Institutional Influences on Local Government Finance

in Three Essays

by

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ABSTRACT

Local governments are creatures of their ecosystems. In this dissertation, institutional influences on local government finances are assessed both theoretically and empirically in this three-essay dissertation. Employing two tax and expenditure limitations (TELs) and local government form, this dissertation evaluates how these three components along with the local government ecology, influence financial outcomes around local fiscal condition, revenue capacity, and forecasting bias.

First, this dissertation examines the effect of removing assessment restrictions on the solvency of local governments. As all TELs are not equivalent, expected impacts from assessment restrictions should be comparatively minimal. Using a multiple synthetic matching design, I match Minnesota municipalities against weighted counterfactuals before the lifting of assessment restrictions in 2011 and evaluate outcomes. Results indicate that municipal solvency was unaffected by a release from assessment restrictions.

The second essay evaluates the moderating effect of voter support of TELs on property taxes. I propose that municipalities in favor of restrictions would have limited tax growth, even without restrictions; and oppositional constituencies face the greatest shift. Using voter support for the Taxpayer Bill of Rights Amendment in Colorado as a moderator, constituent preferences differentiate the change in property tax trends from implementation of the amendment. Employing both a Hausman-Taylor model and a comparative matching design, a significant relationship is found between the impact of property tax restrictions and the preferences of local government voters.

In the last essay, I investigate an association between form of government and municipal revenue forecasting bias. Granting that a municipal governments form alters the nature of the governance that it provides; the essay presents that a reformed council-manager form of government would have lower revenue forecast bias via political pressure than a mayor-council form. Results from pooled ordinary least squares design indicate no statistically significant relationship between forecast bias and municipal form of government.

The dissertation serves to illuminate, and eliminate, some institutional predictors of local government finances, and intones an ecological dominance over local government finance. Further, the dissertation provide significant nuance in how additional research can provide definitive answers on the effects of structural changes on the finances of local governments.

DEDICATION

To my friends, colleagues, mentors, and especially my family:

Without you I never would be at this point.

Thank you.

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TABLE OF CONTENTS

Page

CHAPTER Page

LIST OF TABLES

Table Page

LIST OF FIGURES

1: INTRODUCTION

Local governments are creatures of their ecosystem; their state structure, constituencies, and their own structure shape operations. Whether it's Arizona's decision to restrict municipal plastic bag bans, Malibu Public Library's choice of celebrity speakers, or Austin's increasing pressure to move towards a mayor-council form; the constraints and available decisions of each local government are vast. However, none of these choices are made without influence from their state own government, the constraints imposed upon them by their own constituents, and their own form of governance.

Without understanding how the surrounding objects influence the financial operation of the local government, completing the goals of the government itself becomes much more difficult. Each piece of insight from this dissertation will not only allow local governments to better serve their constituents, but to simultaneously provide insight to constituents themselves about how their local government should operate. In all three essays, the public ultimately controls how their local government operates, yet the public is operating with incomplete information. This dissertation looks to fill important gaps in public and government understanding, while – in parallel – filling the gaps of academic understanding.

This dissertation examines a three streams of institutional influence on local government finances. The scope encompasses drivers of fiscal capacity and planning within local governments. The aim is to provide new understandings for local governments and their constituents about their finances. If the municipality or state is considering adoption or removal of tax and expenditure limitations or new governmental

forms the additional information from this dissertation can serve to inform those decisions.

The following dissertation is split into three separate and related essays and mirrors the layout by chapter below, a short conclusion follows in a closing fifth chapter. Each chapter is relatively consistent with the standard journal format for quantitative content, comprising an independent introduction, theory and prior literature, methodology, results, and discussion and conclusion.

This dissertation explores institutional influences on local government finance; specifically, revenue generation, restriction, and forecasting. In expanding the understanding of tax and expenditure limits, the first paper examines the often-ignored nuance involving diverse types of tax and expenditure limits (where states impose restrictions on the capacity of local governments to raise or spend revenues). While tax and expenditure limits are widely researched (see, Stallman, Maher, Deller and Park, 2017 for a full review), the nuance of the 'less stringent' restrictions have mostly been overlooked. The first chapter utilizes a removal of assessment restrictions in Minnesota to evaluate whether freeing local governments from this restriction impacts short-term solvency and total revenues.

Continuing, and on tax and expenditure limitations, the second essay examines the heterogeneous impacts of tax and expenditure limits within a single state. While within-state tax and expenditure limits have been examined (Skidmore and Tosun, 2011; Park, Maher, and Ebdon, 2018; and Martell and Teske, 2007); and the influence of voter preference on tax and expenditure limitations has been evaluated at the state level (Eliason and Lutz, 2018). The second essay tackles the combination of these subjects,

analyzing how property tax revenues in local governments with different constituent preferences shift as a result of the implementation of TABOR in Colorado.

