When Voters Matter: The Growth and Limits of Local Government Responsiveness

Michael W. Sances*

August 2, 2017

Abstract

I use over-time variation resulting from geographic sorting to estimate the impact of local preferences on local policy. Building a new panel data set covering 3,000 unique counties between 1957 and 2012, I find a causal impact of changes in preferences on changes in policy. However, this responsiveness has not been constant, but began in the 1970s, gradually increasing since. Local responsiveness is also not unlimited: while public safety and infrastructure spending respond, policies more constrained by state laws do not. These results corroborate recent evidence of local responsiveness using a more extensive sample, and a research design that rules out many alternative explanations. They also reveal many local policies to be unresponsive due to state-imposed constraints.

*Assistant Professor, University of Memphis, msances@memphis.edu. For helpful comments, I thank Justin de Benedictis-Kessner, Mike Craw, Christopher Goodman, Eric Groenendyk, Vlad Kogan, Whitney Ross Manzo, and participants at the 2017 MPSA conference, the 2017 SPSA conference, and the 2017 State Politics and Policy Conference. A previous version of this paper was entitled "Polarization's Impact on Local Government."

Do local governments respond to the preferences of their voters? Until very recently, many observers would have viewed the possibility of local responsiveness as remote. In his influential work *City Limits*, Peterson (1981) characterizes “attempts to explain [local] public policy in terms of political variables” as “largely a muddle” (9). The lack of consistent evidence for responsiveness was to be expected, according to Peterson, given the numerous constraints imposed on local governments by economics and by state and federal regulations. As recently as 2012, the authors of a study on political accountability in cities could cite Peterson’s work as reason to expect a lack of accountability; yet they acknowledge that “empirical studies” of local responsiveness, now over thirty years since, are “few” (Arnold and Carnes 2012, 950).

Empirical studies of local accountability may now be characterized as slightly more than “few,” and may no longer be fairly cast as inconsistent. In just the past few years, several studies find strong evidence in favor of local responsiveness. Using different samples and measures of preferences and policy, both Tausanovitch and Warshaw (2014) and Einstein and Kogan (2016) find robust positive associations between city preferences and city policy. Other studies show that voters do appear to evaluate local officials on the basis of policy and performance, a seemingly necessary condition for responsiveness. In “the first time-series analysis of mayoral approval,” Arnold and Carnes (2012, 949) find that variation in crime and economic performance influence voter approval of New York mayors, while both Boudreau et al. (2015) and Sances (2017) find evidence of spatial voting in contemporary mayoral campaigns. A third set of studies shows party control of local government influences policy, suggesting a link between voters’ spatial reasoning and the connection between opinion and policy (Gerber and Hopkins 2011; de Benedictis-Kessner and Warshaw 2016).

Yet while local responsiveness is now much more plausible than it was even ten years ago, questions remain. Many of the studies cited above provide only indirect evidence of responsiveness. Only Tausanovitch and Warshaw (2014) and Einstein and Kogan (2016) directly examine the link between local preferences and local policy, and both studies do so primarily using cross-sectional data from the 2000s and later. The reliance on cross-sectional data means many alternative explanations for the observed relationship between opinion and policy cannot be ruled out, while the focus on recent years means we cannot know whether responsiveness is the central tendency of local governments, or only a modern aberration. Further limiting the scope of existing findings, these studies also focus exclusively on larger cities, ignoring the tens of thousands of smaller cities, county governments, special districts, and school districts that make up the vast majority of local governments in the United States.

Also unknown is precisely *when* local governments will be limited by upper-level governments. While theoretical, Peterson's argument about state-imposed constraints has a strong factual and legal basis. Local governments have no rights in the federal constitution, are created and empowered by state governments, may not pass legislation that conflicts with state law, and are always at risk of their decisions being overruled by state legislatures. Oddly, however, the existing evidence suggests local governments are almost entirely unconstrained when it comes to responding to local voters. Is it possible to reconcile these empirical findings with the legal realities of intergovernmental relations?

In this paper, I both corroborate and qualify existing evidence of local responsiveness using data on all local governments in the U.S. between 1957 and 2012. Using results from all elections for president, governor, and senator, I construct a county-level measure of Democratic vote share to proxy for voter preferences. Next, using the federal Census of Governments, I measure total local government spending and revenue by aggregating over all municipal, county, and school and special district governments located within a county's borders. I then estimate the causal impact of local preferences on local policy using a difference in differences design, which controls for unobserved heterogeneity that biases cross-sectional estimates. To rule out time-varying omitted variables, I control for a number of covariates, including demographics, intergovernmental revenue, and migration; I also estimate specifications with state-by-year fixed effects, which absorb any time-varying confounders that vary at the state level, and use a lagged dependent variable specification to estimate a lower bound on the effect.

Consistent with previous results, I find that local preferences have a causal impact on local policy. Over the entire sample, a one-standard deviation change in Democratic vote share causes spending and revenue to increase by around \$180 and \$170, respectively. Yet examining this relationship over time reveals that responsiveness began only in the 1970s, gradually increasing ever since. Moreover, responsiveness is not the same for all local policies. On the spending side, police, fire, and infrastructure spending do respond, but education, health, welfare, and corrections do not. And in contrast to past results, the mix of revenue sources – reliance on more regressive sales taxes versus less regressive property taxes – does not respond to local preferences once state factors are taken into account.

As most government in the United States is local government, the study of local responsiveness has broad implications for representation. This paper contributes to our understanding of representation, offering a more comprehensive and nuanced account that looks across time and policy areas, and breaks out of the restrictive focus on large cities. My results also help to establish the limits of local responsiveness. While Peterson may have overstated the degree to which local governments are constrained by state policies, more recent works may have overlooked important policy areas where local governments are indeed limited.

These results also have implications for the study of polarization, particularly geographic sorting. Although the basic facts, causes, and effects of polarization have been extensively explored, the local policy implications of this national trend have not. This may be due to a longstanding assumption that local politics in the United States is conducted independently of national politics, and is thus immune to national polarization. My results cast doubt on this assumption – at least in the contemporary era – and call for more scholarly attention to be paid to national polarization's effects on subnational governments. Put another way, while local governments can not fully escape the influence of state laws, they are also not immune to national political trends.

Local Government Responsiveness

Until recently, most scholarship on local government assumed that local officials were unresponsive to citizen preferences. Statutorily, the power of local governments is greatly limited by state legislatures (Peterson 1981; Allard, Burns, and Gamm 1998), while local elections are seen as low-salience affairs with little hopes of enforcing accountability (Berry and Howell 2007). If these characterizations of local politics are true, then it makes little sense to look for evidence of local reactions to shifts in the preferences of local voters.

However, in recent years a host of studies find evidence of static policy responsiveness in local government. Most prominently, Tausanovitch and Warshaw (2014) find a robust relationship between citizen ideology in cities, estimated using multi-level reweighting and post-stratification, and cities' taxing and spending behavior; while Einstein and Kogan (2016) find a similarly positive relationship when measuring preferences using Democratic vote share in the 2008 presidential election. Similarly, Hajnal and Trounstine (2010) find that county-level Democratic vote share is associated with redistributive spending in a sample of over 7,000 cities. In studies using somewhat more limited samples, Palus (2010) finds an aggregated ideology measure is related to spending patterns in 26 urban areas, and Choi et al. (2010) and Percival, Johnson, and Neiman (2009) find that measures of local liberalism are associated with higher spending by county governments in Florida and California, respectively.

Yet reviewing the existing evidence of local responsiveness also calls attention to the static nature of nearly all existing analyses. Of the studies reviewed, only Einstein and Kogan (2016) examine how within-locality changes in public opinion lead to within-locality changes in policy, and in this case only for two states, California and Wisconsin, between 1982 and 2004.[1] This

[1]Hajnal and Trounstine (2010) analyze data for four years between 1986 and 2001, yet they do not examine within-city variation. The authors report conducting a pooled cross-sectional regression with year fixed effects, but not city fixed effects, and do not report using lagged dependent variables (1140).

focus on cross-sectional variation not only limits our ability to learn how responsiveness may have changed over time, but it also raises the possibility that the observed relationship between opinion and policy is spurious. While all the studies mentioned above conduct multivariate analysis, examining the relationship between opinion and policy while adjusting for other factors, this design relies on the assumption that all potential confounding variables have been properly measured and included. It also assumes that the causal arrow runs from opinion to policy, and not the other way around. This assumption may be violated if – as a long tradition in the field of public economics argues – voters move to communities with policies they already agree with, rather than inducing responsiveness after they have moved (Tiebout 1956). Alternatively, if citizens simply adopt the views of local elites (Lenz 2012), then policy will look "responsive" in a cross-sectional analysis even when it is no such thing.

Existing studies have also focused either on city policy, on one hand, or county government policy on the other. This excludes the vast majority of local governments in the United States: there are currently over 90,000, of which municipalities account for 19,000 and counties 3,000.[2] While scholars have begun to document the overall growth in local government spending since the 1950s (Berry, Grogger, and West 2015), the connection between aggregate local policy and opinion has not been explored.

The results of existing studies are also, in part, difficult to reconcile with the realities of federalism. For instance, two studies find a connection between local preferences and reliance on the sales tax (Tausanovitch and Warshaw 2014; Einstein and Kogan 2016). The theoretical argument is that because sales taxes are more regressive, more liberal voters will prefer that their officials rely on it less. Legally, however, the use of the local sales tax is heavily regulated by state legislatures. Only 33 states explicitly authorize local governments to levy a sales tax, while some others allow selected "home rule" jurisdictions to do so without the legislature's explicit consent (National League of Cities 2016). Even when a sales tax is present, however, the maximum rate

[2]The 2012 Census of Governments reports 16,000 township governments, 13,000 school district governments, and 38,000 special district governments (Hogue 2013).

is typically set by the state legislature. In 1962, the Advisory Committee on Intergovernmental Relations noted this maximum rate ranged from 0.5 percent to 3 percent across the states.[3] Thus, even though Democrats and Republicans may favor different types of local taxes (Einstein and Kogan 2016), local governments do not legally have much discretion in their ability to respond to local preferences in this domain.[4]

Local governments are also constrained in what they spend. Some important local functions, such as education spending, are heavily subsidized via state aid; additionally, almost all states legally require school district spending to be equalized (with varying degrees of success) across districts (Jackson et al. 2016). Other functions, such as welfare spending, involve local governments often simply acting as administrative agents for state and federal programs such as TANF (welfare) and, in ten states, SNAP (food stamps). In contrast, public safety and the maintenance of roads are strictly local functions over which local officials conceivably have more discretion. While these legal realities suggest important variations in government responsiveness across policy areas, most existing studies have examined only total spending.

