centralized repository of election outcomes: >4,000 votes matched to income and demographic characteristics for 1,000 townships, 247 cities and 680 villages.

- Ohio's 2020 GDP was $700 billion which would have ranked it 21st in the world in terms of GDP.

- Local property tax levies in Ohio must specify the purpose of the tax. The type of tax we use is the general levy for current expenses: includes salaries&materials to support services like public safety and public health, garbage collection, and the maintenance, operation and repair of parks, roads, bridges and public buildings.

Data

- 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. Mean current expense tax levy is .26% of assessed value &duration for >90% of sample is 5 years

- When the tax is due to expire, the city asks voters to renew it. If voters choose not to renew, the tax is removed and local government spending declines by an equivalent amount.

Regression discontinuity design

- RD requires a ‘running’ variable taking different values on each side of a cutoff that determines whether agents receive treatment or serve as controls.

- Taxes and the associated government spending change abruptly at the 50% vote share cutoff below which a tax levy is not renewed.

- Voting shares around the 50% cutoff are a source of exogenous variation allowing us to estimate impact of a change in taxes and spending.

- Testable assumption: observations on either side of c have similar characteristics.

Regression discontinuity design: specifics

- The running variable is vote share *V* (proportion of votes in favor of tax levy). *c* is the cutoff value of *V* that controls which observations serve as controls and which receive treatment: a reduction in property taxes and local spending. As local property taxes follow simple majority rule, 50% is our cutoff.

- 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:

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

- *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.

- η indexes years before and after the vote so that positive values of η test for the time it takes for treatment to first appear and for persistence and rate of decay of effect.

Regression discontinuity design: bandwidth

- Ideally, τ would be estimated exactly at the cutoff c. Not possible as there are few observations at the cutoff and those with c = 0.50 are all failed tax levies.

- We estimate τ within some bandwidth h of c as we need variation from tax levies that fail and tax levies that pass, plus we require sufficient statistical power.

- h should be large enough to allow for precisely estimated treatment effects but not so wide that observations on either side of c have different characteristics, as that would violate randomization of agents around c.

- Imbens&Kalyanaraman's (2012) method estimates a bandwidth that minimizes the mean squared error of the treatment effect estimator.

- We estimate τ with a triangular kernel as it produces the MSE-optimal estimates and consider 5 selection procedures for h.