The third institutional context for local government finances is the structure of the local government itself. With a new movement against the 'reform' manager-council taking hold in Austin, Texas, more cities may rethink their choice between governmental forms. As noted in Carr's (2015) review, the structure of municipal government influences a wide array of outcomes. More recently, municipal form of government is found to be associated with municipal solvency (Jimenez, 2020), corruption (Nelson and Alfonso, 2019), and hiring behaviors (Einstein and Glick, 2018); the impacts of municipal form of government on bias in revenue forecast still remains to be seen.

Additionally, the dissertation employs a variety of analytical techniques to address the questions presented, complementing several already rich tracks in analytical understanding. The first essay employs a both a standard differences-in-differences design along with the use of the synthetic model – demonstrating the growing use of multiple matching under the synthetic approach (Zeng, Zhao, and Dai, 2020). Essay two similarly employs two techniques: a modified synthetic approach portraying the importance of matching strategy (see, Dube and Zipperer, 2015), while also using the (in my opinion) underutilized Hausman-Taylor design. The third essay uses the 'simple' form pooled OLS but makes clear the needs of additional data standardization to achieve unbiased and rational results.

Most importantly – as Selznick (1996) quoted of Dewey (1938) – "any problem that does not grow out of actual (or "practical") social conditions is facitious;" and goal of this dissertation is to provide practicable information for better governmental choices.

During the spring legislative session, Nebraska's Governor Ricketts noted the need for the state to entertain increased local government tax restrictions (Ozaki, 2021); will it change perspectives if research intones that some municipalities will be unchanged and others highly restricted? As noted, the City of Austin faces a movement to move away from the council manager form of government; would a change from "reformed" municipal governance face less scrutiny with the knowledge that it is not associated with different levels of budget forecast bias?

2: MISSING THE TEL FOR THE FOREST

The Impact of Assessment Limits on Municipal Fiscal Health - Evidence from Minnesota

Abstract

Municipal tax and expenditure limitations (TELs) have received attention linking TEL restrictiveness to negative outcomes in municipal fiscal wellbeing. However, research linking TELs and municipal fiscal health does not describe impacts of individual TEL types, such as levy ceilings, or assessment restrictions. This research employs Combined Annual Financial Reports to examine impacts of assessment restrictions on the fiscal health of municipalities from 2009 through 2014. Using synthetic case-control design I find removal of assessment restrictions in Minnesota in 2012 had no statistically significant impact on municipal fiscal health and minimal, erratic increases in revenue. Results are – in part – counter to previous research that examined TELs in aggregate. The paper further suggests that future research on TELs investigate specific TEL types rather than aggregate measures to be meaningful to policy-makers for intervention.

Introduction

Concerns over municipal fiscal health and the ability for municipalities to meet financial obligations still remain as municipalities emerge from the Great Recession (Yang, 2017). Full fiscal recovery remains elusive for many municipalities that not only struggle to return to pre-recession revenue levels, but also face a growing backlog of infrastructure projects. States have the ability to constrict or enable fiscal recovery for municipalities. State policy makers have a wide range of tools to impact the fiscal outcomes for municipalities. Tax and expenditure limitations (TELs) – which limit tax increases, cap expenditures or require voter notification before tax increases – are just one of those tools. Consequently, providing state policy actors with an understanding how to best position municipalities for fiscal success and understanding how to enable recovery before the next economic downturn is imperative.

Despite some improvements since the Great Recession has ended; recovery to pre-recession levels of revenue, service delivery, pension funding and infrastructure remain distant (Kim and Warner, 2016; Maher, Park and Harrold, 2016; Wang and Wu, 2018). Some municipalities may deal with the lack of full recovery through austerity measures such as sales of infrastructure, outsourcing services, or layoffs, but questions remain whether these are effective long-term solutions. Some municipalities have resorted to user fees to make up lost ground while some have resorted to employing local option sales taxes. (Wang, 2018),

Other municipalities recovering from economic downturns may see increasing property values as a means of recovering lost revenue from an economic downturn;

however, restrictions on municipal property assessments may limit the capacity of municipalities to take advantage of increasing property values. From 1978 through 2012, Minnesota municipalities were unable to assess a property at full value if the market value increased at a rate higher than assessment restriction[12]. During periods of strong property value growth, municipalities instead assessed properties at a restricted value based on maximum limits, potentially inhibiting property tax revenues along with assessments.