[3] These caps are evidently still in place across states. For instance, in Massachusetts localities are permitted only to adopt a sales tax on meals, which must be 0.75 percent (Massachusetts Budget and Policy Center 2013). In North Carolina, counties can levy a sales tax up to 2.5% (Huie 2014); in California, the statewide cap is set at 2% (California State Board of Equalization 2016)

[4] Why then have existing studies found such relationships? One possibility is omitted variable bias. More conservative voters may also live in states that authorize cities to raise more revenue from the sales tax. It may be that all cities, if they had the chance, would raise a greater share of revenue from this tax. Then the true cause of the sales tax share is the state law, but we mistakenly attribute it to voter preferences. A way to account for this explanation is by controlling for state policies – does more local conservatism lead to a greater share of sales revenue, holding state laws fixed? While Einstein and Kogan (2016) do control for state fixed effects, they are unable to adjust for county and year effects given the cross-sectional nature of their data.

Data and Measures

Local Preferences

Measuring the underlying preferences of voters within a given geographic area is a longstanding issue in political science (Erikson, Wright, and McIver 1993; Lax and Phillips 2009). While recent years have seen the development of multi-level reweighting and post-stratification (MRP) (Park, Gelman, and Bafumi 2004) to generate estimates of city preferences (Tausanovitch and Warshaw 2013, 2014) these estimates are only feasible in the contemporary period. As an alternative, I follow several past studies of local responsiveness (e.g., Hajnal and Trounstine 2010; Einstein and Kogan 2016) and measure preferences using the share of the two-party vote received by the Democratic candidate in recent elections. This measure not only has the advantage of being available for many years into the past, but it is also based on administrative data and is arguably less prone to measurement error than survey responses, which with MRP estimates often come from only a small number of responses per geographic area. Practically speaking, in the Online Appendix, I show that 2008 Democratic presidential vote share is correlated with the MRP-based ideology measure at .86 in the Tausanovitch and Warshaw (2014) sample cities.

Abrams and Fiorina (2012) argue presidential vote share may inaccurately measure local preferences, as it excludes statewide races that often see cross-over voting. I avoid this issue by computing a "normal vote" measure for each county in each time period by averaging over election results for president, governor, and federal senator, for all even-year elections between 1950 and 2012. Following Ansolabehere and Snyder (2006), I measure a county's partisanship at year t by averaging the two-party vote received by the Democratic candidate in all races for president, U.S. Senator, and governor over eight years prior to t. For instance, for 1962, I average over the elections from 1954, 1956, 1958, and 1960; for 1967, I average results from 1960, 1962, 1964, and 1966. I calculate Democratic vote at at five-year intervals, as data on local fiscal policy are released by the Census every five years, in years ending in two and seven.[5]

[5] I obtained county-level election data between 1950 and 1990 from the Inter-university Consortium

Figure 1: Variation in local preferences, 1957-2012.

An advantage of using over-time data for studying representation is that we can actually observe large changes in preferences over time. To demonstrate this variation, I plot the population-weighted standard deviation of Democratic vote, by year, in Figure 1. I plot separate time series for the South[6] and the non-South, as the South's one-party status prior to the 1960s is well-known. In both regions, counties have seen an increase in dispersion for Democratic vote share, particularly since the 1970s. The standard deviation of the Democratic vote in the non-South was about nine percentage points in 1957, then fell to slightly less than eight in 1967. In that year, the standard

for Political and Social Research (ICPSR; studies 1 and 13), and between 1992 and 2012 from Congressional Quarterly's Voting and Elections Collection.

[6]Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia.

deviation began gradually rising, until hitting about thirteen points in 2012. In the South, we see a parallel increase in sorting since 1977, but preceded by a decline in the standard deviation between 1957 and 1972. As might be expected, and as shown in the Online Appendix, the higher standard deviations for the South in the earlier years are due to the lopsided nature of election results in that region in those years. Putting aside these regional dynamics, it is evident that all counties have become more ideologically sorted since the 1970s. In particular, in the 1970s a given county would vote Democratic about eight percentage points more or less than the average county; by 2012, this difference had increased to between twelve and fourteen points.[7]

It is important to acknowledge that – given the lack of detailed, small-area opinion estimates going back in time – the use of Democratic vote share as a measure of opinion means any analysis of representation will be somewhat limited. While it will be possible to demonstrate that policy *responds* to opinion, the measure does not allow us to assess *congruence*, or the distance between what voters want and what policy is (Erikson, Wright, and McIver 1993; Matsusaka 2010). It is possible, for instance, that local governments over-respond to an increase in voter liberalism, increasing spending more than voters desire (Bafumi and Herron 2010). Exploring whether *any* responsiveness occurs, and how it has changed over time, however, is itself an important first step in understanding the historical dynamics of local democracy.

[7]This pattern is consistent with several other studies of geographic sorting. For instance, Nall (2015) finds that the difference in Democratic vote share between urban and rural areas consistently increased between 1932 and 2012. Theriault (2008), McCarty, Poole, and Rosenthal (2009), and Sussell and Thomson (2015) all find that U.S. House districts have become increasingly lopsided since the 1950s. At the county level, Bishop and Cushing (2008) famously show that there were significantly more "landslide" counties in the 2004 presidential election than in the 1972 election. While Abrams and Fiorina (2012) note that drawing conclusions based on just two time periods is problematic, several other scholars have found consistent increases in measures of county-level sorting in every year since the 1970s (Glaeser and Ward 2006; Bishop 2012; Sussell and Thomson 2015).

Local Government Policy

To measure local government policy, I follow past cross-sectional studies by examining spending (per capita) and own-source revenue (per capita). As noted by Tausanovitch and Warshaw (2014), total government spending reflects the "scope of government," the central ideological divide between the two major political parties in the United States (Poole and Rosenthal 2007). I examine own-source revenue as it also taps into a key political issue, namely taxation and what citizens are asked to pay for services;[8] it is more plausibly under the control of local officials, whereas total spending is influenced in part by state and federal aid and mandates.

In contrast to most past studies that have examined local policy, I aggregate all three measures across all local governments – including municipalities, school and special districts, and county governments – located with an individual county. I obtain these data from the Census of Governments, which releases data on spending and revenue categories, aggregated across all local governments to the county level, every five years. These county aggregates are available back to 1957, and I begin my analysis in this year (United States Census Bureau 2008).[9]

[8] According to the 2012 Census of Governments, 92% of local own-source revenue came from either taxes (over 64%) or "charges" (28%), including user fees and other payments made by the public (Barnett et al. 2014; United States Census Bureau 2016).

[9] I accessed the Census of Governments data online at https://www2.census.gov/pub/outgoing/govs/special60/. Data up to 2002 are located in the file `County_Area_Fin.zip`, while data for 2007 and 2012 are located in the files `county_area_finances_2007.zip` and `county_area_finances_2012.zip` (accessed July 22, 2016). Previous studies by political scientists utilizing these data include Anzia and Moe (2015) (using the city-level panel), Berry, Grogger, and West (2015), and Ansolabehere and Snyder (2006) (both using the county aggregates panel). Previous studies using Census of Governments cross-sections include Minkoff (2009) (using the 1997 data). Before 1972, the data do not include specific entries for total expenditures, total own-source revenues, and total sales taxes for all counties. I therefore use measures that are present in all years back to 1957: total direct general expenditures, instead of total ex-

Additional Variables

I also collect county-level data on several other variables that may influence both public opinion and public policy, or may be correlated with within-county, over-time changes in either. These variables include population, population density, median family income (Peltzman 1980), the share of residents who are white (Massey and Denton 1988), the share of residents who are aged 65 or older (Poterba 1997), the number of local governments in a county (Berry 2008), total transfer revenues from the state (per capita), and a measure of migration (the actual population less the expected population based on birth rates, divided by the expected population; see Winkler et al. 2013). Aside from the number of governments and intergovernmental revenue, all variables are only available every ten years; I linearly interpolate values in non-Census years.[10]

---

penditures; and total own-source general revenues, instead of total own-source revenues. Direct general spending includes 90% of total spending on average, and excludes only intergovernmental spending, utility expenditure, liquor store expenditure, and insurance trust expenditure (e.g., unemployment compensation) (Barnett et al. 2014; United States Census Bureau 2016). General revenue is similarly defined, with 87% of own-source revenue classified as "general."

[10] I obtained figures on county population, race, and age from the U.S. Census via Social Explorer (see http://www.socialexplorer.com). I obtained median family income figures from the Census via Social Explorer for the years 2000 and 2010, the Census web site for the years 1960, 1970, 1980, and 1990, and from the Census 1952 County and City Data Book (via ICPSR, study 12) for 1950. I obtained figures on the number of local governments back to 1957 from the file `2_Govt_Org_County_Area_Counts.xls`, contained in the compressed archive `1-3_Govt_Org_Nat_CoArea_ElecOff.zip`, downloaded from the page at https://www2.census.gov/pub/outgoing/govs/special60/ (accessed July 22, 2016). State transfer revenue comes from the same source as the expenditure and revenue data, and migration figures are from Winkler et al. (2013).

Summary Statistics

According to the federal government, there are currently 3,144 counties or county-equivalent units in the United States. After excluding observations with missing data for one or more variables in one or more years, my final sample consists of 3,001 counties over twelve years, or every five years from 1957 to 2012. These observations come from all 48 of the continental states; I exclude Alaska and Hawaii (36 counties) only because fiscal data are unavailable for the former prior to 1967, and income is missing for the latter in 1950. The remaining 107 counties are excluded if they are missing data on any of the outcomes or control variables; most (84 counties) are excluded due to missingness on the migration variable.

