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SPENDING WITHIN LIMITS: EVIDENCE FROM MUNICIPAL FISCAL RESTRAINTS

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This paper studies the role of a constitutional rule new to the literature — a limit placed by a city on its own ability to tax or spend. We find that such a limit exists in at least one in eight cities. After limit adoption, municipal revenue growth declines by 13 to 17 percent. Our results suggest that institutional constraints may be effective when representative government falls short of the median voter ideal.

Keywords: tax and expenditure limits, municipal government, representative government, constitutional rules

JEL Codes: R5, H2, H4, H7, P16

I. INTRODUCTION

Political economists have long debated the extent to which majority rule limits the behavior of elected officials. Black (1958) and Downs (1957) suggest that policy outcomes are limited to the median voter's preference. In contrast, Buchanan and Tullock (1962), Niskanen (1971), and Romer and Rosenthal (1978, 1979) argue that the ballot box provides little, if any, constraint. They contend either that government is fundamentally a Leviathan (a government that maximizes revenue) or that agency problems in political representation are severe; thus, societies must turn to constitutional rules to constrain government.[1]

In this paper, we ask when representation falls short in limiting government and whether constitutional rules that act to constrain representative government are effective.

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[1] This concept of the Leviathan comes from Hobbes (2003), and was extended by Brennan and Buchanan (1980).

Specifically, we study rules at the municipal level that constrain a city's ability to tax or spend. To do this, we identify a hitherto unclassified type of constitutional limit: a fiscal limit placed by a city on its own ability to tax or spend. We call this a locally- or municipally-imposed tax or expenditure limit. We find that these constitutional limits are effective in curbing the growth of municipal spending.

There are several reasons why a self-imposed local limit is unlikely. Under Dillon's Rule, the legal precedent for state preeminence over local government affairs, state legislatures already have near complete control over local governments and therefore provide a de facto constitutional constraint on local government power.[2] In addition, many cities already face stringent state-imposed limits — many adopted after the 1970s — that further restrict municipal revenue gathering. Further, the Tiebout (1956) hypothesis argues that voter mobility constrains the ability of governments to diverge significantly from voter preferences. Specifically, dissatisfied municipal residents and businesses are able to restrict Leviathan governments by credibly threatening to move to nearby jurisdictions with preferred tax and expenditure packages.

A large literature has analyzed fiscal limits that states place on cities, such as California's Proposition 13. However, limits that cities place on themselves, to the best of our knowledge, have not been analyzed in the academic literature. While state-imposed limits on cities can be rationalized as the desire of voters in some cities to control the fiscal behavior of those in other cities (as in Vigdor (2004)), municipally-imposed limits cannot be explained in this way.[3] Thus, the existence of municipal self-imposed local limits poses a direct challenge to the idea that the size of government is sufficiently constrained by electoral institutions.

To examine whether self-imposed limits exist at the municipal level, we conducted a survey of 347 municipalities — all 247 cities with populations of 100,000 or more — and a random sample of 100 cities with populations between 25,000 and 100,000. We find that at least one in eight municipalities have some form of limit; in some cases, voters turn to constitutional constraints, while in other cases either elections or an institutional substitute suffice.

We combine these survey data with numerous other data sources to examine patterns of limit adoption. We find that cities with higher median incomes are less likely to adopt a limit. In addition, local limits are less likely where certain institutional substitutes exist. The first such institutional substitute is general law status. General law cities have tighter state restrictions on behavior than cities operating under home rule, the other possible legal status for cities. The second institutional substitute, which is more likely to exist when a limit is absent, is the presence of a relatively large number of jurisdictions in the metropolitan area. Following the Tiebout hypothesis, more alternative jurisdictions may provide a check on politicians' behavior. Conversely, fewer alternative jurisdictions may require other mechanisms to limit a politician's range of actions. Surprisingly, we

[2] City of Clinton vs. Cedar Rapids and Missouri River Railroad Company, 24 Iowa 455 (1868).

[3] However, one might conceive of a self-imposed limit as a useful tool to attract residents from other cities. We find no support for this claim in the data.

find few statistically significant relationships between limits and current or previous high levels of taxation or variation in taxation.

We next use panel data to show that, on average, after the passage of a limit, the average rate of revenue growth declines by 13 to 17 percent relative to either the pre-limit period or to never-limited cities. This finding is in contrast to the literature on self-imposed limits at the state level, which generally finds that such limits do not affect revenues (Rose and Smith, 2014). Since limit adoption is likely to be endogenous, this result may be driven by other underlying trends in the jurisdiction correlated with limit passage. To address this concern, we use matching techniques to suggest that observed revenue declines may be causally affected by limits. We also use graphical and statistical methods to show that our findings are unlikely to be driven by changes in tastes that precede limit adoption.

Our paper is related to two literatures. The first is the political economy debate on whether electoral institutions can restrict political behavior. The pioneering work of Black (1958) and Downs (1957) has spawned many empirical tests; early contributors include McEachern (1978), Holcombe (1980), Inman (1978), and Munley (1984). In contrast, other researchers argue that government is naturally expansionary and cannot be checked by the ballot box; constitutional rules are thus necessary to limit government growth. Brennan and Buchanan (1979, 1980) discuss some of the provisions enacted during the tax revolt of the 1970s as examples of such constitutional rules.[4] Tabellini and Alesina (1990) discuss the conditions under which constitutional balanced budget limits are likely to be adopted, Azzimonti, Battaglini, and Coate (2008) provide a theoretical framework for assessing the impacts of constitutional limits, and Besley and Smart (2007) demonstrated the importance of term limits when political agency problems exist.[5]

This paper also contributes to a literature focused directly on tax and expenditure limits. One strand of this literature considers limits imposed by states on cities. This focus on local government responses is parallel to our work. Mullins and Wallin (2004) and the Advisory Commission on Intergovernmental Relations (1995) document the presence and extent of these limits, while other researchers study why these limits are imposed (Anderson and Pape, 2010; Alm and Skidmore, 1999; Cutler, Elmendorf, and Zeckhauser, 1999; Ladd and Wilson, 1982, 1983; Stein, Hamm, and Freeman, 1983; Temple, 1996; Vigdor, 2004). Further work examines their effect on expenditures and fiscal structure (Joyce and Mullins, 1991; Figlio and O'Sullivan, 2001; Mullins, 2004; Mullins and Joyce, 1996) and their effect on the distribution of taxation (Chernick and Reschovsky, 1982). Other researchers examine the effect of these limits on service quality (Figlio and Rueben, 2001; Downes, Dye, and McGuire, 1998; Dye and McGuire, 1997; Downes and Figlio, 1999).

[4] In the 1970s, many states passed limits on cities and themselves. Most famous among these limits is California's 1978 Proposition 13.

[5] Persson and Tabellini (1996a, 1996b) discuss the role of a fiscal constitution in funding a federation.

Another strand of the tax and expenditure literature focuses on limits imposed by states on themselves. While the jurisdiction of interest here differs from our focus on municipalities, the focus on the political economy of self-regulation parallels our analysis. Besley and Case (2003) provide an overview of these limits. Most articles in this literature find little effect of state-imposed limits on state behavior (Kousser, McCubbins, and Moule, 2008; Kenyon and Benker, 1984; Bails, 1990; Cox and Lowery, 1990; Joyce and Mullins, 1991; Shadbegian, 1996; Bails, 1990). In contrast, and using different methods, Rueben (1996), Bails and Tieslau (2000), and Knight (2000) all find evidence that self-imposed limits — supermajority requirements for tax increases in Knight's case — do cause small but significant declines in spending. These findings are consistent with our results.

We begin by describing our survey and our findings on the existence of self-imposed limits and the patterns of limit adoption. Section II empirically analyzes the fiscal consequences of limit adoption. In Section III, we consider what types of theoretical models would be consistent with our findings. Section IV concludes.

II. THE EXISTENCE OF LOCAL LIMITS

A. Survey and Supplemental Data

To explore the existence of self-imposed municipal tax and expenditure limits, we undertook a survey of large and mid-sized American cities. Our survey sample consists of all 247 cities with populations of 100,000 or more and a random sample of 100 cities with populations between 25,000 and 100,000.[6] Our sample cities account for 26 percent of the total U.S. population. While the principal purpose of the survey is to identify cities that adopt a local limit, we also use it to collect data on the features of the limits, such as their date of adoption and override provisions, and the perceptions of local officials about the effects of limits.[7]

[6] We use the Census of Governments 2002 Governments Integrated Directory as our sample frame. We keep only cities with the following political descriptions: Charter Township, City, City and Borough, City and County, City-Parish, Consolidated Government, Municipality, Town, and Village. (City and County refers only to cities with coterminous county boundaries, such as San Francisco or Philadelphia, which operate jointly). We refer to all of these entities throughout as either cities or municipalities. Summary statistics comparing our randomly sampled cities (those with populations between 25,000 and 100,000) to all non-sampled cities show few differences; see Appendix Table 1 for the formal test (all appendix tables are in the online appendix at http://home.gwu.edu/~lfbrooks/leahweb/subpages/research.html).

[7] We use the universe of larger cities and a sample of smaller cities for three key reasons. First, by sampling larger cities with certainty, we have a sample that represents a substantially larger proportion of the U.S. population, making our findings more policy relevant. Second, when choosing where in the distribution to sample, we prefer to sample the "thicker" part of the population distribution (smaller cities) rather than the "thinner" part (larger cities). Relative to the thin part of the distribution, the thick part of the distribution has more similar cities, and any sampled city is more likely to be representative of non-sampled cities. Third, the Annual Survey of Government Finances data uses a population weighted sampling scheme so that larger cities are more likely to have finance data.