Starting in 2012, the state of Minnesota lifted restrictions on assessment values. The removal of the restriction came at a pivotal transitional economic time, when property values were beginning to rise to levels consistent with pre-recession values. It was during this transition that Minnesota's legislature, via H.F. 677, made the immediate shift to away from restrictions starting with the fiscal year in 2012. The change in policy required assessment and taxation of property in Minnesota at the market rate, in lieu of the restricted valuation. Thus, municipalities in states maintaining assessment restrictions potentially faced an inability, unlike Minnesota, to increase assessed values of taxable property.

The assessment limits that Minnesota faced prior to 2012 were just one example of state limitations on municipal taxation – tax and expenditure limitations (TELs) – which restrict revenue options for municipalities by limiting tax rate increases, capping expenditures or requiring voter notification before tax increases. As shown in Table 1, 19 states currently employ assessment restrictions. Thus, the potential to impede fiscal

[1] The exact limits during the period 2009-2011 were assessments at 15 percent higher than assessed in the preceding year or 25 percent higher than the preceding assessment year if the assessment was more than one year prior

[2] H.F. 2695 Minn. Leg. 2009, H.F. 677 Minn Leg 2013, Minn Stat. § 273.11, subdivision 1

health for many municipalities exists when rate increases are not readily available. Coping with the politics of tax reduction, recovery from the Great Recession, infrastructure demands, and bankruptcy, municipalities face a particularly difficult financial landscape going forward.

Table 1 – TELs Typology

TEL Type	Definition	Variants	States Employing
Levy Limit	A limit on the total amount collected by a municipality employed towards expenses or the development of reserve funds	Caps or year over year increases	Alabama, Alaska, Arkansas, Colorado, Delaware, District of Columbia, Idaho, Illinois, Indiana, Kansas, Kentucky, Louisiana, Maine, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, Montana, Nebraska, Nevada, New Jersey, New Mexico, New York, North Dakota, Ohio, Pennsylvania, Rhode Island, South Dakota, Texas, Utah, Virginia, Washington, West Virginia, Wisconsin
Assessment Limit	Limits on the growth in assessed value of property used in determining property tax	Year over year increases, restrictions to new assessment for consistent owner	Arizona, Arkansas, California, Colorado, Connecticut, Florida, Georgia, Iowa, Maryland, Michigan, Minnesota, Montana, New York, Oklahoma, Oregon, Rhode Island, South Carolina, South Dakota, Texas
Rate Limits	A restriction on the total percentage of taxed on property, as a portion of assessed value.	Caps or year over year increases	Alabama, Alaska, Arizona, Arkansas, California, Colorado, Delaware, District of Columbia, Florida, Georgia, Idaho, Illinois, Indiana, Iowa, Kentucky, Louisiana, Massachusetts, Michigan, Missouri, Montana, Nebraska, Nevada, New Mexico, North Carolina, North Dakota, Ohio, Oklahoma, Oregon, Pennsylvania, South Carolina, South Dakota, Texas, Utah,

			Washington, West Virginia, Wyoming
Expenditure Limits	Restrictions on municipal budgeting, requiring that expenditures not exceed limits based on previous year expenditure levels	Caps or year over year increases	Arizona, California, Colorado, Maine, Michigan, Minnesota, Nebraska, New Jersey, Washington, Wisconsin
Truth in Taxation Statements	Requirements that municipalities provide information outlining property tax changes to property owners within the jurisdiction prior to effectuation	Notice of ballot measure on property tax, notice of change rate	Arizona, Delaware, Florida, Georgia, Kentucky, Maryland, Missouri, North Dakota, Tennessee, Texas, Utah, Virginia

Sources: Typology based on Lincoln Land Institute (2018) and Amiel, Stallman and Deller (2009); State use retrieved from Lincoln Land Institute (2018)

TELs are politically complicated, as they are incongruent with Tiebout (1956) Sorting, stating that a municipality meets the taxation desires of their constituency. In the case of TELs, state limits on municipal taxation potentially restrict municipalities from meeting the level of taxation desired by the citizens of a municipality. If draconian iterations of TELs are implemented at a state level, a municipality that desires to expand services for constituents may be barred from doing so based on limits to new revenues. Nonetheless, the use of TELs as property tax limits became more popular between 1960 and 1990 and have experienced little growth since this period. Minnesota in 2012 was an outlier in that it removed a TEL from prior implementation, allowing greater maneuverability in municipal taxation. As of 2017, 46 states have restricted municipal revenue through all types of tax and expenditure limitations (Table 1). These restrictions range from Colorado's host of restrictions (known as TABOR) limiting year over year rate increases, rate ceilings, spending restrictions, and limits on slack resource holding to

Tennessee's TEL package which consists only of truth in taxation statements from municipalities intended to reduce tax shifting to mask revenue sources.