I show means and standard deviations for all variables used in the analysis, separately by year, in Table 1. In this table and throughout the paper, I weight observations by county population. I also adjust all monetary variables for inflation, presenting them in real 2014 dollars. For presentational purposes, I present proportion variables on a zero to one hundred scale, divide population, spending, and revenue by 1,000, and scale the number of local governments by 100,000 persons.

	1957	1962	1967	1972	1977	1982
Spending	1.31	1.69	2.14	2.90	3.03	2.80
(real $1,000's per capita)	(0.46)	(0.57)	(0.74)	(1.26)	(1.12)	(0.91)
Revenue	0.88	1.14	1.38	1.79	1.82	1.73
(real $1,000's per capita)	(0.39)	(0.47)	(0.57)	(0.76)	(0.82)	(0.72)
Democratic vote	51.70	53.50	53.16	52.99	51.10	52.33
(percentage points)	(14.21)	(12.20)	(8.76)	(8.16)	(8.08)	(8.31)
Population	55.32	59.10	62.88	66.62	70.31	73.91
(1,000's)	(234.47)	(245.09)	(256.61)	(263.76)	(264.27)	(270.32)
Median family income	38.32	45.27	53.22	59.19	62.23	65.05
(real $1,000's)	(9.57)	(10.67)	(11.59)	(12.07)	(11.90)	(12.65)
Share white	89.18	88.75	88.37	87.22	84.95	83.05
(percentage points)	(12.08)	(11.80)	(11.70)	(11.94)	(12.88)	(13.89)
Share elderly	9.10	9.46	9.80	10.28	10.97	11.65
(percentage points)	(2.57)	(2.64)	(2.88)	(3.13)	(3.27)	(3.41)
Number of govs	60.89	50.82	42.50	38.56	37.28	36.53
(per 100,000 persons)	(127.98)	(102.58)	(83.74)	(70.25)	(68.06)	(67.08)
Population density	0.13	0.13	0.14	0.14	0.14	0.15
(1,000's per sq. mi.)	(0.83)	(0.83)	(0.82)	(0.81)	(0.77)	(0.75)
State revenue	0.36	0.47	0.67	0.97	1.09	1.02
(real $1,000's per capita)	(0.19)	(0.21)	(0.34)	(0.55)	(0.58)	(0.47)
Migration	91.35	41.92	60.91	64.18	87.44	54.43
(as % of expected pop.)	(421.35)	(245.85)	(263.27)	(293.98)	(319.66)	(221.42)

	1987	1992	1997	2002	2007	2012
Spending	3.35	3.73	3.92	4.52	4.90	4.65
(real $1,000's per capita)	(1.10)	(1.24)	(1.22)	(1.42)	(1.57)	(1.61)
Revenue	2.18	2.37	2.49	2.73	3.18	2.99
(real $1,000's per capita)	(0.97)	(1.00)	(1.02)	(1.07)	(1.34)	(1.37)
Democratic vote	49.83	48.79	51.07	49.47	50.83	51.24
(percentage points)	(9.62)	(10.42)	(10.57)	(11.64)	(12.84)	(13.77)
Population	77.38	81.54	86.75	91.60	95.92	100.25
(1,000's)	(283.93)	(298.55)	(314.03)	(327.39)	(337.44)	(348.18)
Median family income	67.41	69.15	69.85	70.66	71.54	69.31
(real $1,000's)	(14.83)	(16.29)	(16.17)	(16.41)	(16.96)	(17.00)
Share white	81.59	79.69	77.12	75.04	73.66	72.24
(percentage points)	(14.54)	(15.28)	(16.06)	(16.50)	(16.38)	(16.46)
Share elderly	12.29	12.62	12.55	12.62	12.91	13.22
(percentage points)	(3.54)	(3.63)	(3.55)	(3.49)	(3.45)	(3.49)
Number of govs	35.23	34.13	33.01	31.27	30.43	29.22
(per 100,000 persons)	(67.23)	(66.70)	(65.93)	(64.38)	(63.73)	(62.81)
Population density	0.15	0.16	0.17	0.17	0.18	0.18
(1,000's per sq. mi.)	(0.75)	(0.76)	(0.78)	(0.80)	(0.81)	(0.82)
State revenue	1.19	1.32	1.42	1.65	1.72	1.56
(real $1,000's per capita)	(0.54)	(0.63)	(0.59)	(0.72)	(0.73)	(0.66)
Migration	68.99	87.09	98.11	81.14	91.20	8.28
(as % of expected pop.)	(235.82)	(200.50)	(212.21)	(187.82)	(196.65)	(411.51)

Table 1: Summary statistics. Cell entries are yearly averages, with standard deviations in parentheses. All statistics are weighted by *population* (with the exceptions of *population* and *population density*). There are 3,001 county observations for each cell entry in each separate year and 36,012 observations in total.

The Effect of Local Preferences on Policy

To study the causal relationship between preferences and policy, I use a regression with fixed effects for year and county. The use of time and unit fixed effects implies the estimated coefficient can be interpreted as the "difference in differences" (Angrist and Pischke 2009), or how the outcomes change within a county over time, when the "treatment" of Democratic vote changes within a county over time. The within-unit comparison ensures that any fixed characteristics of counties – including latent political culture or state laws that do not change over time – are also held constant. Unlike previous analyses that rely on cross-sectional variation, this means we can be more certain that the observed relationship between partisanship and policy is causal, and not the result of other factors.

Formally, I estimate:

$$\text{Policy}_{jst} = \beta * \text{Democratic vote}_{jst} + \text{County}_j + \text{Year}_t + \text{State}_s \times \text{Year}_t + \sum_{k=1}^{K} \pi^k * x^k_{jst} + \varepsilon_{jst}$$

where Policy_{jst} represents the policy measure for county j in state s in year t, County_j is a county fixed effect, Year_t is a year fixed effect, the K x^k_{jt} variables represent the covariates discussed above, and ε_{jst} is a random error term. The K covariates adjust for confounding variables that change over time in conjunction with Democratic vote. As shown above, counties have also experienced changes in terms of income and race since the 1970s.

To further protect against time-varying confounders, in some specifications I also include fixed effects for each state-year combination ($\text{State}_s \times \text{Year}_t$ in the equation above). In such specifications, both the outcome and Democratic vote are expressed as deviations from the average in year t among all counties located in county j's home state. These fixed effects should also capture a good deal of other geographic sorting occurring in the population, as well as any changes in state factors, including laws, that occur over time. In all specifications, I cluster standard errors at the county level to account for within-county dependence across time, and I weight observations by county population. I also express all predictor variables in terms of standard deviations for presentational

	Spending			Own-source revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Democratic vote	0.27***	0.10***	0.18***	0.18*	0.11**	0.17***
	(0.08)	(0.03)	(0.03)	(0.08)	(0.03)	(0.03)
Median family income		0.28***	0.45***		0.31***	0.49***
		(0.07)	(0.06)		(0.07)	(0.05)
Share white		-0.22**	-0.29***		-0.26*	-0.35***
		(0.08)	(0.05)		(0.11)	(0.06)
Share elderly		-0.07	-0.03		-0.04	0.02
		(0.06)	(0.04)		(0.06)	(0.04)
Number of govs		0.20***	0.14***		0.21***	0.12**
		(0.05)	(0.04)		(0.05)	(0.04)
Ln population density		-0.33*	-0.53***		-0.29	-0.40***
		(0.14)	(0.13)		(0.15)	(0.12)
State revenue		0.55***	0.57***		-0.01	0.07*
		(0.07)	(0.04)		(0.07)	(0.03)
Migration		-0.01	-0.00		-0.02	-0.01
		(0.02)	(0.01)		(0.03)	(0.01)
Fixed effects:						
County	Y	Y	Y	Y	Y	Y
Year	Y	Y		Y	Y	
State-year			Y			Y

Table 2: The effect of local preferences on local policy. Cell entries are estimated coefficients from linear regressions, with standard errors in parentheses. There are 12 unique years (1957-2012, at five-year intervals), 3,001 unique counties, and 36,012 county-year observations in each column. Standard errors are clustered at the county level, and observations are weighted by population.

purposes.[11]

Table 2 shows the results, with three specifications presented for each of the policy variables. The estimate in column (1) implies that as a county increases its Democratic vote by one standard deviation over a five-year period, it increases per-capita expenditures by $270 (standard error of $80), relative to its pre-change baseline and relative to the change in spending among counties

[11]In practice, when state-year fixed effects are included, the year fixed effects will drop out due to collinearity. In the Online Appendix I show the results are robust to excluding weights.

that did not change their Democratic vote over the same period. Adding covariates in the second column reduces the estimate to $100 ($30), while adding state-year fixed effects in the third column increases the estimate to $180 ($30).

Columns (4) through (6) repeat the analysis for own-source revenue. As counties become more Democratic by one standard deviation, they increase their per-capita own-source revenue by between $110 ($80) in the specification in column (5), to $180 ($80) in the specification in column (4).

These estimates suggest that local policy does causally respond to local preferences. However, the regression estimated above assumes that partisanship is independent of any confounding variables after conditioning on the other predictors (the fixed effects and the time-varying covariates). This assumption is violated if partisanship at time t is itself a function of policy at time $t-1$. In this case, the error term in the regression will contain $\text{Outcome}_{j,t-1}$, biasing the estimate of β. It is not hard to imagine a scenario in which local government policy impacts local preferences. Beginning with Tiebout (1956), a long line of mostly theoretical work in public economics has argued that citizens may impose discipline on local officials by moving to areas with local policies they like, and out of areas with policies they do not like, in effect "voting with their feet." In this scenario, preferences follow policies, and not the other way around.

Empirical evidence for Tiebout sorting is limited. In a review of the Tiebout model's empirical evidence, Oates (2006) points mainly to the so-called capitalization effect – that real estate values respond to the quality of local services such as education – as empirical evidence. Direct evidence that voters select communities based on local policy is more rare. In political science, Mummolo and Nall (2016) conduct a series of survey experiments to estimate the marginal impact of numerous factors that play into residential choice, including partisanship and local policy. They find that Democrats and Republicans both place roughly equal weight on property tax rates and the presence of local sales taxes; ultimately, Mummolo and Nall conclude that even if partisans did want to sort based on ideology (whether in terms of the ideological content of policy or the preferences of people who live there), they are constrained based on desires for service quality and ability to pay.