We conducted the survey in 2007 primarily by telephone.[8] For each city in our sample, we collected contact information from municipal websites for the City Manager, Budget Director, and Finance Director and attempted to contact each of the 736 officials for whom we had information.[9] In total, we spoke with 412 officials and received responses from 320 unique cities, generating a 92 percent response rate. While none of the questions asked were sensitive, we assured all participants that their identities would remain anonymous.[10]

We define a self-imposed local tax and expenditure limit as a law (appearing in the municipal code or charter) that explicitly caps total municipal revenues or outlays, that caps the overall rate or total revenue generated from a given tax or fee, or that requires a referendum to raise an existing tax or fee.[11] Importantly, we require that this limit be adopted by the city itself and not by the state government.[12]

Finally, when a city reported having a locally-imposed limit, we verified its existence by looking in the municipal code or charter for the limit. If we could not find it, we re-contacted the city to verify the survey response.[13] This led to the exclusion of a handful of false positives, including some state-imposed limits that respondents mistook for locally-imposed limits. We did not conduct a similar exclusion for false negatives — cities that do have a limit but that mistakenly reported that they do not.

Because we do not exclude false negatives, and because our survey is not able to identify local limits that were adopted and then repealed before our survey, we therefore

[8] Before conducting the full survey, we made a preliminary effort to determine whether locally-imposed limits exist. During the summer of 2006, we selected a sample of 60 cities and searched their municipal charters or codes for evidence of limits. We also called local officials from each sampled city to ask whether their city had adopted such limits. The results led us to conclude that a larger survey was warranted. It also revealed that reading municipal documents is a very poor mechanism for identifying limits absent interviews.

[9] Not all municipalities have all three of these offices; we collected contact information for all available types.

[10] The survey instrument is in the online appendix, available at http://home.gwu.edu/~lfbrooks/leahweb/subpages/research.html. Though the survey is presented as a form that respondents could return, the vast majority of responses were by telephone.

[11] We are confident in the reliability of our survey results for several reasons. First, respondents were well qualified to answer the questions we posed. We surveyed only individuals in formal positions in municipal budgeting. Because survey responses were primarily by phone, we know that the questions were answered by the targeted individuals or by someone similarly qualified. Second, our survey results do not appear to suffer from non-response bias. Appendix Table 2 contrasts respondent and non-respondent cities across several key fiscal, demographic, and institutional characteristics, showing that there are very few statistically significant differences (only income varies across response status). This absence of selection into respondent status suggests that our results are representative of the sampled population.

[12] State governments impose two types of limits on cities: those inherent in the municipal incorporation process and those adopted later and commonly known as state tax and expenditure limits.

[13] We also did this in the small set of cases when respondents in the same city did not agree.

interpret our results as a firm lower bound on the presence of local limits.[14] We believe the true extent of local limits to be larger than our estimate indicates.[15]

B. Existence and Types of Limits

Of our 320 respondent cities, 40 or 12.5 percent, have at least one self-imposed limit. In total, the 40 limited municipalities have 56 individual limits. Because these limits are new to the literature, we begin by describing them and then discuss basic covariates of limit adoption.

As shown by the top panel of Table 1, self-imposed limits overwhelmingly target the property tax — historically, the largest source of revenue for local governments (Sokolow, 1998). Property tax rate limits and levy limits, at 39 and 16 percent respectively, make up the majority of the limits we observe. To help clarify these limits, Table 2 gives an example of each type of limit. A rate limit sets a ceiling on the city's property tax rate. For example, the city of Eastpointe, Michigan has a rate limit that caps its property tax rate at 1.5 percent. A levy limit constrains the total amount of money that can be generated from the property tax, independent of the overall rate. Lincoln, Nebraska limits the total property tax levy (i.e., total property tax revenues raised) to no more than a 7 percent annual increase from a 1966 baseline. The third type of property tax restriction we observe is an assessment limit (7 percent of all local limits). Assessment limits are intended to restrict a city's ability to turn rising property values into a taxable base. These limits are usually expressed as an allowable annual percentage increase in assessed value. Baltimore, Maryland limits the annual growth in property assessments to no more than 4 percent.

The most comprehensive and restrictive type of local limit, which exists in two cities (comprising three separate legal limits), is a general revenue or expenditure limit. Such a limit caps either the total amount of own-source revenue or total own-source expenditures and is typically expressed as an annual allowable percentage increase. Anchorage, Alaska limits tax revenue growth to inflation and population growth.

The remaining tax and expenditure limits either apply to the sales tax or are categorized as "other." Sales tax limits, consisting of nearly 11 percent of local limits, typically cap the overall rate that can be charged or restrict the items that can be taxed. Tucson, Arizona limits the municipal sales tax to 2 percent. Limits that fall into the "other" category, just over 21 percent, target a wide range of municipal revenue sources, including entertainment, business, and income taxes, as well as certain user fees. For example, Columbus, Ohio caps the municipal income tax rate at 1 percent.

[14] Cities do not generally have documents that describe how the charter or municipal code changes over time. Even if such documents were available, our pre-survey work (see footnote 7) strongly suggests that it is very difficult to differentiate between city-adopted and state-mandated limits by reading documents alone.

[15] The citation and wording of the locally-adopted limits identified by our survey and located in municipal charters or code are available upon request.

Table 1
Description of Local Limits

	(1) Number	(2) Share
Number of Cities with Limits	40	12.5
By Limit		
Type of limit		
Assessment limit	4	7.1
Property tax rate limit	22	39.3
Other	12	21.4
Property tax levy limit	9	16.1
Revenues or expenditure limit	3	5.4
Sales tax limit	6	10.7
Total	56	100
Where is the limit adopted?		
In municipal charter	38	67.9
In municipal code	13	23.2
No valid response	5	8.9
Is an override possible?		
No valid response	5	8.9
No	2	3.6
Yes: majority vote of the city council	6	10.7
Yes: majority vote of the electorate	36	64.3
Yes: super majority of the city council	3	5.4
Yes: super majority of the electorate	4	7.1
By City		
Has your city reached the legislated limit?		
No valid response	3	7.4
No, but close	3	7.4
No, not close	16	39.3
Yes	19	45.9

Source: Authors' survey

Our survey finds that most self-imposed local limits are constitutional, in the Buchanan and Tullock (1962) sense of setting the constitutional rules that govern the game. As shown in the middle of Table 1, over two-thirds of limits are written into municipal charters, making their repeal more difficult and politically costly than a similar limit in the municipal code. A charter is the municipal correlate of a constitution; the code is parallel to statutory regulation. Although most limits have an override provision, overrides typically require a majority or supermajority vote of the electorate. Only 15

Table 2

Local Limit Examples

City	Limit Type	Description	Override
Eastpointe, MI	Property tax rate	Property tax rate is capped at 1.5 percent.	Majority of voters
Lincoln, NE	Property tax levy	The total property tax levy may not increase annually by more than 7 percent from the 1966 baseline.	Majority of voters
Baltimore, MD	Assessment	Assessments on property cannot increase by more than 4 percent.	Majority of city council
Anchorage, AK	Revenue or expenditure	Total tax revenue cannot increase by more than the rate of inflation plus population growth.	Majority of voters
Tucson, AZ	Sales tax	The city cannot levy a sales tax that exceeds 2 percent.	Majority of voters
Columbus, OH	Other	The city income tax is capped at 1 percent.	Majority of voters

Notes: This table presents an example of each type of local limit about which our survey asked. While the state of Ohio does have a state-level limit on municipalities' income tax rate, the Columbus limit preceded this state-level limit.

Source: Authors' survey

percent of limits can be circumvented through city council action alone, and nearly one-third of these require a council supermajority for override.

Local limits exist in all census regions, though cities in the northeast are least likely to adopt a limit. While cities in the northeast constitute 18 percent of respondent municipalities, they account for only 7 percent of the limits identified by our survey. Midwestern and southern cities, however, constitute 15 and 28 percent of respondent cities, respectively, but account for 35 and 33 percent of all limits. Western cities are 38 percent of respondents and account for 25 percent of all local limits.

Through the survey and subsequent research efforts, we obtained the date of adoption for over half of the limits identified by our survey and, in some instances, the method of adoption as well.[16] Local limits generally come into existence through one of two mechanisms: city council action or a ballot measure proposed by a citizen or interest group. The earliest limit among our sampled cities was enacted in 1925 — a property tax rate limit in the city of Amarillo, Texas — and the median year of limit adoption is 1979. As Figure 1 demonstrates, there is no distinct period of local limit adoption. Unlike many of the stringent state-imposed limits, local limits do not appear to be closely connected to the tax revolt of the late 1970s and early 1980s. They are thus unlikely to be caused by the technical changes to assessment identified in Anderson and Pape (2010). Importantly, many of the local limits we identify are more restrictive than the limit imposed by the state government (if one exists), apply to different revenue sources than the state limit, or pre-date many of the most rigorous state restrictions.

C. Correlates of Limit Adoption

We now provide some stylized facts about the correlates of local limits. We first consider basic descriptive statistics on limit adoption and then move to regressions that investigate the causes of limit adoption.

To this end, we combine the results of our cross-sectional survey data and information on the year of limit adoption with a wealth of data on municipalities.[17] To describe cities' fiscal condition, we use the Census Bureau's Annual Survey of Government Finances, 1970–2006. This survey is a census in years ending in two and seven. In all other years, the Census of Governments collects fiscal information from all larger cities and from a population-weighted sample of smaller cities.