Research indicates that TELs in aggregate have negative outcomes on municipal fiscal health. (Jimenez, 2017, Maher and Deller, 2013). Maher and Deller (2013) report that TELs are associated with decreases in own source revenues and slack resource holdings. However, literature examining connections between TELs and municipal fiscal health presents two major opportunities. First, the opportunity to employ causal inference techniques to TELs is limited because policy variation over the past thirty years is extremely rare. Even as municipal financial data has become more uniform and accessible, temporal policy variation in TELs has all but disappeared. Second, researchers have not investigated individual TEL implementation types; instead opting to investigate all TEL types under one umbrella, with the comingling of potential outcomes. The aggregation of TEL policies – usually implemented concurrently – leaves a gap in the literature where research fails to inform policymakers' choices among TEL packages.

In the current manuscript, I define municipal fiscal health as the ability of a municipality to meet fiscal obligations both in the short and long term, a leading indicator before insolvency occurs (Stone, Singla, Comeaux and Kirschner, 2015) and employ financial indicators to measure municipal fiscal health. Short and long-term obligations consist of a wide range of items such as meeting bond payments, local winter road maintenance, or next week's payroll obligations (Maher and Deller, 2011; Stone, Singla, Comeaux and Kirschner, 2015). If municipalities cannot meet their fiscal obligations, bankruptcy becomes a potential concern.

Municipal fiscal health can adversely impact all residents within a municipality such as through increased borrowing costs, reduced service delivery in the form of police or fire, or eventual economic decline (Mullins and Wallin, 2004; Poterba and Rueben, 1999, Shadbegian, 1999, Hendrick, 2004). Municipalities do not have the same capacity as the federal government to take on additional debt in the case of budgeting shortfalls, so budgeting shortfalls require alternative measures to ensure balanced budgets – often extending budgetary problems.

This paper evaluates the impact of Minnesota's lifting of assessment restrictions on the fiscal health of municipalities within the state in comparison to states that maintained assessment restrictions. To ascertain the impact of assessment restrictions, I employ a synthetic case-control method to develop the counterfactual and determine the average treatment effect in the three years following lifting the assessment restrictions. While the three-year period may appear short, the timing (during an economic upswing) would be expected to show immediate results. The results of the synthetic control model yield no statistically significant results – no increase in municipal fiscal health in Minnesota and minimal, but statistically insignificant increases in revenue. Although revenue outcomes show a jump one year after lifting the assessment restriction, the results across years are not systematic and interpreting the results as a causal effect of lifting assessment restrictions is not reasonable.

These results are noteworthy because they do not align with almost all of the previous research associating a strong negative association across the board between TELs and municipal fiscal health. Implications for further research include a reevaluation

of TEL measurement in quantitative literature. Further, the results of this paper call into question the conclusions of previous research as well as implications for practice.

The next section of the paper outlines literature involving the treatment of TELs and a section describing the theoretical underpinnings follows it. The subsequent section describes the use of novel, synthetic case control methods in relation to TELs and municipal fiscal health. The next section describes the results. The last section closes with a discussion and the implications on state policy practice and future research on TELs.

Literature

The topic of TEL implementations and their association with municipal fiscal health is not entirely novel. Maher and Deller (2013) conducted research connecting municipal fiscal health to TELs. They employed cross sectional data to evaluate the proposed effects within a single year. Additionally, their research looked at the impact of numerous forms of TELs implemented within a state –year over year levy limits, levy ceilings, and assessment limits on municipal fiscal health, as opposed to a single route of policy impact. These are the two limitations to this work on TELs and fiscal health: 1) treating TELs as a bundle, limiting individualized information and 2) the inability to capitalize on year over year policy variation. Studying TELs, and fiscal health and solvency, Jimenez (2017) looks at TEL restrictiveness over time and develops broader causal inference, but nonetheless treats TELs as an aggregate, failing to remedy concerns (addressed at length later) over the monolithic treatment of multiple TEL implementations.

O'Toole and Stipak (1998) present a narrow evaluation of TELs, wherein municipal own-source revenues from property taxes decrease significantly with the implementation of TEL restrictions in Oregon – the expectation of many TEL implementing policy actors. Sun (2014) expands on the picture presented by O'Toole and Stipak (1998) noting that the shift to other revenue sources in municipalities subject to TELs led to larger expenditures due to overcompensation in revenue development. This finding complicates the simple picture provided by the shift to alternative forms of revenue outside of property taxes and may indicate that TEL implementation increases overall revenue. Despite increased revenue, Sun (2014), does not provide a link to eventual fiscal health outcomes for municipalities.