Even scholars who do argue that partisans do intentionally migrate to areas with more co-partisans argue that national, rather than local policy concerns motivate sorting (Tam Cho, Gimpel, and Hui 2013).

Including migration as a covariate in the above regressions should go a good ways toward ruling out Tiebout sorting. Additionally, I account for the general reverse causality explanation by adding a lagged outcome variable to the regression:

$$\text{Policy}_{jst} = \beta * \text{Democratic vote}_{jst} + \text{Policy}_{js,t-5} + \text{Year}_t + \text{State}_s \times \text{Year}_t + \sum_{k=1}^{K} \pi^k * x^k_{jst} + \varepsilon_{jst}$$

I exclude the county fixed effect from this specification, as this would induce a correlation between the lagged outcome and the error term (Angrist and Pischke 2009).[12] I use this regression, in conjunction with the fixed effects specification employed earlier, to calculate bounds on the causal effect of Democratic vote. Angrist and Pischke (2009) show, following Guryan (2001), that under some assumptions about the nature of selection bias, the fixed effects and lagged outcome regressions will provide upper and lower bounds on the true causal effect. In particular, if selection into the treatment is driven by fixed characteristics of counties, but we mistakenly exclude fixed effects and include a lagged outcome, then the lagged outcome regression provides a lower bound. Alternatively, if selection is driven by past values of the outcome, but we mistakenly estimate the fixed effects regression, then the fixed effects regression provides an upper bound.[13]

I present the bounds for each outcome variable in Table 3. The first two columns present the bounds for the effect on spending. The estimate from the lagged outcome regression indicates a lower bound on the true effect of \$40, with a standard error of \$10. The fixed effects estimate, which provides an upper bound, is \$170 (\$30).[14] The estimates and bound widths for revenue are

[12]In practice, the estimates are similar when including both a lagged outcome and fixed effects.

[13]See Holbein and Hillygus (2015) and Caughey and Warshaw (2014) for two recent examples of this bounding strategy in political science.

[14]Note that in these fixed effects regressions I exclude 1957, given that the first year with a value for lagged outcomes is 1962. If I did not exclude 1957, then the difference between the two

	Spending		Own-source revenue	
	Lower	Upper	Lower	Upper
Democratic vote	0.04***	0.17***	0.03***	0.17***
	(0.01)	(0.03)	(0.01)	(0.03)
Covariates	Y	Y	Y	Y
Lagged outcome	Y		Y	
Fixed effects:				
County		Y		Y
State-year	Y	Y	Y	Y

Table 3: Accounting for reverse causality: bounding the causal effects of partisanship. Cell entries are estimated coefficients from linear regressions, with standard errors in parentheses. There are 11 unique years (1962-2012, at five-year intervals), 3,001 unique counties, and 33,011 county-year observations in each column. Standard errors are clustered at the county level, and observations are weighted by population.

similar: the lower bound is $30 ($10), and the upper bound is $170 ($30).

In the Online Appendix, I also present several additional checks on the results. First, I show that the estimated effects are substantively the same whether the sample is restricted to the South or the non-South. Second, I present estimates that drop the population weights, and instead control for population directly. The estimates for revenue and spending are positive, and are generally statistically significant (with the exception of own-source revenue in the all-county sample).

The Growth of Local Responsiveness

To explore changes in responsiveness over time, I first present the bivariate relationship between spending and Democratic vote, separately by year from 1957 to 2012 (results for revenue are similar, and are shown in the Online Appendix). Because there are 3,001 observations per year, I summarize the data by collapsing the outcomes into one hundred percentile bins of Democratic vote share, where I generate the bins separately by year. I then plot these binned averages, along

estimates could be due to differences in samples.

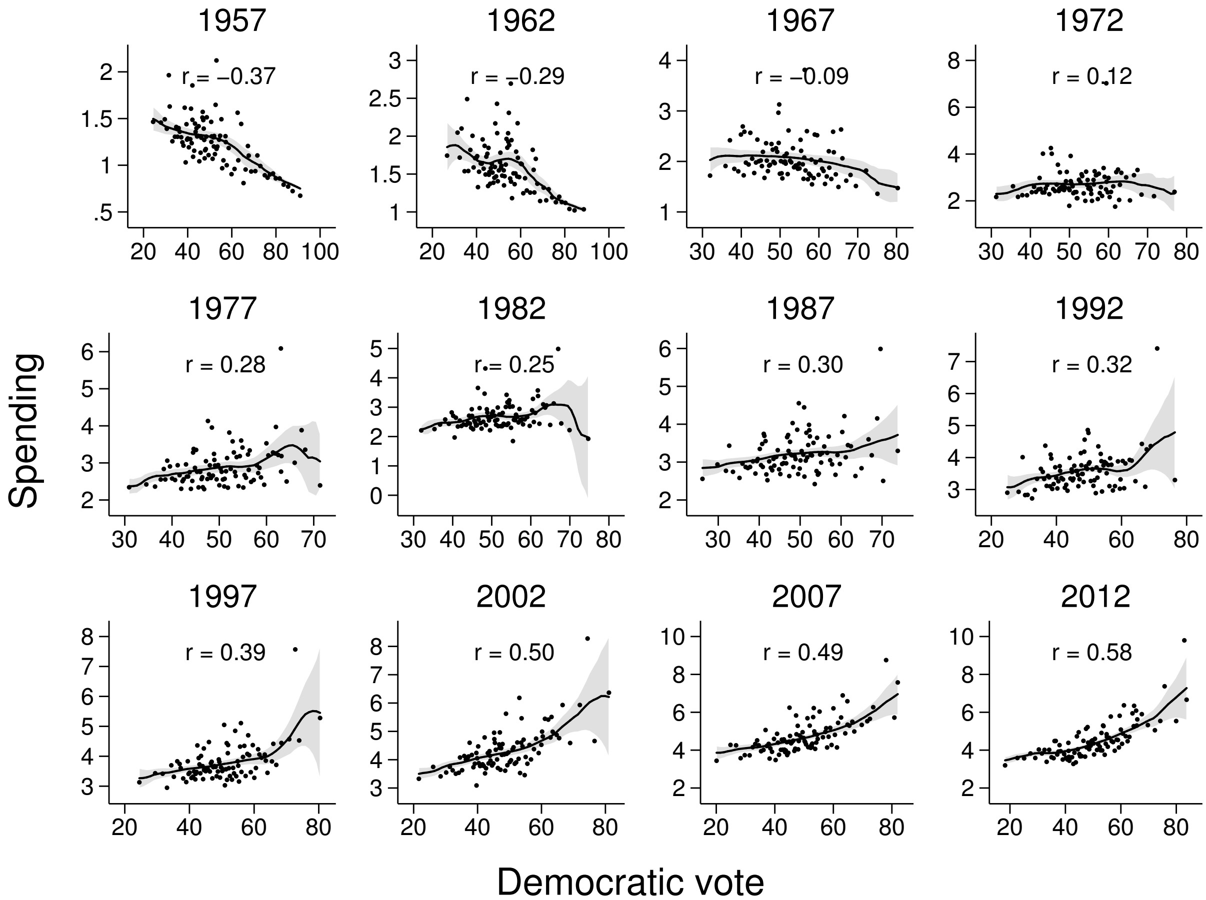


Figure 2: Spending and Democratic vote, 1957-2012. Points are local averages generated within each percentile of Democratic vote. Solid lines are local polynomial fits, shaded areas representing 95% confidence bands.

with a local polynomial smoother to illustrate trends. In the center of each plot, I report the bivariate correlation between the outcome and Democratic vote share for that year, calculated prior to any binning.

Figure 2 shows that in 1957, there was in fact a negative correlation (r = -.37) between spending and Democratic vote. In fact, only beginning in 1972 was there a positive relationship, with a correlation of 0.12. Since 1972, the correlation between local spending and Democratic voting has steadily increased, rising to as much as 0.58 in 2012. Visually, one can start to detect a positive relationship beginning in 1977, with the clearest positive trend in the 2000s.

Before drawing broad conclusions from these plots, however, it is worth noting that these relationships are still cross-sectional, and thus only suggestive regarding the causal relationship between preferences and policy. To more rigorously estimate the time-varying impact of Democratic vote, I estimate an additional specification where I interact the Democratic vote variable with the year fixed effects:

$$\begin{aligned}\text{Policy}_{jst} &= \beta^{1957} * \text{Democratic vote}_{jst} + \sum_{l=1962}^{2012} \beta^{l} * \text{Democratic vote}_{jst} \times \text{Year}_{l} + \text{County}_{j} + \\ &\quad \text{Year}_{t} + \sum_{k=1}^{K} \pi^{k} * x^{k}_{jst} + \varepsilon_{jst}\end{aligned}$$

In this specification, β^{1957} represents the impact of Democratic vote in 1962, and the eleven β^{l} terms represent the additional impact for each of the subsequent years. I do not include state-by-year fixed effects in this specification, as they would be perfectly collinear with the year fixed effects, which are key here for estimating over-time trends in responsiveness.

I plot the twelve β coefficients, for each policy outcome, in Figure 3.[15] The general pattern of results is consistent with the bivariate plots. There was no detectable impact of local preferences on spending until the 1970s; while positive and of a similar magnitude in the 1980s and 1990s, the effects do not become significant again until 2002. In that year and in 2007, the estimate is about

[15] To be more precise, the equation above implies that the effect of Democratic vote in year t is β^{t} for $t = 1957$ and $\beta^{1957} + \beta^{t}$ for $t > 1957$. To see this, let d and d' be two arbitrary values of Democratic vote, and take the example of the effect in 1962.

$$\begin{aligned}& \text{E}[\text{Policy}_{jt} | \text{Democratic vote}_{jt} = d, \text{Year=1962}] \\ - \; & \text{E}[\text{Policy}_{jt} | \text{Democratic vote}_{jt} = d', \text{Year=1962}] \\ = \; & (\beta^{1957} * d + \beta^{1962} * d + ...) - (\beta^{1957} * d' + \beta^{1962} * d' + ...) \\ = \; & (d - d') * (\beta^{1957} + \beta^{1962})\end{aligned}$$

where the ellipses (...) stand in for the covariates and fixed effects. I therefore add the coefficients accordingly in generating these plots.