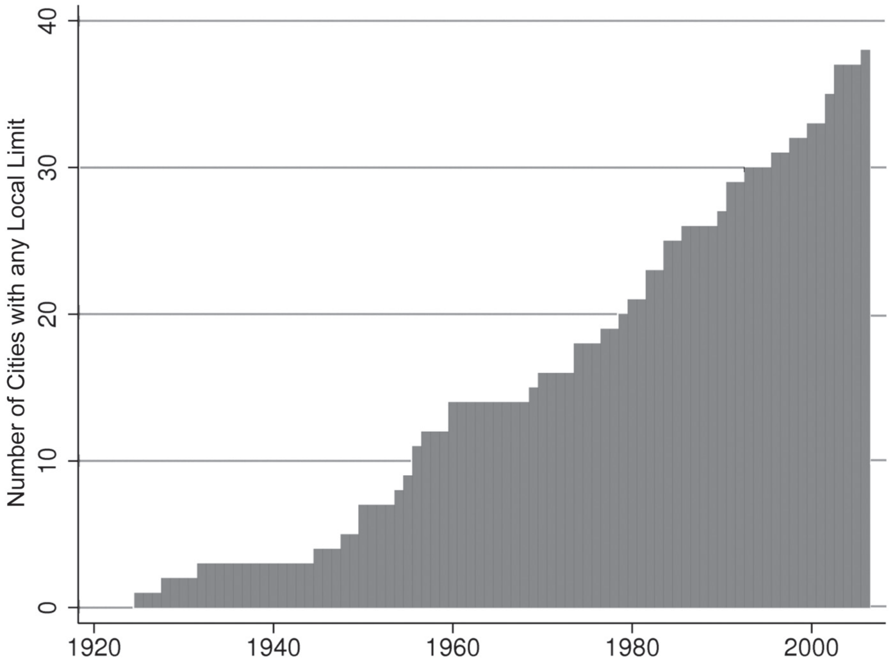
To describe the demographic features of cities, including the metropolitan area in which each city is located, we use decennial census data from the censuses of 1970, 1980, 1990, and 2000 (via Summary Tape File 3).[18] We linearly interpolate all decennial census data between census years. Data on city political structure comes from the 1987 Census

[16] We did not collect information on the method of limit adoption, such as citizen initiative or council vote, in our survey.

[17] For complete details on data sources, see the online Appendix.

[18] Census municipal codes change from 1980 to 1990, and we construct a cross-walk to merge across years. Full information on all data sources is in the Appendix.

Figure 1
Limit Adoption Over Time



Notes: This chart presents the total number of surveyed cities reporting any local limit by year. We do not observe a year of adoption for all limited cities, so the total in this picture does not equal the total number of cities with limits in Table 1.
Source: Authors' survey

of Government Organization and from the Legal Landscape Database, which describes direct democracy provisions in the thousand largest American cities.[19] Information on state-mandated tax and expenditure limits come from Mullins and Wallin (2004) and the Advisory Commission on Intergovernmental Relations (1995). We use the urban consumer price index to convert all of our fiscal and economic data into 2006 dollars.

Cities remain in the estimation sample only when we observe finance data (which we never interpolate). This merging process yields an unbalanced panel of 10,771 city-year observations. All but one respondent city is present for at least one year in the panel

[19] This means we observe most institutions at only one point in time. However, institutional features change quite slowly; see Baqir (2002) on the size of the city council.

data.[20] In all 37 years of the panel, 77 percent of cities are present, and for at least 25 years, 89 percent are present.

We begin with basic descriptive statistics using the 2002 cross-section, the most complete year of data in our sample, presented in Table 3. Panel A shows demographic covariates. The lone demographic characteristic that differs statistically between limited and unlimited cities is median household income. In 2002, the median family income in limited cities (in 2006 dollars) was $55,000, whereas in unlimited cities it was $62,000. In contrast, limited cities are more populous, but not statistically significantly more populous, than unlimited cities. In addition, racial shares do not differ by limit status, suggesting that the heterogeneous demand for public goods that motivates limits does not follow strict racial lines.

We examine whether locally-imposed limits are more likely to appear in the presence of a variety of municipal institutions in Panel B of Table 3. We consider five types of institutions that may plausibly limit politicians' revenue decisions: home rule, initiative power, form of government (mayor-council or other), the number of cities in the metropolitan area, and the presence of a binding state limit.[21]

Cities in the United States have two types of authorizing legal status: general law or home rule. General law cities have only the powers given to them by the state. Any powers not expressly given are the province of the state (Krane, Rigos, and Hill, 2000). Most U.S. cities are general law cities. In contrast, home rule cities have the power to act more independently and design their own institutional structures. In some states, the power of home rule extends to taxation. For example, California home rule cities have broader assessment powers than general law cities and are able to collect a tax when property is sold, which general law cities cannot (League of California Cities, 2011). In some states, home rule is a municipal choice; in others, it is required after a city crosses a given population threshold. Panel B of Table 3 shows that cities with a limit are 25 percentage points more likely to be home rule.

Panel B of Table 3 also shows that, in jurisdictions where voters have access to the citizen initiative, which is the ability to initiate legislation via referendum, fiscal limits are more likely. The type of municipal government, either mayor-council or other, is unrelated to limit status.

The jurisdictional structure of the metropolitan area may also constrain politicians' behavior. As described in the Tiebout (1956) model, the presence of neighboring cities may provide alternative tax and expenditure options for voters. Thus, unsatisfied voters might consider migrating to an alternative city that better meets their needs. It is easier

[20] The unmatched city is Centennial, CO, which was created through a merger of many census-designated places in 2001.

[21] Potentially binding limits include revenue or expenditure limits, property tax levy limits, or the combination of a property tax rate limit and a limit on assessment increases. For more discussion of what constitutes potentially binding, state-imposed limits, as well as their effects on local budgeting, see Brooks and Phillips (2010) and Mullins and Wallin (2004). For a complete correlation table between the types of locally-imposed and state-imposed limits, please see Appendix Table 3.

Table 3

Comparison of Cities with and without Limits

	Local Limit Status		
	Yes	No	t-test: yes=no
	(1)	(2)	(3)
Panel A. Demographic Variables			
Population	349,289	227,844	1.34
	(535,612)	(561,357)	
Median family income ($1,000s)	55.3	62.4	3.11
	(12.1)	(20.9)	
Share African-American	0.15	0.15	0.01
	(0.17)	(0.17)	
Share Latino	0.20	0.18	0.43
	(0.19)	(0.19)	
Panel B. Political and Institutional Variables			
Home rule (1 if yes; 0 otherwise)	0.79	0.55	3.42
	(0.41)	(0.50)	
Citizen initiative	0.94	0.84	2.22
(1 if city has; 0 otherwise)	(0.23)	(0.36)	
Mayor-council form of government	0.48	0.34	1.57
(1 if yes; 0 otherwise)	(0.51)	(0.48)	
Number of cities in the MSA	19.3	41.0	3.98
	(28.61)	(49.05)	
1 if state has a binding limit	0.80	0.70	1.44
	(0.41)	(0.46)	
Panel C. Fiscal Variables ($1,000s, per capita)			
Total revenue	2.03	2.07	0.15
	(1.96)	(1.44)	
Total own-source revenue	1.47	1.60	0.62
	(1.20)	(1.11)	
Total property tax revenue	0.37	0.42	0.70
	(0.40)	(0.46)	

Notes: We report means of the named variables; standard deviations are below the means in parentheses. These data are from the 2002 cross-section. We do not observe all variables for all respondent cities. In Panel A, all "yes" and "no" figures are calculated from 40 and 269 observations, respectively. The same figures of Panel B are 40 and 274. For Panel C, we observe between 36 and 40 "yes" observations for each calculation and between 238 and 274 "no" observations.

Sources: Decennial census data; Census of Governments political and fiscal data to find a city that is a match for a voter's optimal public good and tax package when there are more cities in a metropolitan area. Consistent with this line of reasoning, limit adopters tend to be in metropolitan areas with fewer cities — 19 on average — compared to non-adopters, which are in metropolitan areas with an average of 41 cities.[22]

Interestingly, there appears to be little direct relationship between the existence of state-imposed municipal limits and the adoption of local limits. While 80 percent of limit-adopting cities also face a potentially binding, state-mandated limit, 70 percent of non-adopting cities do as well. We also find no evidence that local limit adoption is related to the strength of the state-mandated limit (i.e., whether the state limit is constitutional or merely statutory).

Panel C of Table 3 examines the relationship between limit adoption and local tax revenue. We do not believe that this relationship is well-described using cross-sectional data, and we present summary statistics here for descriptive purposes only. We find no statistically significant difference in total revenue, total own-source revenue, or total property tax revenue between limited and unlimited cities. The panel analysis in Section II is better suited to examining whether the fiscal time path differs between limited and unlimited cities.

To explore whether the characteristics present in the summary statistics are associated with limit adoption in a multivariate context, we turn to regression analysis. To examine the role of city features that change very slowly, such as political institutions, we use cross-sectional data and a probit model. Such features would be either entirely or substantially collinear in a city fixed-effects model. For city features that exhibit greater variation over time, such as revenues, we rely on a hazard model, where the dependent variable is time to limit adoption.

We begin with the cross-sectional analysis, for which we estimate the probit model in (1). The dependent variable, $local\ limit_c$, is equal to one if the city ever has a local limit and is zero otherwise. The covariates of interest, $\boldsymbol{V}_c$, are median income and the two institutional variables — home rule status and the number of jurisdictions in the city's metropolitan area — that differ significantly between limited and unlimited cities in the summary statistics. We assess whether these variables are still associated with limit adoption, controlling for region and other demographic and institutional covariates, using

$$(1) \qquad local\ limit_c = \Phi(\beta_0 + \beta_1 \boldsymbol{V}_c + \beta_2 region_c + \beta_3 institutions_c + \beta_4 demographics_c + \varepsilon_c).$$

We present estimated coefficients from (1), evaluated at the independent variable means, in Table 4.[23] The first four columns present results using only regional dummies as covariates. The first column presents the coefficient on median family income in

[22] Data on the number of municipalities in a metropolitan area come from decennial censuses. For each year in the sample, we count the number of cities with more than 25,000 people per metropolitan statistical area (MSA). We find the maximum and the minimum for each MSA over all years. We report the maximum here; no results are affected by using the minimum.