TEL Measurement

TELs, like any policy has proven difficult to effectively measure with adequate nuance and deference to the myriad complication involved. Starting in the 1990's, an important set of prior literature distinguishes a tax and expenditure limit as relevant when the limit is binding on a taxing authority (Mullins and Joyce, 1991; Shadbegian, 1998; Skidmore, 1998; Corina and Walters, 2006). However, there has been little effort to clearly delineate which tax and expenditure limits are or are not binding, and for what outcomes without first seeing outcomes. As a result, evaluating the effectiveness of a TEL and simultaneously determining bindingness[3] inevitably creates a post hoc ergo propter hoc fallacy. Further, Mullins and Joyce (1991) appear to recognize the concern

[3] As framed by Mullins and Joyce (1991) bindingness is the potential constraint they "imply as a function of not only a physical ceiling, but also by public sentiment."

over bindingness and press towards the creation of a scale based on the expected stringency of TEL elements, similar to the form employed in ACIR (1985).

Until very recently, the majority of interceding research bundles multiple TEL policy options into one independent variable using the scale created by Amiel, Deller and Stallman (2009; Maher and Deller; 2011; Maher and Deller, 2013; Maher, Park andHarrold, 2016; Maher, Deller, Stallmann and Park, 2016, Jimenez, 2017). The scale scores 11 facets of TELs to construct an aggregate total score. For example, assessment restrictions receive a score of 5 for an assessment limit above 5% and a score of 8 for an assessment allowing no new assessments which is then added to other scale elements [A full accounting of Amiel, Deller and Stallman's (2009) scale is provided in Table 2]. Although a wide range of publications have accepted the scale, there are significant limitations in its construction. The scale is based on expected restrictiveness of TELs, but no research has validated the scoring used in the scale. Further, Amiel, Stallman and Deller (2009, p. 12) noted that many choices in the scale creation were "normative value judgements" and the "hope that others can experiment with alternative weighting schemes." Up until this point, research determining the appropriateness of alternative weighting or schemes is not known. Only one study, Nicholson-Crotty and Theobald (2010), has employed alternative weights but did not consider comparative weights of different policies.[4]

Table 2 – TEL Index Created by Amiel, Stallman and Deller (2009)

Local TEL Index		Points
Type of TEL	Overall Property Tax Rate Limit	7

[4] Additional research has employed dummy variables to evaluate the impacts of TELs, and clearly suffer from greater constraints than a TEL index.

	Limited to more than or equal to 2.5%	2
	Limited to more than 2.5%	1
	Specific Property Tax Rate Limit	6
	Assessment Increase	4
	No Approved increase	4
	Lower of 5% or equal to inflation of 5% whichever is less	3
	Limit less than or equal to 5%	2
	Limit more than %5	1
	General Revenue Limit	3
	No new tax or rate increase	4
	Limit equal to inflation and/or population growth	3
	Limit is less than or equal to 5%	2
	Limit is between 5% and 10%	1
Statutory/Constitutional	Full Disclosure	1
Scope	Constitutional	1
	County	1
	Municipality	1
	Special District	1
	Other	1
Overrides/Exemptions	Sales Tax Option	-1
	Other Taxes	-1
	Debt Service	-1
	Home Rule	-1
	Special Levies	-1
	Capital Improvements	-1
	Emergency	-1
	Construction	-1
	Other	-1
Methods of Override	No Approved Overrides	4
	Super Majority Referendum	2
	Majority vote by local representatives	1
	Simple majority referendum	1
	Appeal to Courts	1
	Appeal to State Board	1

For instance, general expenditure limits and a levy limits of 2.5% are both scored equally by Amiel, Deller and Stallman (2009) preventing policy makers from knowing which TEL is most appropriate or impactful. In this regard, weighting and design of the scale can bias results. Without understanding the impact of individual policies, the influence of potential bias remains a black box.

TEL Evaluation Methods

First, the advantage of using panel data is obvious for purposes of making causal claims. The lack of policy change in TELs over time renders most statistical techniques claiming causality inappropriate, and instead can only claim correlation. While discerning the potential impact of TELs does not require the use of panel data method – drawing causal instead of correlative conclusions denotes the advantage Jimenez (2017) achieves over Maher and Deller (2013). The advantage of causal claims is the initial advantage of the current research over much of the prior literature connecting TELs to municipal fiscal health.