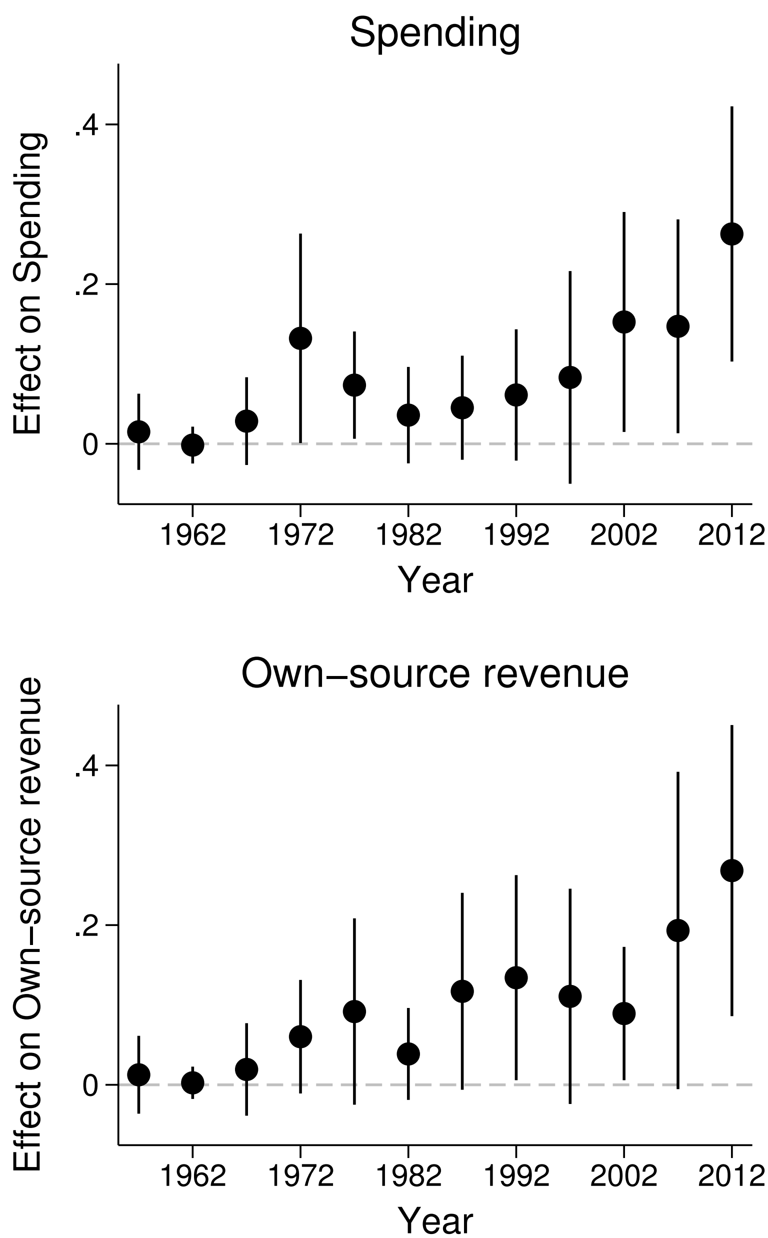


Figure 3: Local responsiveness over time.

$150 increase in spending for a one standard deviation increase in Democratic vote; in 2012, the estimate rises to $260. The bottom panel shows similar results for own-source revenue.

The Limits of Local Responsiveness

Theoretically, local governments are not entirely free to respond to local preferences. Citizens may, for instance, desire a program of direct cash assistance for the economically unfortunate; in the United States, however, such public assistance programs are the domain of the state and federal governments. Generally speaking, some spending areas are more constrained than others by state legislation. Education, public safety, and transportation are primarily local affairs, while health, welfare, and corrections are primarily state affairs. While local governments may spend on the latter categories, their activities may still be limited by state regulations. For example, a significant share of local spending is on health and hospitals; yet, much of this spending may be on "safety net" hospitals that are funded to a significant extent by the state-federal Medicaid program. Even education, historically a primarily local responsibility, is increasingly subject to state and federal mandates, including the common legal requirement that education spending is roughly equalized across districts (Jackson et al. 2016). And as discussed above, the presence and maximum rate for the sales tax is also restricted by state laws, potentially limiting the choice of revenue instruments.

Outcome	Share	No covariates b	(s.e.)	+ Covariates b	(s.e.)	+ State-year effects b	(s.e.)
Spending							
(1) Total spending	100	0.24***	(0.07)	0.11***	(0.03)	0.17***	0.03
(2) Education	46	0.05**	(0.02)	0.01	(0.01)	0.00	0.01
(3) All other	25	0.12***	(0.03)	0.07***	(0.02)	0.09***	0.02
(4) Health and hospitals	7	0.01	(0.01)	-0.01	(0.01)	0.03	0.02
(5) Highways	6	0.00	(0.00)	0.01*	(0.00)	0.01**	0.00
(6) Police	5	0.02***	(0.00)	0.01***	(0.00)	0.01***	0.00
(7) Welfare	4	0.00	(0.00)	0.00	(0.00)	0.00	0.00
(8) Fire	3	0.01***	(0.00)	0.01***	(0.00)	0.01***	0.00
(9) Housing	3	0.03***	(0.01)	0.01***	(0.00)	0.02***	0.00
(10) Corrections	1	0.01	(0.00)	0.00	(0.00)	0.00	0.00
Revenue							
(11) Total revenue	100	0.17*	(0.07)	0.12**	(0.04)	0.17***	0.03
(12) Property tax	78	0.06*	(0.03)	0.08***	(0.02)	0.11***	0.02
(13) Non-property tax	22	0.12	(0.08)	0.04	(0.03)	0.07***	0.02
(14) Share non-property tax		-0.06	(0.43)	-0.64*	(0.28)	0.33	0.31

Table 4: Responsiveness by spending and revenue category. Cell entries are estimated coefficients from linear regressions, with standard errors in parentheses. There are 11 unique years (1962-2012, at five-year intervals), 3,001 unique counties, and 33,011 county-year observations in each column. Standard errors are clustered at the county level, and observations are weighted by population. The "No covariates" and "+Covariates" columns report estimates from specifications with county and year effects; the "+State-Year Effects" column includes covariates, county fixed effects, and state-year fixed effects.

To explore which categories of local spending and revenue are most responsive, I repeat the analysis using several of the largest categories of direct expenditure: education (42% of total direct expenditure in 2012), health and hospitals (9.4%), police (6%), highways (4.3%), public welfare (3.7%), housing (3%), fire protection (3%), correction (1.9%), and all other spending (including sewerage, waste management, general administration, air transport, natural resources, parks, libraries, and a residual category). To explore which revenue categories are responsive, I divide total own source revenues into property tax and non-property tax revenues. The latter variable should capture sales taxes and other, similarly more regressive sources, such as charges and fees.[16] Because many of the variables capturing specific spending and revenue categories are only available beginning in 1962, I focus only on 1962-2012 for this part of the analysis.

Table 4 shows the results for each spending and revenue variable, using the same three specifications reported in Table 2 above – a difference in differences specification with county and year effects but without covariates (the "No covariates" columns), a specification that adds the same covariates as before (the "+Covariates" columns), and a specification that includes county fixed effects, covariates, and state-by-year fixed effects (the "+State-year effects" columns). The first column shows the share of total spending or revenue that each category makes up, and the results are sorted from largest to smallest share. To ensure that the results from above replicate using the 11-year sample, rows (1) and (11) use total spending and total own-source revenues as outcome variables.

The results are interesting: although education spending is by far the largest share of local spending in a county – 46% on average – it is only significantly responsive to preferences in the specification without covariates (row 2). When covariates are included, the estimate decreases from a \$50 dollar change (for a one-standard deviation increase in Democratic vote) to a \$10 change, with a standard error of \$10. When state-year effects are included, the estimate rounds to \$0, with a standard error of \$10. The next largest category (after the "all other" category) is health and hospitals, which makes up 7% of total local spending (row 4). It too is unresponsive to local preferences, regardless of the specification: the point estimates vary from \$10 to minus \$10 to \$30, but are never significantly different from zero. Welfare (4%; row 7) and corrections (1%; row 10)

[16]The share of revenue from sales taxes is not available until 1972. However, the average county derives 94% of its total tax revenues from either the sales tax (13%) or the property tax (81%). Non-property tax revenues should therefore be a reliable proxy for sales tax revenues.

are similarly unresponsive.

Aside from education spending, policies that are commonly thought of as the purview of local governments do tend to respond. Spending on highways (row 5) makes up 6% of total spending, and (in the more stringent specifications) increases by $10 for every standard deviation increase in Democratic vote (the standard errors round to $0, but the estimates are statistically significant at the .05 level). Police (row 6), fire (row 8), and housing (row 9) spending are significant regardless of the specification; in the regression with state-year effects, the magnitudes of the police and fire effects are $10, and the housing effect is $20.

On the revenue side, both the property tax and non-property tax revenues appear to respond. The estimates are more precise for property tax revenues, and the effect sizes are between $60 and $110 (row 12). For non-property tax revenues, the estimates are less precise, but the state-year effects specification yields an estimate of $70 with a standard error of $20 (row 13). It is intuitive that the inclusion of state effects would remove a good deal of the variance in sales tax revenue, given the legal limits noted above.

Most importantly, the *share* of revenue from non-property tax revenues only appears responsive to local preferences in the second specification (row 14). Without covariates, the estimate is correctly signed, according to the hypothesis that more liberal voters desire less regressive taxes, but is not significantly different from zero – the estimate is -0.06 percentage points with a large standard error of 0.43 percentage points. With covariates, the estimates imply that a one standard deviation increase in Democratic vote causes a 0.64 percentage point decline in the share of revenues from sales tax, with a standard error of 0.28 percentage points. While statistically significant, this estimate may only be interpreted as a causal effect if we believe that changes in local preferences are independent of time-varying state factors, which seems implausible. Controlling for state laws flips the sign, and renders it statistically insignificant, implying that the local share of sales taxes is unresponsive to local preferences after accounting for state factors.