[23] Ordinary least squares (OLS) estimates show no appreciable qualitative differences.

Table 4

Cross-sectional Evidence on Limit Adoption

	(1)	(2)	(3)	(4)	(5)	(6)
Median income ($1,000s)	–0.003**			–0.002*	–0.002*	–0.001
	(0.001)			(0.001)	(0.001)	(0.002)
1 if home rule, 0 otherwise		0.121***		0.115***	0.102***	0.0370*
		(0.039)		(0.037)	(0.036)	(0.019)
Number of cities in MSA / 100			–0.101	–0.066	–0.047	–0.058*
			(0.065)	(0.061)	(0.056)	(0.033)
Regional dummies	x	x	x	x	x	x
Other institutional covariates					x	x
Demographic covariates						x

Notes: The dependent variable in this regression is a dummy variable equal to one if a city has a local limit, and zero otherwise. These probit regressions are performed on the 2002 cross-section, and we report coefficients evaluated at the variable means (Stata's dprobit). Estimates are for the largest sample that contains information on all variables. Regional dummies are Midwest, South, and West; Northeast is the omitted category. Demographic covariates are population, log of population, share of people age 25 and older with four years of college, share of people age 25 and older with at least a high school education, number of housing units, real median family income, share African-American, share Hispanic, share employed in government, share of housing units built since last census, housing units built before 1940, share manufacturing employment, share service employment by industry, unemployment rate, share of people less than age 18, share of people greater than age 64 years, share of population of foreign origin, vacancy rate, poverty rate, housing units per person, and a Herfindahl index for income. Institutional variables are dummy for council-manager cities, total number of elected officials, dummy if the mayor is directly elected, and share of representatives elected at large. The number of observations is 254. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

Source: Please see data appendix

thousands of dollars. As in the summary statistics, this measure is significantly associated with limit adoption. A one standard deviation increase in median income ($20,000) is associated with a 6 percent lower likelihood of limit adoption. In the second column, the coefficient suggests that a switch from general law status to home rule status is associated with a 12 percent greater likelihood of adopting a local limit. In the third column, cities in metropolitan areas with more cities are insignificantly less likely to adopt a local limit.

Column 4 provides results with all three variables together with the regional dummies. Median income and home rule both remain statistically significant. The results in Column 5 add controls for institutional covariates (see the table notes for the complete list of covariates), and the pattern persists. The model described in Column 6 adds demographic covariates, including population and log of population. Home rule remains a significant predictor of local limit status, and the number of cities in the MSA becomes significantly associated with limit adoption. A 10 percent increase in the number of cities in an MSA (roughly four additional cities) yields a roughly 20 percent decrease in the likelihood of adopting a limit. In this specification, the coefficient on median income is now statistically indistinguishable from zero, probably because this variable is substantially correlated with other demographic covariates. The institutional findings are consistent with the hypothesis that a local limit acts as a substitute for other institutions and with the moderating role of Tiebout competition on partisanship found in Ferreira and Gyourko (2009).[24]

While the probit model is appropriate for examining the correlation between slow-changing municipal features and limit adoption, it is not well-suited to examining how time-varying features may determine local limit adoption. For example, local limits could be adopted in response to high levels of taxation or to variability in taxation. To assess whether limit adoption is related to fiscal behavior before the limit, we estimate hazard models where the dependent variable is the time to adoption of a limit.[25] We use both Cox proportional hazard and exponential hazard models. Specifically, we estimate

$$(2) \quad h(t) = h_0(t)\exp(\beta_1 \textit{real own-source revenue pc}_{c,t} + \beta_2 \boldsymbol{I}_c + \beta_3 \boldsymbol{V}_{c,t}),$$

where $h_0(t)$ is the baseline hazard function. We use this equation to estimate whether the likelihood a city adopts a local limit is responsive to the city's own-source revenues

[24] Another obvious covariate, suggested by the literature on heterogeneity and public goods, is a Herfindahl index for racial or income heterogeneity. We find no association between racial heterogeneity and limit adoption and a very weak association between income heterogeneity and limit adoption. We also find no evidence of a positive association between limit adoption and the local government share of employment. For many additional covariates, including other measures of income distribution, see Appendix Table 4.

[25] We observe date of limit adoption within our sample range for 21 of the 40 limit-adopting cities. This raises the concern that our estimates may be biased by use of a selected sample of limit adopters. To evaluate this, we compare the 21 in-sample and 19 out-of-sample cities using means of the regression covariates. We find statistically significant differences for only 2 of 26 covariates. While this is above the 1.25 covariates we would expect to have a significant difference due to random chance (at the 5 percent level), it signals that differences between in-sample and out-of-sample adopters are not large.

at time t (own-source revenues are those raised from local taxation and not received from other governments, such as the state). We control for variables $\boldsymbol{I}_c$, which are time-invariant city characteristics and include home rule status, the number of cities in the metropolitan area, and others (see table notes for full list). Additional controls $\boldsymbol{V}_{c,t}$ are time-varying and include population, log of population, and median family income (see table notes for the complete list). When we estimate the exponential hazard model, we include year dummies.[26]

We present the results of these specifications in Table 5. The table highlights the key variable of interest, real own-source revenues per capita, and the three variables of interest from the cross-sectional analysis: home rule status, the number of cities in the metropolitan area, and the real median family income. Regardless of specification and covariates, own-source revenue is not statistically significantly related to limit adoption. Interpreting the coefficient in Column 1 — and all remaining coefficients are roughly of the same magnitude — a 10 percent increase in real own-source revenue statistically insignificantly increases the likelihood of limit adoption by 3 percent. Thus, we find no clear evidence that limits are driven by high levels of taxation.[27]

One might believe, however, that limits are driven not by high total levels of taxation but by particular types of taxation or by variance in taxation (Anderson and Pape (2010) explain why variance among households could lead to limit adoption; unfortunately, we do not have the individual-level data to test for the importance of this type of variance). If this is the case, we should look at both specific tax sources and variance in tax revenue. Table 6 repeats the analysis in (2), using total own-source revenue, total revenue (from all sources), total tax revenue, and property tax revenue as the independent variable of interest. In addition, we also examine variation in each of these series by estimating separate regressions with the three-year moving average and the three-year coefficient of variation for each revenue source.

Table 6 shows the results from these 12 models (corresponding to four revenue sources by three measures of variation). In this table, each coefficient comes from a separate regression. The coefficient on the taxation variable is statistically significant at the 5 percent level in only two of the 12 specifications, specifically where the dependent variable is total tax revenue and the ratio of property tax revenue to its three-year moving average. While the statistically significant hazard ratios suggest large effects, we are reluctant to infer too much from them for two reasons. First, although the level of total tax revenue is significantly associated with limit adoption, the level of own-source revenue — that which the median voter ultimately pays — is not significant. Second, if variability in property taxes drives limit adoption, the coefficients on the ratio of property tax to its three-year moving average and on the coefficient of variation should have the same sign; they do not. Furthermore, four of the hazard ratios in the table are less

[26] Formally, a properly specified Cox proportional hazard model does not require year dummies, as they should be captured in the $h_0(t)$ term. In practice, our model fails to converge with their inclusion.

[27] This evidence, however, is not sufficient to reject the Leviathan hypothesis. Theoretically, Leviathan power is revealed by a difference between revenues raised and revenues desired by the median voter; such a difference is possible with high or low revenue levels.

Table 5
Local Limit Adoption and Tax Revenues

	Cox Proportional Hazard		Exponential Survivor Model	
	(1)	(2)	(3)	(4)
Real own-source revenue, per capita ($1,000s)	1.220 (1.227)	1.252 (1.402)	1.225 (1.277)	1.245 (1.358)
Key covariates from cross-sectional regressions				
1 if home rule, 0 otherwise	2.892 (1.608)	2.627 (1.342)	2.889 (1.609)	2.588 (1.311)
Number of cities in MSA	0.102 (1.502)	0.105* (1.731)	0.101 (1.504)	0.099* (1.769)
Real median family income ($1000s)	1.011 (0.837)	1.024 (1.475)	1.011 (0.821)	1.030 (1.492)
Region dummies (Northeast omitted)	x	x	x	x
Time-varying covariates I	x	x	x	x
Time-varying covariates II		x		x
Tim-invariant covariates I	x	x	x	x
Time-invariant covariates II		x		x
Year fixed effects			x	x

Notes: This table reports hazard ratios from a hazard model that considers the likelihood a city adopts a local limit at time t, given that it has not adopted a limit at time $t - 1$; t-statistics are below hazard ratios. Time-varying covariates I are population, log of population, and real median family income in $1,000s. Time-invariant covariates I are region dummies, home rule status, and the number of cities in the metropolitan area. Time-varying covariates II are a dummy for state binding limits on cities, share African-American, share Hispanic, and a Herfindahl index for income. Time-invariant covariates II are a dummy for initiative power and a dummy for being the mayor-council form of government. We use the largest sample for which all variables are available. Standard errors are clustered at the city level. The number of observations is 7,204. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

than one, indicating a decrease in the likelihood of limit adoption with an increase in either the level or variability of taxation. Overall, we take these estimates as providing no strong evidence that levels or variance in taxation drive limit adoption.[28] Of course, it is possible that taxation is important, but our sample is too small to measure it with sufficient precision.