The main distinction between the current research and the research of Maher and Deller (2013) and Jimenez (2017) is the separation of the TELs based on the structure of their limits. The testing of a singular form of TEL allows for a more nuanced understanding of policy outcomes. Since I evaluate the individual policy implementation of Minnesota, it enables assessment of direct policy impact from a specific TEL. The policy outcomes determined from this research provide policy actors knowledge as to which TEL implementations will have their intended effects. The ability to provide

information to policy actors is in opposition to the current volume of literature which only notes policy outcomes resulting from aggregate policy packages.

Theory

Property taxes are the most stable source of revenue for municipalities. As a result, municipalities that have greater reliance on property taxes achieve greater fiscal health (Carroll, 2009). Additionally, municipal movement away from property taxes and increased reliance on other sources of municipal funding increases fiscal volatility in certain circumstances and reduces volatility in others (Carroll, 2009; Clair 2012; Johnson, Pagano and Russo, 2000; Jordan and Wagner, 2008). Increased revenue volatility reduces municipal fiscal health (Carroll and Goodman, 2013). While there is reason to believe that municipal taxation limits can stifle revenue choices, increase volatility and fiscal health, it is not clear how (or if) assessment restrictions fit into the broader TEL story.

TELs, if treated broadly, are shown to have direct effects limiting revenue options and shifting municipalities towards new budget funding environments, TEL impositions are also associated with the need to shift budget priorities themselves (Jimenez, 2017). Large infrastructure projects on extended planning schedules replaced with smaller projects that allow for greater budget flexibility and short-term political gains. Similarly, limits on municipal taxing capacity can limit choices available for municipalities to finance new projects (Scharff, 2016). Funding restrictions impact the fiscal status of the municipality and the overall decision-making process of the municipality. Forestalling a large project in the short term may lead to increased municipal resources, but economic development could lag because of eventual infrastructure inadequacy. In turn, the overall decision-making process of the municipalities can have outsized effects on municipal

fiscal health outcomes. Thus, any analysis looking to illuminate the connection between TELs and municipal fiscal outcomes must address potential nuanced changes in spending habits along with revenue sources themselves.

TELs, by definition, impose restrictions on the ability of municipalities to raise tax rates beyond a designated level. TELs prevent municipalities with expanding budgets or decreasing revenues do not have the most stable, risk averse method for municipalities to increase revenues – increasing property tax revenues. When municipalities are unable to increase property taxes to alleviate budget shortfalls, municipalities will either cut budgets, rely on alternative taxes or increase borrowing. These municipal reactions are associated with decreased municipal fiscal health. Cutting budgets reduces fiscal health by reducing slack resources (Hendrick, 2004). Alternative taxes reduce fiscal health through increased volatility (Carroll, 2009). Increased borrowing decreases fiscal health by increasing liabilities and future borrowing rates (Capeci, 1994). Given the empirical backing, the measure employed to express municipal fiscal health should reflect each of these reactions.

Initially, one expects that assessment restrictions will act like other TELs, where municipalities are unable to increase property tax revenues consistent with increases in property values. With the removal of assessment restrictions in Minnesota, municipal revenues could grow in conjunction with property values. Conversely, in the states where assessment restrictions remain, municipal revenues would be similarly restricted, resulting in comparatively lower measures of municipal fiscal health in those states. However, there are several reasons why assessment restrictions are not expected to behave like other TEL implementations.

The first way that assessment restrictions diverge from other TELs is in stringency of restrictiveness. Assessment restrictions do not cap revenues at a specific level. Instead, assessment restrictions allow for some revenue growth. Given that assessment restrictions do not cap revenues, only the rate of growth, a typical reaction of municipalities is to limit the growth of expenses commensurate with the growth of revenues. It is much easier for municipalities to manage minimal growth (e.g., assessment restrictions) than it is to manage wholly capped revenues (e.g., strict levy limits). Therefore, the ability of municipalities to cope with the fiscal ramifications of assessment restrictions is more attainable.

Second, the impact of assessment restrictions are highly dependent on economic conditions. In the case of a contracting economy, assessment restrictions innately have no impact because property values are not increasing. Thus, for assessment restrictions to have an impact on revenue generation, property values must be increasing at a rate that is above the assessment restriction rate. While property values certainly show a growth trend over the long term (at minimum by inflation), the question remains whether property values exceed assessment restrictions to a degree that inhibits municipal fiscal health, an aggregate municipal measure. For example, the impact of the small number of properties in downtown Phoenix abutting Arizona's assessment restrictions on the overall revenues City of Phoenix budget is unclear.

Moreover, assessments are commonly free from restrictions when property is transferred to a new owner. Because property values are a function of increasing sale prices, a number of properties spurring property tax growth are exempt from the restrictions. For example, if a neighborhood experiences property value growth because

of homes sold above previous assessments, property value growth in those sold homes is captured by the municipality without the possibility of running into the restriction limits. Consequently, a significant portion of the driving force behind property assessment increases is exempt from the assessment restrictions.