This analysis reveals local responsiveness, while evident in the aggregate, is not without limits. The largest spending categories show little responsiveness to local preferences; nor does the choice of specific taxes. As a share of total spending, the policies that do respond, while important, are a small share of the total budget, only about 11% when combined. While these results may be disheartening to those hoping for more democracy in local government, they are at least consistent with the realities of American federalism.

Conclusion

While a long-dominant view of local politics suggests accountability is rare given constraints on local officials, recent work consistently finds a cross-sectional relationship between local policy and local opinion. I corroborate these results by leveraging the over-time shift in local preferences caused by geographic sorting. I use a measure of local partisanship that incorporates gubernatorial, senatorial, and presidential elections, and I link this measure to measures of total local spending and revenue in 3,001 counties over twelve years. Distinct from previous work, I am able to credibly estimate the impact of local preferences while holding numerous potential confounding variables constant, and I employ a bounding strategy to account for possible reverse causality.

My results help to reconcile Peterson's (1981) argument with more recent empirical evidence. For instance, one reason why Peterson could characterize empirical evidence of responsiveness as "muddled" is because in 1981, the relationship between local preferences and policy had yet to fully develop. As I show, estimates of responsiveness become the largest and most precise only in the 2000s, the period on which more recent studies focus. And while recent work has shown Peterson's claims of limits to be overly pessimistic, I show they are not wholly without merit. For example, given that state laws determine what local governments can spend on and what types of revenue they can raise, it would be surprising if local preferences were able to overcome these constraints. In contrast to past work, I show that certain spending and revenue policies are indeed limited when taking state laws into account.

A remaining question is what has led to the increase in local responsiveness. One possibility is that local officials have simply become more responsive; however, we have no a priori reason to expect this. If anything, conventional accounts of local elections suggest turnout has decreased over time, which would theoretically make officials less responsive. Alternatively, the consistency of the signals sent by voters to local officials may have become stronger as a result of geographic sorting. The increase in responsiveness may also be a result of the "nationalization" of state and local politics – while voters' preferences for national policy may have been disconnected from their preferences for local policy in the 1960s, the two may have become linked over time. Relatedly, it is also possible that the robust association between opinion and policy is simply a happy coincidence: if local officials are being arbitrarily selected from a very liberal or conservative area, then policy will look very liberal or conservative even in the absence of active electoral pressure (Kogan 2017).

"With socioeconomic variables controlled, these 'political' variables do not predict policy liberalism in the manner once hypothesized." This description of the state responsiveness literature, written by Wright, Erikson, and McIver in 1987, is hard to fathom forty years later, given the powerful relationship between state opinion liberalism and state policy these authors, and many others since, would find when adopting better measurement strategies. It may be that local politics scholars will soon look back upon Peterson's argument of non-responsiveness as similarly incomprehensible. Yet the study of local responsiveness will itself remain limited, should it fail to account for the very real limits that local governments face.

References

Abrams, Samuel J., and Morris P. Fiorina. "'The Big Sort' That Wasn't: A Skeptical Reexamination." *PS: Political Science & Politics* 45(2): 203-210.

Advisory Committee on Intergovernmental Relations. 1962. *State Constitutional and Statutory Restrictions on Local Taxing Powers.* Washington: Advisory Committee on Intergovernmental Relations.

Allard, Scott, Nancy Burns, and Gerald Gamm. 1998. "Representing Urban Interests: The Local Politics of State Legislatures." *Studies in American Political Development* 12(2): 267-302.

Angrist, Joshua David, and Jörn-Steffen Pischke. 2009. *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press.

Ansolabehere, Stephen, and James M. Snyder. 2006. "Party Control of State Government and the Distribution of Public Expenditures." *The Scandinavian Journal of Economics* 108(4): 547-569.

Anzia, Sarah F., and Terry M. Moe. 2015. "Public Sector Unions and the Costs of Government." *Journal of Politics* 77(1): 114-127.

Bafumi, Joseph, and Michael C. Herron. 2010. "Leapfrog Representation and Extremism: A Study of American Voters and Their Members in Congress." *American Political Science Review* 104(3) :519–42.

Barnett, Jeffrey L., Cindy L. Sheckells, Scott Peterson, and Elizabeth M. Tydings. 2014. *2012 Census of Governments: Finance—State and Local Government Summary Report.* United States Census Bureau. Accessed June 27, 2016 via https://www2.census.gov/govs/local/summary_report.pdf.

de Benedictis-Kessner, Justin, and Christopher Warshaw. 2016. "Mayoral Partisanship and the Size of Municipal Government." *Journal of Politics* in press.

Berry, Christopher. 2008. "Piling On: Multilevel Government and the Fiscal Common-Pool." *American Journal of Political Science* 52(4): 802-820.

Berry, Christopher, Jeff Grogger, and Martin West. 2015. "The Growth of Local Government." Working paper, Harris School of Public Policy, University of Chicago. Accessed June 24, 2016 via https://drive.google.com/file/d/0B0mMGz0Nu3bpb0t3VE1wRTlRZVE/view?usp=sharing.

Berry, Christopher R., and William G. Howell. 2007. "Accountability and Local Elections: Rethinking Retrospective Voting." *Journal of Politics* 69(3): 844-858.

Bishop, Bill. 2012. "More on the Big Sort: Bill Bishop Responds to Abrams and Fiorina." *The Monkey Cage* March 21. Accessed June 24, 2016 via http://themonkeycage.org/2012/03/more-on-the-big-sort-bill-bishop-responds-to-abrams-and-fiorina/.

Bishop, Bill, and Robert G. Cushing. 2008. *The Big Sort: Why the Clustering of Like-Minded America is Tearing us Apart*. New York: Houghton Mifflin Harcourt.

Boudreau, Cheryl, Christopher S. Elmendorf, and Scott A. MacKenzie. 2015. "Lost in Space? Information Shortcuts, Spatial Voting, and Local Government Representation." *Political Research Quarterly* 68(4): 843-855.

California State Board of Equalization. 2016. "California City & County Sales & Use Tax Rates." Accessed June 21, 2016 via https://www.boe.ca.gov/sutax/pam71.htm.

Caughey, Devin, and Christopher Warshaw. 2014. "Dynamic Representation in the American States, 1960–2012." Working paper, Department of Political Science, Massachusetts Institute of Technology. Accessed June 27, 2016 via http://caughey.mit.edu/sites/default/files/documents/StateRepresentation141010.pdf.

Choi, Sang Ok, Sang-Seok Bae, Sung-Wook Kwon, and Richard Feiock. 2010. "County Limits: Policy Types and Expenditure Priorities." *The American Review of Public Administration* 40(1): 29-45.

Tam Cho, Wendy K., James G. Gimpel, and Iris S. Hui. 2013. "Voter Migration and the Geographic Sorting of the American Electorate." *Annals of the Association of American Geographers* 103(4): 856-870.

Einstein, Katherine Levine, and Vladimir Kogan. 2016. "Pushing the City Limits: Policy Responsiveness in Municipal Government." *Urban Affairs Review* 52(1): 3-32.

Erikson, Robert S., Gerald C. Wright, and John P. McIver. 1993. *Statehouse Democracy: Public Opinion and Policy in the American States*. Cambridge University Press.

Gerber, Elisabeth R., and Daniel J. Hopkins. 2011. "When Mayors Matter: Estimating the Impact of Mayoral Partisanship on City Policy." *American Journal of Political Science* 55(2): 326-339.

Glaeser, Edward L., and Bryce A. Ward. 2006. "Myths and Realities of American Political Geography." *The Journal of Economic Perspectives* 20(2): 119-144.

Guryan, Jonathan. 2001. "Desegregation and Black Dropout Rates." Working paper, National Bureau of Economic Research. Accessed June 27, 2016 via www.nber.org/papers/w8345.pdf.

Hajnal, Zoltan L., and Jessica Trounstine. 2010. "Who or What Governs? The Effects of Economics, Politics, Institutions, and Needs on Local Spending." *American Politics Research* 38(6): 1130-1163.

Holbein, John B., and D. Sunshine Hillgyus. 2015. "Making Young Voters: The Impact of Preregistration on Youth Turnout." *American Journal of Political Science* 60(2): 364-382.

Hogue, Carma. 2013. *Government Organization Summary Report: 2012.* United States Census Bureau. Accessed June 25, 2016 via https://www2.census.gov/govs/cog/g12_org.pdf.

Huie, Canaan. 2014. "A (Somewhat) Brief History of Local Sales Taxes." Moore & Van Allen. Accessed June 21, 2016 via http://www.mvalaw.com/news-publications-396.html.

Jackson, C. Kirabo, Rucker C. Johnson, and Claudia Persico. 2016. "The Effects of School Spending on Educational and Economic Outcomes: Evidence from School Finance Reforms." *Quarterly Journal of Economics* 131(1): 157-218.

Kogan, Vladimir. 2017. "Administrative Centralization and Bureaucratic Responsiveness: Evidence from the Food Stamp Program." *Journal of Public Administration Research and Theory* forthcoming.

Lax, Jeffrey R., and Justin H. Phillips. 2009. "How Should We Estimate Public Opinion in the States?" *American Journal of Political Science* 53(1): 107-121.

Lenz, Gabriel S. 2012. *Follow the Leader? How Voters Respond to Politicians' Policies and Performance*. University of Chicago Press.

Massey, Douglas S., and Nancy A. Denton. 1988. "The Dimensions of Residential Segregation." *Social Forces* 67(2): 281-315.

Matsusaka, John G. 2010. "Popular Control of Public Policy: A Quantitative Approach." *Quarterly Journal of Political Science* 5(2): 133–67.

McCarty, Nolan, Keith T. Poole, and Howard Rosenthal. 2009. "Does Gerrymandering Cause Polarization?" *American Journal of Political Science* 53(3): 666-680.

Minkoff, Scott L. 2009. "Minding Your Neighborhood: The Spatial Context of Local Redistribution." *Social Science Quarterly* 90(3): 516-537.

Mummolo, Jonathan, and Clayton Nall. 2016. "Why Partisans Don't Sort: The Constraints on Political Segregation." *Journal of Politics* forthcoming.