[28] One might also hypothesize that limit adoption is driven by the mix of revenue sources. Whether we include revenue shares by source or a Herfindahl index for the mix of revenue sources, we find no statistically significant relationship with limit adoption.

Table 6
Local Limit Adoption and Volatility in Revenues

	Real Per Capita Dollars	Real Per Capita Dollars Relative to 3-Year Moving Average	3-Year Coefficient of Variation
	(1)	(2)	(3)
Own-source revenue	1.239	1.237	0.228
	(1.341)	(0.494)	(0.684)
Total revenue	1.233*	0.876	2.425
	(1.882)	(0.118)	(0.521)
Total tax revenue	1.475**	1.607	0.053
	(2.104)	(0.805)	(0.622)
Property tax revenue	2.536	3.239**	0.007
	(1.355)	(2.045)	(1.116)

Notes: Each cell in this table reports the hazard ratio from a separate Cox proportional hazard model that considers the likelihood a city adopts a local limit at time t, given that it has not adopted a limit at time $t - 1$. t-statistics are below hazard ratios. The first coefficient in this table is the same specification as the first coefficient in Table 5 but with a sample of 6,949 observations. This is the largest sample available for all the estimates in this table, and all coefficients in this table come from models estimated with this sample. All models in this table use the full set of covariates from Table 5. Asterisks denote significance at the 5% (**) and 10% (*) levels.
Source: See data appendix

Finally, to complement the quantitative evidence on limit adoption and to explore other avenues for limit adoption, we read newspaper articles detailing the adoption of local limits. We were able to find newspaper accounts for 17 of the 40 limited cities. After reading the articles, we classified them by reported cause of the limit. Our qualitative work finds three primary causes of limits: (1) that taxes are too high, (2) that a politician wants to raise tax X and in return promises to limit tax Y, and (3) that an entrepreneurial politician wishes to build a reputation by leading the passage of a limit. Douglas Bruce, who spearheaded Colorado Springs' stringent limit, which in turn led to the adoption of a statewide limit, is the best example of this case. Bruce subsequently gained statewide office.[29] This case study research found no evidence that locally-imposed limits were adopted at the behest of the state.

[29] Sealover, Ed, "GOP Chooses Bruce to Fill House Seat." *The Gazette*, December 1, 2007, http://gazette.com/gop-chooses-bruce-to-fill-house-seat/article/30390.

III. FISCAL CONSEQUENCES OF LIMIT ADOPTION

A. Empirical Strategy

With this background on the causes of limit adoption in mind, we examine the fiscal consequences of limit adoption. Some of the limits we observe are likely to be strictly binding, such as those on overall revenues. However, other limits, such as a limit on property assessment increase without a similar limit on the property tax rate, have no strict ability to limit revenues; they may limit behavior only through the set of revenue choices to which a politician has access. For example, a politician can circumvent the assessment limit to raise the desired amount of revenue simply by increasing the property tax rate.

However, even if a limit never directly binds politicians' fiscal behavior, there are at least two additional reasons why revenues and limits can change concurrently. First, a limit might be adopted simultaneously with a change in population characteristics. In this case, the limit may never bind, but the level of revenues declines. We show empirically that this case is unlikely to be the exclusive explanation for our findings.

To explore the second reason limits may dampen revenues, suppose there is a cost of running for office. In this case, imposing a limit may repel candidates who prefer higher government spending, thereby changing the pool of candidates. Consequently, municipal revenues again decline without a binding limit. However, we are comfortable calling this type of result an "effect" of the limit in the causal sense, as the limit generates different political patterns and therefore changes revenues.

We begin with a simple test of whether limit adoption decreases the level of expected revenues relative to unlimited cities. To estimate whether revenues decline after limit adoption, we estimate

$$(3)\quad \ln(\textit{total revenue pc})_{c,\,t} = \beta_0 + \beta_1 \textit{year}_t + \beta_2 \textit{city}_c + \beta_3 \boldsymbol{X}_{c,\,t} + \beta_4 \textit{state limits}_{c,\,t} + \beta_5\{\textit{local limit city}\} \times 1\{\textit{post limit}\}_{c,\,t} + \varepsilon_{c,\,t}\,.$$

The dependent variable is logged municipal total revenue per capita, $\ln(\textit{total revenue pc})_{c,t}$, where c denotes city, and t denotes years 1970 through 2006.

The variable of interest in this model is $1\{\textit{local limit city}\} \times 1\{\textit{post limit}\}_{c,t}$, which takes on the value one when the observation is a limited city after the limit. The coefficient on this variable, β_5, measures the percent change in total revenue after limit adoption, relative to non-adopting cities and adopting cities before the limit.[30] If limit adoption decreases revenue, we expect $\beta_5 < 0$.

[30] It is standard,when including an interaction term to separately include both parts of the interaction. In this case, the "post-limit" period exists only for limit adopters, so a "post-limit" dummy is effectively the same as our variable of interest. In addition, a "local limit city" dummy would be collinear with our city fixed effects, which more flexibly control for any type of variation that is constant within a city over time.

Covariate $\boldsymbol{X}_{c,t}$ is a matrix of time-varying city characteristics, including population, log of population, median family income, racial composition, and other variables (see the table notes for complete list). The vector *state limits*$_{c,t}$ contains two dummy variables: the first is equal to one if the state has a self-imposed limit (state limit on itself) and the second is equal to one if the state has a limit on cities.[31]

We employ year fixed effects, *year*$_t$, to account for macroeconomic and political factors affecting all cities in a given year. We include adopting and non-adopting cities in this regression to identify the year effects primarily from non-adopting cities, which allows us to separate post-limit effects from overall macroeconomic conditions. For example, suppose limits were adopted only in economic downturns that cause revenue declines. If we did not include year fixed effects, we would overestimate the contribution of limits to revenue declines.

We also use city fixed effects, *city*$_c$. These fixed effects capture unchanging or slowly changing institutional, demographic, and cultural characteristics of cities, as well as any fixed component of state-level restrictions. With the exception of state-imposed and local limits, we observe municipal institutional data only at a single point in time. These features are therefore captured in the city fixed effects and do not enter directly.[32]

While the specification in (3) is consistent with the simplest motivation for limits, there are strong reasons to believe that it does not correspond well with the institutional details. The literature on state limits on cities recognizes limits on total revenue or expenditures as being the most stringent type. Our sample has two cities with such limits: Colorado Springs, Colorado and Anchorage, Alaska. Even these most restrictive limits do not attempt to lower total revenue levels. Instead, these limits restrict future increases in revenues to population plus inflation. In the specification above, if all cities had limits like these two cities, there would be no change in revenues levels after limit adoption, or $\beta_5 = 0$, since our specification uses real per capita dollars. However, these very stringent limits do attempt to decrease the growth rate of revenues, in extreme cases to zero.

More generally, a limit may decrease revenues in a given category if it restricts growth in both the tax base and tax rate in that category. To decrease total revenue, a limit must bind on all tax rates and bases. Consider limits on assessments, which generally restrict the growth of assessed values to *x* percent per year. If tax rates were fixed (the fixedness of tax rates varies greatly by jurisdiction), such a limit should slow the growth of revenues rather than cause an absolute decline. Of the examples we present in Table 2, only two of the limits implicitly restrict both the tax base and rate, and one

[31] For state limits on cities, we only use limits or combinations of limits that are considered to be "potentially binding," as defined in footnote 20.

[32] An alternative source for time-varying institutional data is the International City/County Management Association (ICMA), which conducts a survey of municipal forms of government every five years. The survey's response rate, however, is under 50 percent, and responses come disproportionately from cities that are professionally managed (ICMA, 2006). The Census of Governments remains the most complete source of data on local institutions.

is Anchorage's limit described in the previous paragraph. The other similar example in the table is the limit on the growth — not the level — of the total property tax levy in Lincoln, Nebraska. For these reasons, we believe that limit adoption is most likely to impact revenue growth, rather than revenue level.

To examine the impact of limits on the rate of change in revenues, we estimate (4) below. Equation (4) modifies (3) by replacing the "post-limit" dummy with an interaction of "post-limit" and a time trend; it also includes a linear trend for revenue change in non-adopting cities and a trend for adopting cities before the limit (to include all these trends, we drop an additional year dummy). Thus, instead of measuring whether on average cities with limits have a level change in revenues after the limit, we measure whether the annual rate of change in revenue differs after limit adoption.

Specifically, we use a linear trend variable, t (we omit the t subscript), interacted with three indicator terms for limit status. The trend variable t increments by one for each year of the sample and yields a coefficient that reports the average percentage point change in the dependent variable, net of covariates. The first of the three indicator variables for limit status that we interact with the trend is $1\{\textit{never local limit city}\}_c$, which is equal to one for cities that never adopt a local limit. The coefficient on this term reports the average annual percentage point change in total revenues for never limited cities. The second indicator that we interact with the trend is $1\{\textit{local limit city}\} \times 1\{\textit{pre limit}\}_{c,t}$, which is one for limit-adopting cities before adoption. The third indicator variable we interact with the trend is $1\{\textit{local limit city}\} \times 1\{\textit{post limit}\}_{c,t}$, which is one for limit-adopting cities after adoption. The interpretation of coefficients on these terms are, respectively, the average annual percentage point change in revenues before local limit adoption and the average annual percentage point change in revenues after local limit adoption. These three interaction terms are mutually exclusive. Each observation has a non-zero value for one of the three trend variables in each year.