Lastly, it is highly unlikely that states maliciously choose to implement TELs in such a way that harms municipal fiscal health. Instead, states implementing TELs are often acting to provide a sort of 'tax insurance,' whereby the aim is to guard against extreme shifts in property taxes (Anderson, 2006). As a result, assessment restrictions represent a boundary to property tax growth far outside the norm. If assessments grow a rate under the level determined extreme by the state, assessment restrictions will have no impact on revenues or municipal fiscal health.

As a result, there are two competing sets of hypotheses labeled 1a-b and 2a-b:

> [1a] Measures of municipal fiscal health will be higher in states that remove assessment restrictions compared to states maintaining assessment restrictions.
>
> [1b] Municipal revenues will be higher in states removing assessment restrictions compared to states maintaining assessment restrictions.
>
> [2a] Measures of municipal fiscal health in states with assessment restrictions will remain consistent with states that remove assessment restrictions compared to states maintaining assessment restrictions.

[2b] Municipal revenues in states with assessment restrictions will remain consistent with municipal revenues states removing assessment restrictions compared to states maintaining assessment restrictions.

Data

This study combines three data sources by hand and fuzzy matching for municipalities across all states with assessment restrictions in 2009: Comprehensive Annual Financial Reports (hereinafter "Annual Report"), the Census' American Community Survey and the hand coding of the assessment restriction policy variable. This unique data set allows evaluations of municipal financial indicators and control variables over time, and importantly enables examination of the policy shifts in property tax levy ceilings. The resulting panel dataset covers the years 2009 to 2014 to generate observations at the municipality-year level in the 21 states that had assessment restrictions in place as of 2009.

The first portion of the dataset, generated by GovRank (2017), employs data scraping techniques to retrieve top line financial information from municipal Annual Reports from the webpages of municipalities. Manual requests augmented the data scraping in cases where the Annual Reports were not publicly available by internet. Municipalities defined by the Census Bureau as incorporated and having population above 709 received manual requests for Annual Report information. The cutoff point of 709 residents was chosen because a majority of smaller municipalities do not perform verified audits on their financial information. Municipalities develop Annual Reports on a yearly basis under guidelines provided by Governmental Accounting Standards Board,

Statement 34 (GASB-34). Annual Reports provide financial condition figures such as revenue, expenses, and liabilities for the municipality.

The operating ratio – employed as the outcome of interest signifying fiscal health – is operationalized as total revenues divided by the total expenses of the municipality. Annual Reports provide both components of the operating ratio (total revenues and total expenses). The use of the operating ratio as an indicator of municipal fiscal health is supported by Wang, Dennis, and Tu (2007), Stone, Singla, Comeaux, and Kirschner (2015) and Gorina, Joffe and Maher (2018). Low operating ratios indicate that municipalities are operating close to or with a yearly deficit. Operating deficits or lower operating ratios are associated, but not the only explainer of municipal fiscal health and potential default in municipalities (Stone, Singla, Comeaux and Kirschner, 2015).

Operating ratio is widely employed as a fiscal health indicator because of the immediate ties to budget solvency and is an indicator of fiscal distress (Gorina, Joffe and Maher, 2018; Wang Dennis and Tu, 2007). Additionally, it is consistent with Jimenez's (2018) rationale for employing unrestricted net position. Jimenez employs unrestricted net position capture whether municipalities are raising revenues to cover expenses in the current period, whether municipalities are relying on past savings, whether municipalities are funding pensions, and because it mirrors financial reserve holdings (Jimenez, 2018). Effectively, the aim of fiscal health measures is to determine whether the fiscal position of a municipality has worsened in the prior year (Jimenez, 2018).

However, the strict employment of net asset ratio can be interpreted as problematic due to municipal slack resource holding tendencies. The net asset ratio may be highly subject to the political tendencies described by Marlowe (2005; 2011) on slack

resources. As such, drops in net asset ratio may be intentioned corrections by political actors. If the net asset ratio is employed it may harbor volatility resulting from political capture. For instance, if a municipality has large unrestricted net asset funds one year and moves them to restricted funds the next, the net asset ratio may reflect a negative impact on fiscal health when the actual effect is null.