National League of Cities. 2016. "Revenue from Taxes." Accessed June 21, 2016 via http://www.nlc.org/build-skills-and-networks/resources/cities-101/city-finances/local-revenue-structures/revenue-from-taxes.

Nall, Clayton. 2015. "The Political Consequences of Spatial Policies: How Interstate Highways Facilitated Geographic Polarization." *Journal of Politics* 77(2): 394-406.

Oates, Wallace E. 2006. "The Many Faces of the Tiebout Model." In Fischel, William A., and Wallace E. Oates, eds., *The Tiebout Model at Fifty: Essays in Public Economics in Honor of Wallace Oates*. Cambridge: Lincoln Institute of Land Policy.

Palus, Christine Kelleher. 2010. "Responsiveness in American Local Governments." *State and Local Government Review* 42(2): 133-150.

Park, David K., Andrew Gelman, and Joseph Bafumi. 2004. "Bayesian Multilevel Estimation with Poststratification: State-Level Estimates from National Polls." *Political Analysis* 12(4): 375-385.

Peltzman, Sam. 1980. "The Growth of Government." *Journal of Law & Economics* 23(2): 209-287.

Percival, Garrick L., Martin Johnson, and Max Neiman. 2009. "Representation and Local Policy: Relating County-Level Public Opinion to Policy Outputs." *Political Research Quarterly* 62(1):

164-177.

Peterson, Paul E. 1981. *City Limits*. University of Chicago Press.

Poole, Keith T., and Howard L. Rosenthal. 2007. *Ideology and Congress*. New York: Transaction Publishers.

Poterba, James M. 1997. "Demographic Structure and the Political Economy of Public Education." *Journal of Policy Analysis and Management* 16(1): 48-66.

Sances, Michael W. 2017. ""Ideology and Vote Choice in U.S. Mayoral Elections: Evidence from Facebook Surveys." *Political Behavior* forthcoming.

Sussell, Jesse, and James A. Thomson. 2015. *Are Changing Constituencies Driving Rising Polarization in the U.S. House of Representatives?* Santa Monica, CA: RAND Corporation. Accessed June 24, 2016 via http://www.rand.org/pubs/research_reports/RR896.html.

Tausanovitch, Chris, and Christopher Warshaw. 2013. "Measuring Constituent Policy Preferences in Congress, State Legislatures, and Cities." *Journal of Politics* 75(2): 330-342.

Tausanovitch, Chris, and Christopher Warshaw. 2014. "Representation in Municipal Government." *American Political Science Review* 108(3): 605-641.

Theriault, Sean M. 2008. *Party Polarization in Congress*. Cambridge University Press.

Tiebout, Charles M. 1956. "A Pure Theory of Local Expenditures." *Journal of Political Economy* 64(5): 416-424.

United States Census Bureau. 2008. "County Government Finances Data User Notes." Accessed June 21, 2016 via https://www2.census.gov/pub/outgoing/govs/special60/County_Govt_Fin.zip (file `_Finance_Publication_Data_Guide.xls`, tab 4b).

United States Census Bureau. 2016. "Federal, State, & Local Governments – Definitions." Accessed June 21, 2016 via https://www.census.gov/govs/definitions/.

Winkler, Richelle, Kenneth M. Johnson, Cheng Cheng, Jim Beaudoin, Paul R. Voss, and Katherine J. Curtis. 2013. "Age-Specific Net Migration Estimates for US Counties, 1950-2010." Applied Population Laboratory, University of Wisconsin-Madison. Accessed May 26, 2017 via http://netmigration.wisc.edu/.

Wright, Gerald C., Robert S. Erikson, and John P. McIver. 1987. "Public Opinion and Policy Liberalism in the American States." *American Journal of Political Science* 31(4): 980-1001.

Online Appendix

1 Correlation Between MRP Ideology and Democratic Vote

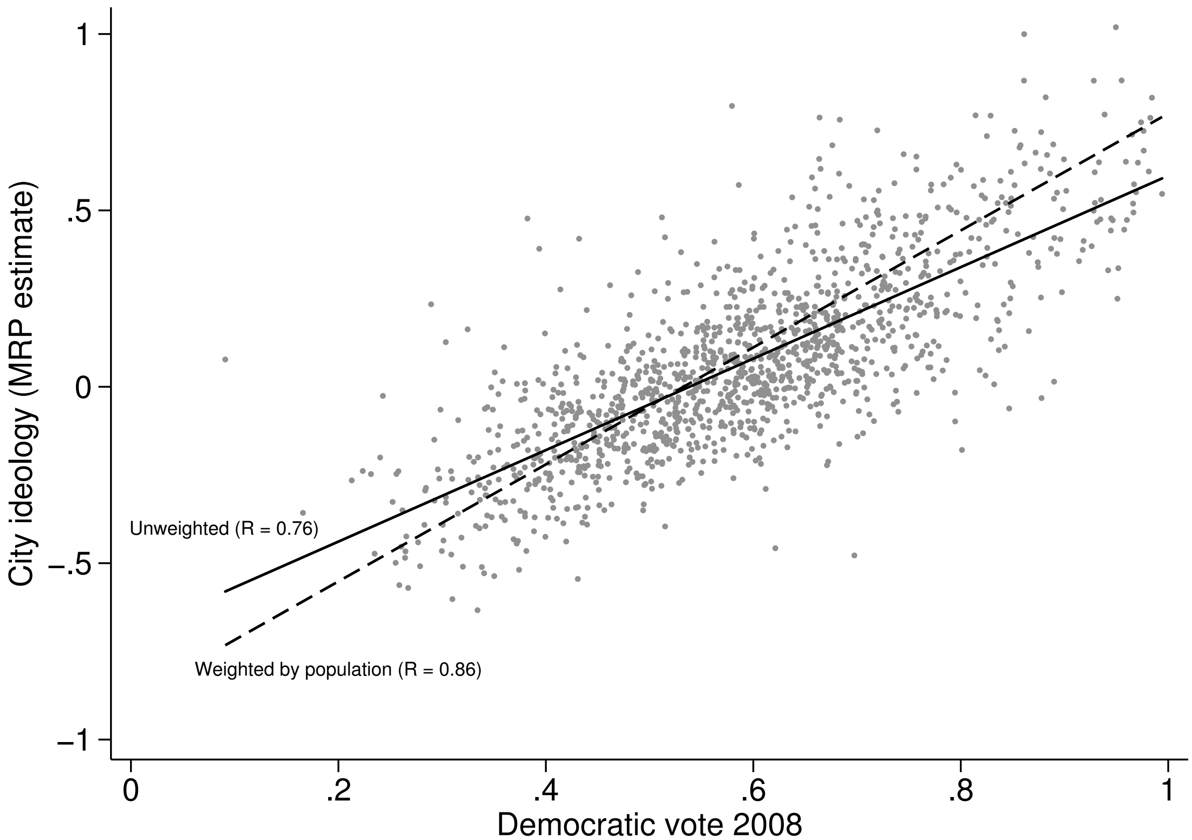
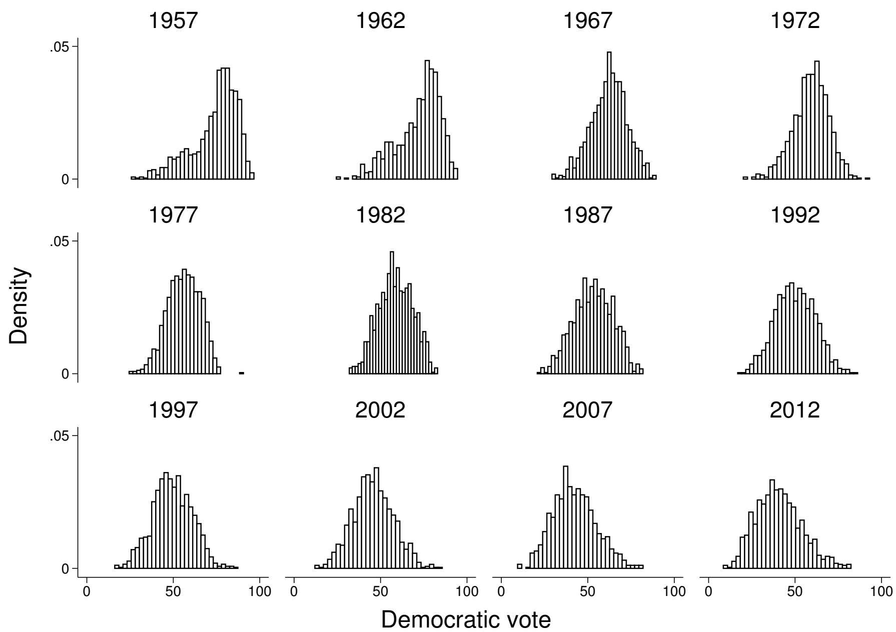


Figure A1: Correlation between MRP estimate of local ideology and Democratic vote in Tausanovitch and Warshaw (2014) cities.

2 Distribution of Democratic Vote

(a) South



(b) Non-South

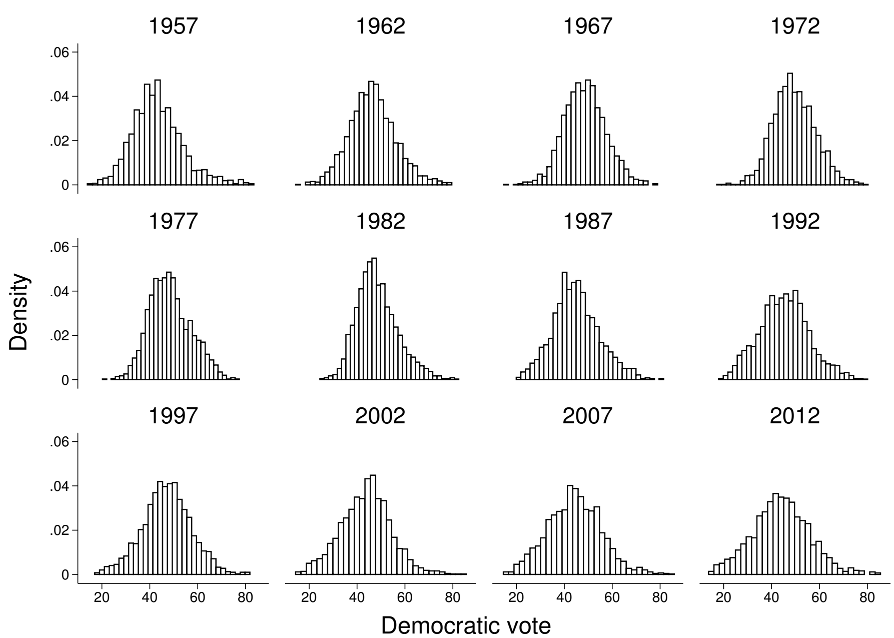


Figure A2: Distribution of Democratic vote, by South.