The final estimating equation is therefore:

$$\text{(4)} \quad \ln(\textit{total revenue pc})_{c,\,t} = \beta_0 + \beta_1 \textit{year}_t + \beta_2 \textit{city}_c + \beta_3 \boldsymbol{X}_{c,\,t} + \beta_4 \textit{state limits}_{c,\,t} + \beta_5 t \times 1\{\textit{never local limit city}\}_c + \beta_6 t \times 1\{\textit{local limit city}\} \times 1\{\textit{pre limit}\}_{c,\,t} + \beta_7 t \times 1\{\textit{local limit city}\} \times 1\{\textit{post limit}\}_{c,\,t} + \varepsilon_{c,\,t}.$$

This specification generates two potential counterfactuals for the percentage point change in revenues after the limit (β_7): the percentage point change in revenue for non-limited cities (β_5) and the percentage point change in revenue for cities with limits before the limit was adopted (β_6). A comparison of the post-limit rate of change to the pre-limit rate of change (β_7 versus β_6) is likely the cleaner empirical test because both the treatment and control groups, by construction, possess the same time-invariant component of municipal selection into limit adoption (for which we control via the city fixed effect). However, a comparison of the percentage point change in post-limit revenue relative to the percentage point change in revenue in never-limited growth (β_6 versus β_5) is also empirically interesting. Never-limited cities are a good counterfactual if their growth is similar to limited cities pre-limit; in the next subsection, we show that this is true empirically.

To claim that the differences between limited and unlimited cities are causal, limits must be randomly assigned across cities and time. As described above, limits are clearly not adopted randomly. However, the covariates we have identified as being statistically significantly associated with limit adoption — median income, home rule status, and the number of cities in the metropolitan area — change either slowly or not at all.[33] Thus, these characteristics are well-captured by city fixed effects. In addition, our time series investigation into the causes of limit adoption (see the end of Subsection II.C) finds no statistically significant time-varying correlates of limit adoption. If there are no time-varying observed components correlated with limit adoption and if the same holds for time-varying unobserved components, the fixed effects approach suffices to identify a causal effect.

Of course, it is possible that unobserved features of cities, such as the political tastes of citizens, do co-vary in time with limit adoption in a way we have not been able to capture with our data. Further, it is possible that our comparison of the rate of revenue change before limit adoption (β_6) with the rate of revenue change after the limit (β_7) or the rate of revenue change in non-adopting cities (β_5) picks up the difference in these time-varying unobservables rather than (or in addition to) the impact of the local limit. In the event that these unobservables are quite important, we use three additional procedures to more accurately estimate a causal effect. We first use matching to better pair adopting cities with non-adopting cities. Second, we use graphical evidence to show that our findings are unlikely to be driven by pre-existing trends in revenue. Third, we statistically investigate whether revenues decline in advance of limit adoption in order to rule out anticipatory effects of limit passage.

B. Results

We begin with evidence on whether the adoption of a local limit is associated with an absolute decline in own-source revenues. The first column of Table 7 shows estimates of β_5 from (3) for all cities. In this panel, there are 279 non-adopting cities, 21 cities that adopt a limit at some point between 1970 and 2006, and 17 cities that adopt a limit either before or after our sample period (we omit the two limited cities for which we do not observe a date of adoption). This column reports coefficients from a specification with only city and year fixed effects. The second column adds demographic controls, and the third column adds the two types of state limits. The fourth column omits the 17 limited cities that adopted limits before 1970.

Regardless of specification, we find that limits have an insignificant negative effect, in the range of 3 to 4 percentage points, on revenues. This is a plausible decline, but it

[33] Over our 30-year sample, the variance of median income is three times larger across cities than within cities.

Table 7

Local Limit Adoption and Total Revenue Per Capita

	Total Revenue				Own-Source Revenue
Dependent Variable:	(1)	(2)	(3)	(4)	(5)
1{limit adopting city} × 1{after limit adoption}	–0.039	–0.029	–0.029	–0.032	–0.041
	(0.029)	(0.038)	(0.038)	(0.038)	(0.046)
Year fixed effects	x	x	x	x	x
City fixed effects	x	x	x	x	x
Demographic controls		x	x	x	x
State limits, on cities and states			x	x	x
Drop pre-1970 adopters	no	no	no	yes	no
Observations	10,183	10,183	10,183	9,637	10,183
Number of unique cities	317	317	317	300	317

Notes: Dependent variable is per capita total own-source revenue, and standard errors are clustered at the city level. Each column is a separate regression, and we report the coefficient β_5 from (5). 21 cities with local limits report a year of limit adoption over the period observed (1970–2006); 17 cities report dates of adoption outside of our sample period. Demographic control variables are population, share of people age 25 and over with a college degree, share of people age 25 and over with a high school degree or more, number of families, number of housing units, number of occupied housing units, civilian labor force 16 and over, real median per capita income, people below the poverty level, share African-American, share Hispanic, share employed in government, share of housing units built since last census, housing units built before 1940, share manufacturing employment, number of occupied housing units with more than 1.01 people per room, share service employment (by industry), share wholesale/retail employment, unemployment rate, share of people less than age 18, share of people greater than age 65, share of population of foreign origin, number of vacant housing units, and log of population. "State limits, on cities and states" refers to two variables: one is a dummy equal to one if the state has a potentially binding limit on municipalities (defined as either a property tax levy limit, a general revenue or expenditure limit, or a property tax rate limit combined with an assessment limit) in that state in a given year, and the other is a dummy equal to one if the state has a limit on state spending in that year.

Source: See data appendix

is never statistically significantly different from zero at the 5 percent level. This null finding is consistent with the design of limits, which generally try to restrict revenue growth rather than cause declines in revenue levels. The final column in the table uses own-source revenue as the dependent variable and shows that this pattern is not driven by significant decline in own-source revenues that is made up through intergovernmental revenue.

However, we believe that the institutional details more closely motivate the specification in (4), which tests whether the rate of change of revenues differs after limit adoption. Table 8 presents results from this estimation. The left panel again includes all cities and uses the same pattern of covariates as in Table 7. Regardless of specification, the results are consistent. After limit passage, Column 3 reports that cities with a limit have revenue growth of 1.7 percent per year ($\beta_7 = 0.0165$). Panel B reports that this growth rate is a statistically significant 15 percent smaller than the pre-limit trend. In addition, we can reject (with $p = 0.025$) the hypothesis that the post-limit trend is greater than the pre-limit trend (the third row in Panel B).

We also compare the post-limit growth rate of 1.7 percent in Column 3 to the 1.9 percent growth rate of never-limited cities ($\beta_5 = 0.0185$). Such a comparison makes sense only if revenues in never-limited cities grow at the same rate as revenues in limited cities before the limit. Our hazard models in Section II.C suggested this finding, and these results provide additional supporting evidence. When the dependent variable is total revenue, the p-values for a test of equality between the never-limit and pre-limit trends are never lower than 0.6. That is, we can never reject that the pre-limit and never-limit growth trends are equal. This suggests that never-limited cities may provide a good counterfactual for limited cities.

Having ascertained that never-limited cities are a plausible control group, we now compare the post-limit growth rate in never-limited cities to that in limited cities. The bottom half of Panel B in Table 8 reports that post-limit revenue growth in limited cities is 10 percent lower (though not statistically significantly lower) than in never-limited cities. We can reject the hypothesis that the post-limit trend is greater than the never-limit trend with a p-value of 0.076 (the final row in Panel B).

We put our estimated effect of limits on revenues into context in two ways.[34] Relative to the literature on the effect of tax and expenditure limits, our finding of a 15 percent effect is large. Many studies find no effects of limits at all (Kousser, McCubbins, and Moule, 2008; Mullins, 2004). Studying the effect of property tax limits in Oregon on school behavior, Figlio (1998) finds a 5 percent decrease in the student-teacher ratio.[35] In dollar terms, the estimates in Column 3 suggest that after 10 years, unlimited cities

[34] Our survey evidence also argues that limits have a fiscal effect. Roughly half of cities (responses from officials weighted to correspond to one answer per city) suggested that the limit had some effect; see Appendix Table 5 for details.

[35] Our results are, however, substantially smaller than those found by Dye and McGuire (1997) for a property tax restriction in the Chicago area; this may be due to the fact that Dye and McGuire analyze state-imposed rather than self-imposed limits.