Income by Vote Share: Effective Sample Year t+1

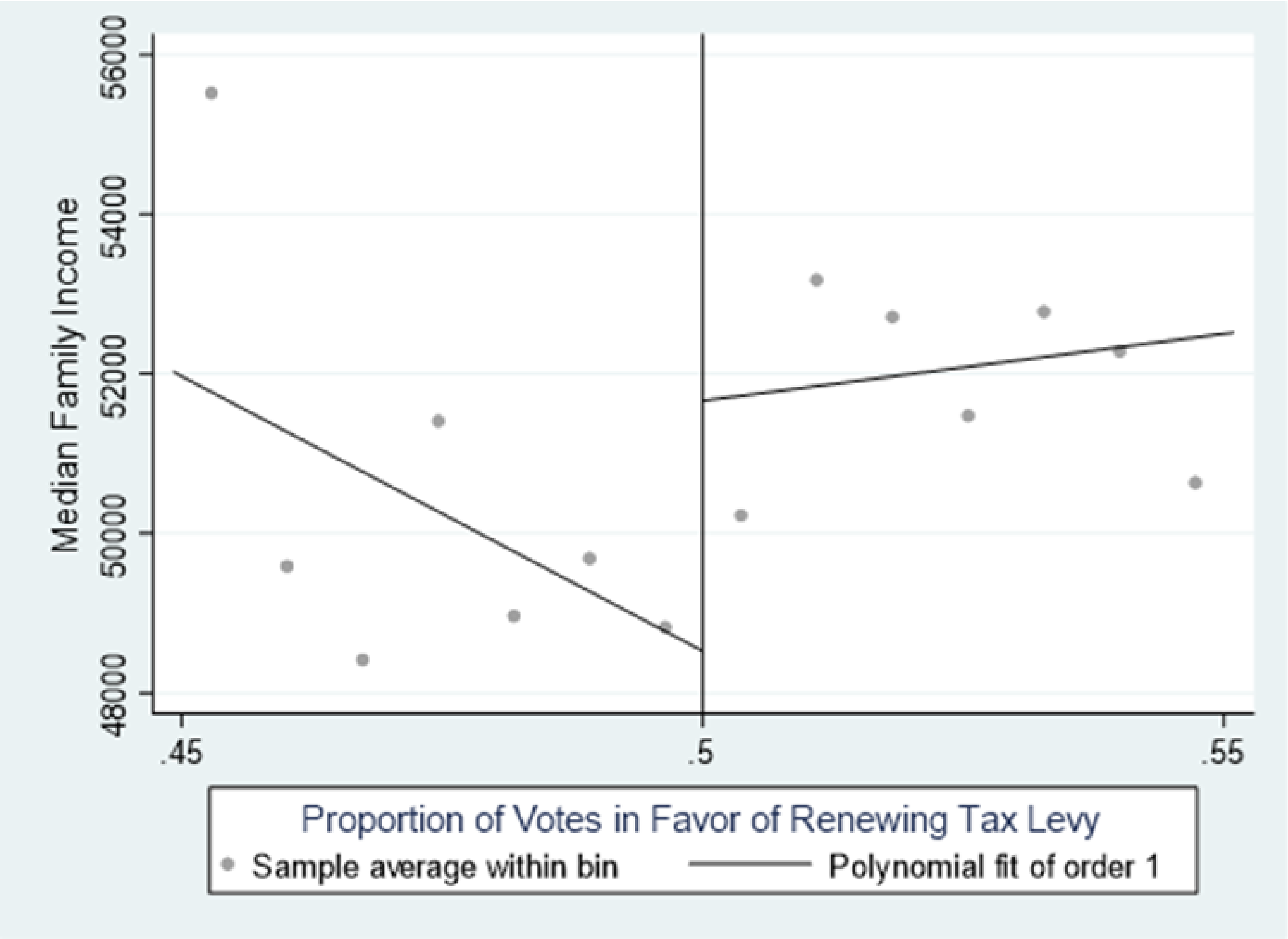

Table 1
Covariate Means by Tax Levy Renewal Status within Effective Bandwidth

	Failed Levies	Passed Levies	*t*-test for equal means
Income	47.7	48.6	0.72
Poverty Rate	0.20	0.19	0.40
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
% 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
% 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

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

Time Period Relative to Year of Vote	Estimates (p-values)	Number of Observations
t+1	-7,020* (0.04)	1,275
t+2	-7,431* (0.03)	1,142
t+3	-4,625 (0.20)	957
t+4	-2,274 (0.58)	801
t+5	-1,944 (0.62)	640
t-1	-5,085 (0.16)	1,422
t-2	-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, 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.

Table 3: High-income (above median) and Low-Income (below income) locations

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

Table 4: Locations in bottom quartile of income and lowest 40% of income

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

Table 5: High-poverty (top quartilie) vs low-poverty (bottom quartile) locations

	High-Poverty Locations		Low-Poverty Locations	
Time Period Relative to Year of Vote	Estimates (p-values)	Number of Observations	Estimates (p-values)	Number of Observations
t+1	-10,767* (0.05)	289	-6,982 (0.30)	321
t+2	-9,315* (0.03)	238	- -	
t+3	-16,032* (0.01)	171	7,187 (0.32)	273
t+4	-12,340* (0.01)	146	- -	
t+5	-27,012* (0.01)	112	- -	
t-1	-3,992 (0.39)	349	9,455 (0.10)	342
t-2	-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 25th percentile) cities, for example. Low-poverty is bottom 25th 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.

Conclusions

- The cumulative balanced budget spending multiplier is 1.5, the same as govt spending multiplier in Nakamura and Steinsson (2014) and Acconcia et al. (2014), comparable to regional multiplier estimates of about 2 in Chodorow-Reich et al. (2012), Serrato & Wingender (2016), Shoag (2016) and Fishback & Kachanovskaya (2015), and at upper bound of govt spending multipliers in Ramey & Zubairy (2018).

- Results imply importance of mechanisms related to the prevalence of liquidity constrained agents:

Conclusions

- Effect of govt spending benefiting broader populace is greater than that of taxes for property owners, suggesting a welfare-improving redistribution mechanism is at work when both local taxes and spending rise: Higher spending financed by higher property taxes so redistribution takes place from wealthier households & firms to poorer individuals with limited property ownership and higher propensities to consume, raising local incomes as in Farhi&Werning (2017).

- Increase in income following a locally tax-financed increase in spending is pre-eminent in low-income and high-poverty areas where liquidity constraints are more likely to bind (suggests mechanism resembling Farhi&Werning, 2017).