3 Results by South

In this section I replicate Table 2 in the main text, separately for Southern and non-Southern counties. Table A2 shows the estimates for the non-South are around $20; Table A2 shows the estimates for the South are smaller by about half; all the estimates are statistically significant at conventional levels.

	Spending			Own-source revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Democratic vote	0.43***	0.21***	0.21***	0.37**	0.23***	0.20***
	(0.12)	(0.05)	(0.04)	(0.13)	(0.06)	(0.04)
Median family income		0.38***	0.52***		0.41***	0.57***
		(0.07)	(0.06)		(0.07)	(0.05)
Share white		-0.23*	-0.30***		-0.28*	-0.37***
		(0.09)	(0.07)		(0.12)	(0.08)
Share elderly		-0.04	-0.02		-0.02	0.03
		(0.08)	(0.05)		(0.08)	(0.04)
Number of govs		0.04	0.10*		0.04	0.06
		(0.04)	(0.04)		(0.04)	(0.03)
Ln population density		-0.74***	-0.71***		-0.72***	-0.49***
		(0.16)	(0.16)		(0.20)	(0.15)
State revenue		0.59***	0.58***		0.03	0.11**
		(0.07)	(0.05)		(0.06)	(0.03)
Migration		0.01	0.02		0.00	0.01
		(0.03)	(0.02)		(0.03)	(0.02)
Fixed effects:						
County	Y	Y	Y	Y	Y	Y
Year	Y	Y		Y	Y	
State-year			Y			Y

(N= 1,930, 12 years, 23,160 observations.)

Table A1: Results for Non-Southern counties. Cell entries are estimated coefficients from linear regressions, with standard errors in parentheses. Standard errors clustered at the county level, and observations are weighted by population.

	Spending			Own-source revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Democratic vote	0.13**	0.09**	0.10**	0.13**	0.09*	0.11**
	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Median family income		0.07	0.14		0.13	0.19
		(0.13)	(0.11)		(0.11)	(0.10)
Share white		-0.16	-0.16*		-0.21*	-0.18*
		(0.09)	(0.07)		(0.09)	(0.08)
Share elderly		-0.11	-0.11		-0.06	-0.06
		(0.07)	(0.07)		(0.06)	(0.06)
Number of govs		1.71***	1.43***		1.87***	1.83***
		(0.42)	(0.36)		(0.42)	(0.38)
Ln population density		0.32	0.08		0.31	0.11
		(0.19)	(0.18)		(0.18)	(0.20)
State revenue		0.55***	0.56***		-0.05	-0.04
		(0.05)	(0.06)		(0.04)	(0.06)
Migration		-0.02	-0.02		-0.04**	-0.02
		(0.02)	(0.02)		(0.01)	(0.01)
Fixed effects:						
County	Y	Y	Y	Y	Y	Y
Year	Y	Y		Y	Y	
State-year			Y			Y

(N=1,071 counties, 12 years, 12,852 observations.)

Table A2: Results for Southern counties. Cell entries are estimated coefficients from linear regressions, with standard errors in parentheses. Standard errors clustered at the county level, and observations are weighted by population.

4 Results Excluding Weights

The results in the main text weight by county population. In this section I show estimates from specifications without weights, and controlling for log population, for several population cutoffs. These estimates are from specifications with covariates and with county fixed effects and state-by-year fixed effects.

Figure A3 plots the results for spending and revenue. For all cities, the point estimate without weights for spending is about \$50, and the 95% confidence interval does not cross zero. The estimates become larger and more precise, rising to about \$10, when examining only counties with greater than 10,000, 25,000, and 100,000 persons. The estimates for revenue are similar, save that the 95% confidence interval crosses zero when examining all counties.

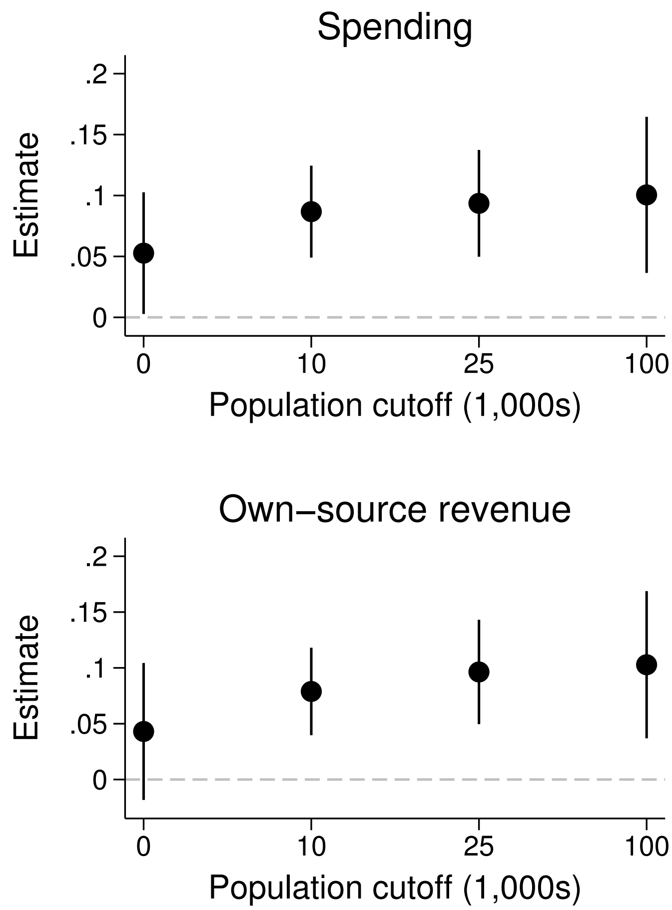


Figure A3: Results excluding weights.

5 Scatter Plot for Own-Source Revenue

Figure 2 in the main text plots spending against Democratic vote share by year. In this section, I replicate the analysis using own-source revenue. The results, shown in Figure A4, are similar.

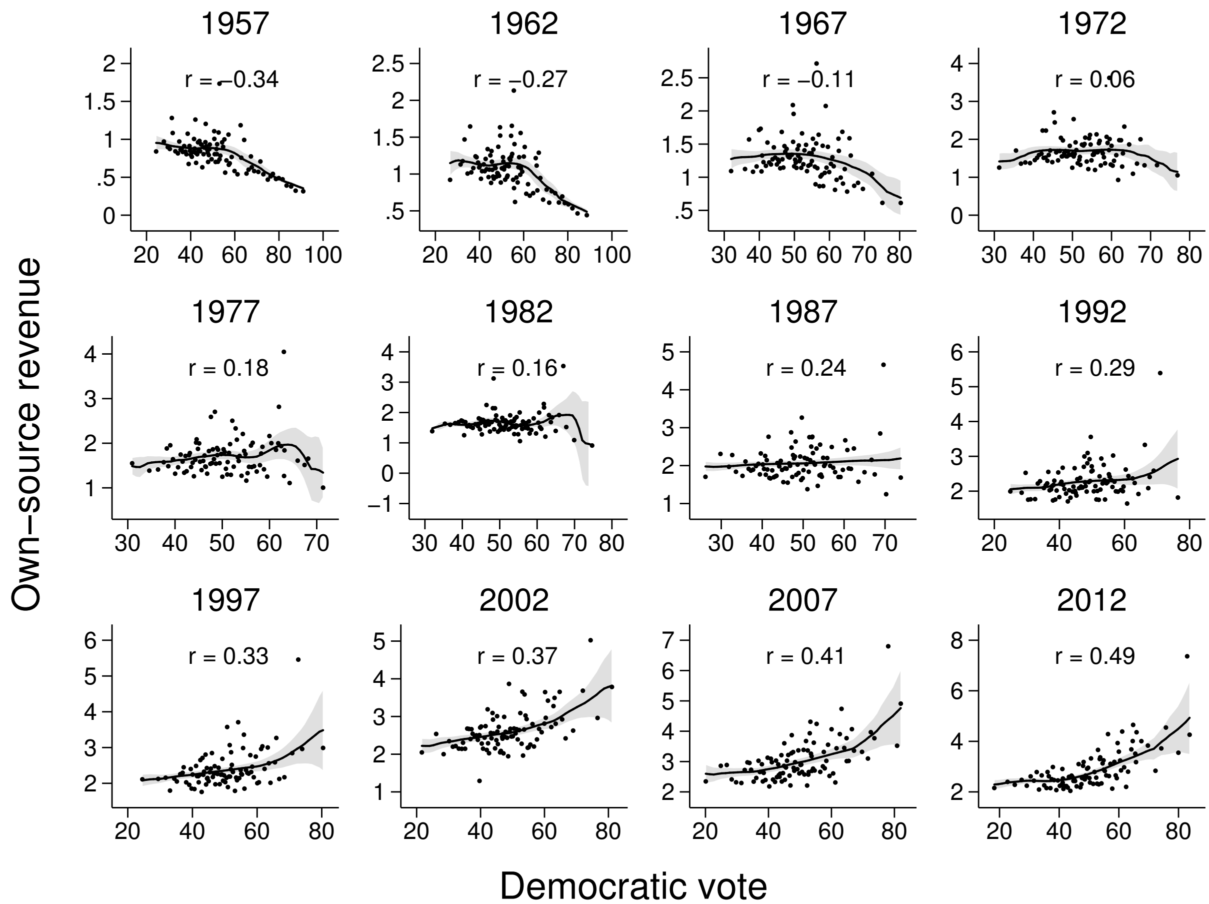


Figure A4: Revenue and Democratic vote, 1957-2012. Points are local averages generated within each percentile of Democratic vote. Solid lines are local polynomial fits, shaded areas representing 95% confidence bands.