Table 8

Local Limit Adoption and Trend in Own-Source Revenues Per Capita

	Total Revenue			Own-Source Revenue	Total Tax Revenue	Property Tax Revenue	Total Revenue
Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Coefficient Estimates and Specification							
Linear time trend ×							
1{never local limit city} [β5]	0.0227***	0.0185***	0.0185***	0.0154***	0.0052	–0.0104*	0.0197***
	(0.0008)	(0.0033)	(0.0032)	(0.0037)	(0.0042)	(0.0057)	(0.0033)
1{local limit city, pre-limit} [β6]	0.0233***	0.0195***	0.0195***	0.0152***	0.0088*	–0.0058	0.0206***
	(0.0020)	(0.0038)	(0.0038)	(0.0046)	(0.0048)	(0.0066)	(0.0040)
1{local limit city, post-limit} [β7]	0.0202***	0.0165***	0.0165***	0.0126***	0.0048	–0.0116*	0.0185***
	(0.0011)	(0.0034)	(0.0033)	(0.0041)	(0.0043)	(0.0062)	(0.0036)
Year fixed effects	x	x	x	x	x	x	x
City fixed effects	x	x	x	x	x	x	x
Demographic controls		x	x	x	x	x	x
State limits, on cities and states			x	x	x	x	x
Drop pre-1970 adopters	no	no	no	no	no	no	yes
Observations	10,183	10,183	10,183	10,183	10,177	10,166	9,637
Unique cities	317	317	317	317	317	317	300

Table 8 (Continued) Local Limit Adoption and Trend in Own-Source Revenues Per Capita

	Total Revenue			Own-Source Revenue	Total Tax Revenue	Property Tax Revenue	Total Revenue
Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel B. Percentage Difference in Trends							
(Post-limit trend) – (pre-limit trend)	–0.132**	–0.153**	–0.152**	–0.1699	–0.453*	1.006	–0.102*
Standard error	(0.053)	(0.067)	(0.068)	(0.106)	(0.243)	(1.400)	(0.061)
p-value, test (post-limit ≥ pre-limit)	0.016	0.023	0.025	0.075	0.016	0.022	0.062
(Post-limit trend) – (never-limit trend)	–0.109*	–0.108	–0.109	–0.179	–0.086	0.117	–0.063
Standard error	(0.057)	(0.076)	(0.076)	(0.119)	(0.317)	(0.266)	(0.091)
p-value, test (post-limit ≥ never-limit)	0.032	0.078	0.076	0.050	0.394	0.324	0.248

Notes: This table presents estimates for β_5, β_6, and β_7 from (6). Standard errors are clustered at the city level. 21 cities with local limits report a year of limit adoption over the period observed (1970–2006); 17 cities report dates of adoption outside of our sample period. Control variables are population, share of people age 25 and over with a college degree, share of people age 25 and over with a high school degree or more, number of families, number of housing units, number of occupied housing units, civilian labor force age 16 and over, real median per capita income, people below the poverty level, share African-American, share Hispanic, share employed in government, share of housing units built since last census, housing units built before 1940, share manufacturing employment, number of occupied housing units with more than 1.01 people per room, share service employment (by industry), share wholesale/retail employment, unemployment rate, share of people less than age 18, share of people greater than age 65, share of population of foreign origin, number of vacant housing units, and log of population. "State limits, on cities and states" refers to two variables: one is a dummy equal to one if the state has a potentially binding limit on municipalities (defined as either a property tax levy limit, a general revenue or expenditure limit, or a property tax rate limit combined with an assessment limit) in that state in a given year, and the other is a dummy equal to one if the state has a limit on state spending in that year. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

have $37 higher annual per capita revenue than limited cities. This is almost exactly the same amount that Shadbegian (1999) finds for the effect of state-imposed limits on local own-source revenues. Put differently, $37 is about one-third of annual spending on parks and recreation for cities in our sample (for 2002, in 2006 dollars). Broadly, our evidence is consistent with limits weakly constraining politician behavior.

The right hand set of columns in Table 8 explores the robustness of this finding. When we restrict the analysis to own-source revenues, the limits are associated with a (statistically insignificant) 17 percent decline in revenue growth. This suggests that local limit adopters do not compensate for lost local revenue by obtaining increased intergovernmental transfers from either the state or federal government. Columns 5 and 6 indicate that this decline comes from tax revenue, possibly property tax revenue; with these dependent variables, the trend coefficients are not always individually significant, though we can always statistically significantly reject the hypothesis that the post-limit growth rate exceeds the pre-limit growth rate (Panel B, the p-value from third row).

The final column of Table 8 uses total revenue as the dependent variable and drops the 17 cities with limits outside of our sample period; the results are very similar to those presented in Column 3. This empirical strategy is arguably cleaner since we now use only cities that enact a limit during the period of time included in our analysis. This specification also shows that after limit adoption, limited cities ($\beta_7 = 0.0185$) have a decline in the rate of revenue growth relative to both limited cities pre-limit ($\beta_6 = 0.0206$) and never-limited cities ($\beta_5 = 0.0197$). Comparing pre-limit to post-limit growth, we find a 10 percent decrease in the rate of revenue growth (Panel B). We reject that post-limit growth exceeds pre-limit growth in this final specification (Column 7) at the 6 percent level (Panel B, p-value, third row). Thus, regardless of sample (including or excluding early adopters), counterfactual (never-limited cities or pre-limit cities), or dependent variable, limit adoption is associated with a decline in municipal revenue growth.

The estimates in Table 8 do not distinguish between limits by revenue type. In addition, the estimates do not differentiate between the state legal context and the likelihood that a particular limit is binding. Unfortunately, our sample is not large enough to distinguish between these fine-grained categorizations. If we only include in our analysis the most rigorous limits — those that restrict the property tax and those that restrict total tax revenues or expenditures — our results remain unchanged, and the difference between the coefficients widens (Appendix Table 6).[36]

C. Evidence on Causality

The estimated coefficients show a substantive relationship between local limits and relative declines in revenue growth but require strong assumptions to yield causal relationships. Our data offer no natural experiment or obvious instrument. Thus, to

[36] We do not find any difference in post-limit revenue growth by a variety of municipal institutions: home rule status, mayor-council cities, or a binding state limit.

provide further causal evidence, we use three strategies: propensity score matching and both visual and regression tests for the presence of anticipatory revenue behavior before limit adoption.

We begin with propensity score matching, which weights non-adopting cities with covariates similar to adopting cities more heavily than the OLS estimation of (4). To provide a causal estimate, a matching procedure must meet two criteria. First, treated and untreated observations must have at least some common support; in this case, this means that there must be unlimited cities with propensity scores similar to limited cities. The second requirement for matching to yield a causal estimate is that, once observable criteria are controlled for via the match, limit status is "as good as random." This second requirement is inherently unobservable, like the exclusion restriction in an instrumental variables framework.

If these assumptions are satisfied, this empirical strategy yields causal estimates of the effects of limits on fiscal behavior. We generate propensity scores in two ways — using cross-sectional data and time-series data. For the cross-section, the propensity score uses all cities in the sample for which we observe data in 1970. With this sample, we estimate a probit for local limit adoption as a function of all the demographic and institutional data employed above (as in (3) and (2)). For the time-series matching, we use a probit model for local limit adoption in a given year as a function of the same covariates. The predicted values from these regressions are used to create weights that are employed in new estimations of (4).[37] Both types of propensity scores have some common support for limited and unlimited cities. Using propensity scores from cross-sectional information, the 10^{th}–90^{th} percentile range for unlimited cities is [0.005,0.330], while the range for limited cities is [0.065,0.707]. Using time-series information, the 10^{th}–90^{th} percentile range for unlimited cities is [0.000,0.224] and for limited cities [0.055,0.587].

The matching results in Table 9 affirm that revenue growth in limited cities declines after local limit adoption, even relative to cities that are more like adopters. The coefficients on revenue growth pre- and post-adoption using matching techniques are quite similar to those generated in our original estimates (Table 8). Panel B shows that post-limit growth is bounded between 12 and 17 percent lower than pre-limit growth. We can always reject (the third row of Panel B) that post-limit growth is greater than pre-limit growth. While we do lose some precision in our ability to distinguish revenue growth post-limit from never-limited growth (the bottom half of Panel B), the point estimates tell a broadly similar story to that of Table 8.

The remaining challenge to the claim that limits causally affect revenue growth, in our opinion, is the possibility that public sentiment about the size of government changes concurrently with limit adoption. We evaluate this possibility graphically and then statistically. Figure 2 shows the log of total revenue per capita as a function of the time to limit adoption, where the limit begins in year one. Each dot in Figure 2a is

[37] Our approach follows Imbens (2004). The weights are $\lambda_c = \left[\text{limit}_c / e(X_c) + (1 - \text{limit}_c) / (1 - e(X_c))\right]^{1/2}$, where the propensity score is $e(X_c)$ (the fitted value from the matching estimation) and $limit_c$ is coded one for cities that have a locally adopted tax and expenditure limitation and zero otherwise. When we use time series data in the matching process, the equation has a t subscript in addition to the c subscript.

Table 9

Using Matching to Test Results

	Non-Adopters and Adopters, Including Adoption Before 1970		Non-Adopters and Adopters, Excluding Adopters Before 1970	
	Propensity Score Based on		Propensity Score Based on	
	Cross-Section	Time Series	Cross-Section	Time Series
	(1)	(2)	(3)	(4)
Panel A. Coefficient Estimates				
Linear time trend ×				
1{never local limit city} [β5]	0.0179***	0.0177***	0.0199***	0.0194***
	(0.0037)	(0.0034)	(0.0040)	(0.0037)
1{local limit city, pre-limit} [β6]	0.0190***	0.0189***	0.0222***	0.0212***
	(0.0042)	(0.0040)	(0.0045)	(0.0042)
1{local limit city, post-limit} [β7]	0.0158***	0.0158***	0.0196***	0.0188***
	(0.0040)	(0.0036)	(0.0044)	(0.0040)
Year fixed effects	x	x	x	x
City fixed effects	x	x	x	x
Demographic controls	x	x	x	x
State limits, on cities and states	x	x	x	x
Observations	7,714	8,764	7,201	8,218
Number of cities	317	317	300	300
Panel B. Percentage Difference in Trends				
(Post-limit trend) – (pre-limit trend)	–0.1655**	-0.1630**	–0.1201**	–0.1151**
Standard error	(0.0634)	(0.0673)	(0.0574)	(0.0593)
p-value, test (post-limit ≥ pre-limit)	0.006	0.015	0.022	0.033
(Post-limit trend) – (never-limit trend)	–0.1145	–0.1080	–0.0162	–0.0336
Standard error	(0.0809)	(0.0772)	(0.0788)	(0.0825)
p-value, test (post-limit ≥ never-limit)	0.071	0.079	0.419	0.342

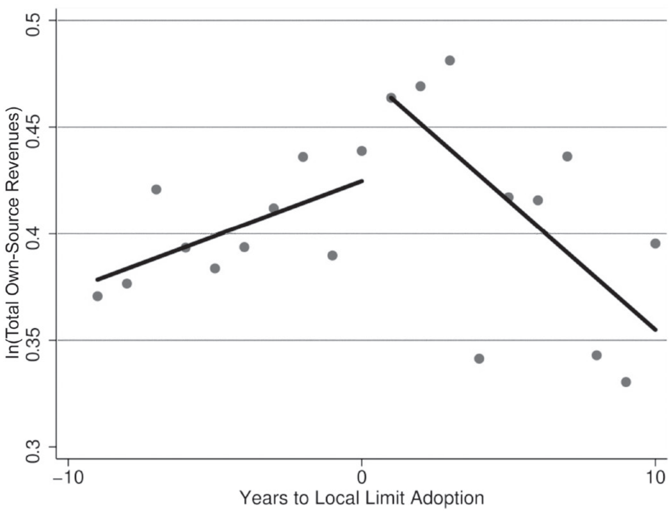
Notes: This table repeats columns the estimation in Columns 3 and 7 of the previous table, using propensity score weights. Standard errors are clustered at the city level. For the cross-section, the propensity score estimation uses cities in the sample for which we observe data in 1970. With this sample, we estimate a probit for local limit adoption ever as a function of population, share of people age 25 and over with a college degree, share of people age 25 and over with a high school degree or more, number of families, number of housing units, number of occupied housing units, civilian labor force age 16 and over, real median per capita income, people below the poverty level, real median gross rent, share African-American, share Hispanic, share employed in government, share of housing units built since last census, housing units built before 1940, share manufacturing employment, number of occupied housing units with more than 1.01 people per room, share service employment (by industry), share wholesale/retail employment, unemployment rate, share of people less than age 18, share of people greater than age 65, share of population of foreign origin, number of vacant housing units, log of population, and census division dummy variables (9 divisions). For the time-series matching, we estimate a probit for local limit adoption in a given year as a function of the same covariates. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

Sources: Authors' survey; U.S. Census Bureau; see Section III in text for complete details

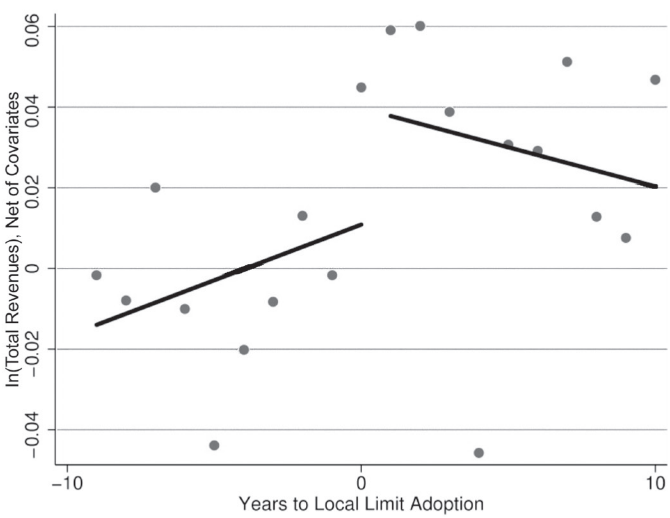
Figure 2

Fiscal Consequences of Limit Adoption

Panel A. Revenues and Time to Adoption



Panel B. Revenue Residuals and Time to Adoption



Notes: Panel A presents average annual log of real per capita municipal revenues for limited cities, where year one is normalized to be the year of limit adoption. Panel B presents the same cities but reports average annual log of real per capital municipal revenue net of city fixed effects, year fixed effects, and a linear trend for cities that do not adopt limits. The solid lines are best-fit lines for the 10 years pre- and post-limit adoption.

average revenue for all ever-limited cities by years before and after limit adoption. We include separate best-fit lines for the 10 pre- and post-limit years. Suppose that public sentiment changes slowly but that limit status changes discretely, as seems very likely empirically. In this case, if public sentiment drove the results we observe, the average growth rate of revenues should begin to decline before the limit is adopted. This figure shows that the data do not support this hypothesis. After limit adoption, in both the near and longer terms, the rate of revenue growth decreases.

Of course, this analysis is subject to concerns about which cities are likely to be early or late limit adopters and the prevailing macroeconomic conditions at the time of adoption. For this reason, we use city and time fixed effects in the regression. Figure 2b reports the average of residuals for limit-adopting cities from a regression of log real revenue per capita on city fixed effects, year fixed effects, and a linear trend for non-adopting cities (specifically, the residuals from (4), omitting the β_3, β_4, β_6, and β_7 terms). Because these residuals are net of year fixed effects that are identified from limited and non-limited cities, this figure is somewhat analogous to a difference-in-difference specification. Again, there is no visual evidence that the pre-limit trend in revenues has changed.

We test this argument more concretely by dividing the pre-limit trend term in (4), $\beta_6 t \times 1\{local\ limit\ city\} \times 1\{pre\ limit\}_{c,t}$, into two parts, one to measure the trend in revenues far from limit adoption and another to measure the trend close to adoption. Close and far are defined as being j years away from limit adoption. These terms are therefore $\beta_{6a} t \times 1\{local\ limit\ city\} \times 1\{pre\ limit\} \times 1\{t \leq adoption\ year - j\}_{c,t}$ for the years far before limit adoption and $\beta_{6b} t \times 1\{local\ limit\ city\} \times 1\{pre\ limit\} \times 1\{t > adoption\ year - j\}_{c,t}$ for the j years before limit adoption. Using $j \in \{2, 3, 4, 5\}$, we find that we can reject the hypothesis that the pre-limit rate of revenue growth near the limit (β_{6b}) is lower than the post-limit rate of revenue growth (β_7). Put differently, the timing of the discrete break at the limit is important, and the trend in revenues pre-limit does not begin to change shortly before limit adoption.

In sum, we find that limited cities decrease revenue growth after the adoption of a local limit. This is true even when we use matching to compare limited cities more closely to cities with the same observed characteristics. Other evidence suggests it is unlikely that our result is driven by preference changes that occur at a similar time as the adoption of the limit.

IV. DISCUSSION

Most of the existing literature explains fiscal restraints as a consequence of electoral institutions that fall short of implementing the preferences of the median. When the median voter's preferred policy is not implemented, institutional substitutes, such as direct democracy (i.e., ballot initiative), may alleviate welfare losses. Consistent with this view, seminal papers by Buchanan and Tullock (1962), Niskanen (1971), and Romer and Rosenthal (1978, 1979) argue that the ballot box provides little if any constraint. They contend that either government is fundamentally a Leviathan (a government that maximizes revenue) or agency problems in political representation are severe; thus, societies must turn to constitutional rules to constrain government.

There are several reasons why a self-imposed fiscal limit may be used as a possible remedy for institutional failure. First, voter uncertainty about the actions of politicians (moral hazard) combined with a desire to hold them accountable may create incentives for voters to constrain their behavior (Besley and Smart, 2007). Alternatively, an incentive to limit government may arise when the type of politician elected to office is unobserved by voters (adverse selection), whereby spending on public goods may exceed that preferred by a median voter with less demand for public goods. Yet another explanation derives from intertemporal uncertainty, whereby the median voter desires to limit government due to uncertainty about whether the preferences of the median voter in the future will differ from her own (Tabellini and Alesina, 1990; Anderson and Pape, 2010).

Given our empirical findings, particularly the lack of evidence on a connection between pre-limit tax revenues and patterns of adoption, we believe that adverse selection rather than moral hazard may be partially driving voter incentives to adopt these limits. To understand how this might be the case, we propose a variant of the model constructed by Coate and Knight (2011), where voters across municipalities differ in their demands for public goods and where voters are uncertain about both the cost of public goods and the type of politician elected for office. Although there is real option value in spending more on public goods when costs are low, uncertainty may create an incentive for voters with low demand for public goods to limit spending. Thus, when voters prefer relatively less public goods, then a self-imposed limit can curb spending if a politician with a high taste for public goods is elected.

Although preferences for local public goods are typically unobserved, many researchers have argued that income can be a relatively good proxy for preference types. Specifically, both across and within countries, demand for public goods increases with income. (Seminal contributions in this literature are Borcherding and Deacon (1972) and Bergstrom and Goodman (1973); a recent contribution is Hokby and Soderqvist (2003), and Lindauer (1988) provides cross-country evidence). Thus, if income is a good proxy for preference type, wealthier cities may be less likely to adopt expenditure limits. Indeed, this conjecture is consistent with our empirical finding that limit adoption is more likely in less wealthy municipalities.

In sum, this explanation highlights agency problems inherent in representative government and motivates demand for a fiscal limit without using the obvious assumption of a Leviathan government. This is an assumption that appears to be without empirical merit in our case. We formally develop this argument in our online theory appendix and leave the empirical investigation of the precise mechanisms underlying local limit adoption for future research.

V. CONCLUSION

We document that at least one in eight cities has a self-imposed restriction on its ability to tax or spend. To the best of our knowledge, this type of self-imposed municipal limit has not been analyzed in the literature. We use these limits to explore when the power of the median voter suffices to limit government and when institutional constraints are required to curb representative government. Since limit adoption is prevalent, we show that the power of the median voter is surely not sufficient in all cases.

We find that limit adoption is less likely in higher income cities and in cities located in metropolitan areas with more jurisdictions. Limit adoption is also substantially more likely in home rule jurisdictions. We find no evidence that limit adoption is statistically significantly associated with the level of tax receipts.

Finally, our evidence suggests that limits have fiscal consequences. After the adoption of a limit, municipal revenue growth in the average limited city declines by 13 to 17 percent.

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DISCLOSURE

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