Operating ratio achieves many of the same goals more directly and transparently than the unrestricted net position. Since the operating ratio provides revenues over expenses, it directly measures whether revenues are covering necessary outlays for the budget period. Provided municipalities are not taking on debt with balloon payments, increases in debt would be reflected in yearly expenses. Additionally, operating ratios consistently above 1.0 would mirror growth in slack resource funds. The only foreseen shortcoming of the operating ratio is the inability to capture pension funding. The inability to capture pension funding is not expected to be endemic in the short-run financial outcomes seen in this research.

The second source of data is the 5-year estimates from the U.S. Census Bureau's American Community Survey (ACS) which provides key demographic controls for municipalities. Use of 5-year estimates is because of near complete coverage across all six years of the panel and most municipalities. One and 3-year ACS estimates provided limited coverage to smaller municipalities and would untenably reduce the sample in the synthetic model state of interest, Minnesota. The 5-year estimates, because they are averaged estimates, lack some of the granularity of the 1-year or 3-year estimates, but still adequately capture trends such as population and income as they are relevant for municipal finance.

The variables employed from the ACS controls were: total population of the municipality, percent of population in public school, percent of the population that is African-American, percent of the population that is Hispanic, median income, percent of the public with bachelor's degrees or more, median age, percent of the public receiving retirement income, unemployment, and housing value. [Descriptive statistics provided in Table 3]. The choice of control variables is because of perceived connections with the fiscal health of the municipality, and initial choices were in favor of inclusion over preclusion.

Table 3 – Descriptive Statistics Comparison: State Counterfactuals

	Comparison States – N=11,630			**Restricted Comparison States – N=4,368**			**Trend Matched States – N=3859**			**Minnesota – N=1,180**		
Variable	Mean (SD)	Min	Max	Mean (SD)	Min	Max	Mean (SD)	Min	Max	Mean (SD)	Min	Max
Operating Ratio	1.16 (.438)	.042	22.50	1.09 (0.416)	.042	17.67	1.11 (0.283)	0.294	8.67	1.12 (0.219)	0.632	4.33
Total Revenues	7.14e7 (5.76e8)	9,873	2.57e10	5.34e7 (8.40e8)	39,452	2.57e10	5.30e8 (1.58e8)	168,680	2.61e9	2.62e7 (7.61e7)	966177	1.01e9
Total Population	25,632 (57,763)	36	986,320	19,632 (47,513)	36	986,320	24,040 (59,165)	36	837,533	13,742 (32,244)	191	394,424
% in Public K-12	17.33 (5.35)	0	43.02	17.33 (4.86)	0	40.33	15.90 (5.43)	0	40.33	17.21 (3.86)	0.048	0.318
% Black	9.83 (15.72)	0	96.05	12.13 (19.15)	0	96.05	7.42 (13.26)	0	91.68	2.27 (4.001)	0	29.19
% Hispanic	17.46 (20.46)	0	100	7.72 (10.16)	0	78.58	13.19 (17.11)	0	97.72	3.95 (5.006)	0	38.89
Median Income	53,862 (28,425)	11,354	250,001	50,484 (26,341)	11,354	250,001	52,613 (22,024)	14,861	250,001	57,153 (21,650)	23,542	148,235
% with bachelor's Degree	16.13 (9.45)	0	60.51	14.94 (8.54)	0	48.54	17.58 (9.40)	0	60.51	18.64 (8.539)	1.86	45.99
% with Retirement Income	18.02 (6.87)	0	55.58	19.04 (6.22)	0	50.00	17.66 (7.71)	0	55.58	15.78 (5.456)	1.54	38.86
% Unemployed	9.14 (4.76)	0	38.46	9.98 (5.05)	0	37.12	8.06 (4.48)	0	38.46	3.25 (3.26)	0	38.32
Median House Value	224,723 (202,202)	18,800	1,000,001	210,344 (195,414)	18,800	1,000,001	203,109 148,128	18,800	1,000,0001	96,713 (96,713)	34,600	651,700

Note: Some values, such as median house values have consistent max numbers due to census reporting ceilings

For the third portion of the dataset, I code the two independent variables as dummy variables using information from the Lincoln Land Institute's 'Significant Features of the Property Tax,' along with the legislative background of Minnesota's assessment restrictions. The first variable, all municipality years in Minnesota as a one, the treatment state. The second variable, coded as one in municipality-years after the change in policy in Minnesota, those ranging from 2012 through 2014. All years in all

other municipality-years receive a score of zero to fulfill the requirements of a DiD design.

To obtain the combined dataset, I employ fuzzy matching (the merging of data sets based on approximal matches) based on state, municipality name, and population to combine the ACS and GovRank datasets. The fuzzy matching was hand audited for any cases that were unclear (e.g. hypothetical match of Springfield Village to Springfield City and populations that are close but not exact). If the match remained unclear after the hand audit I excluded the observation excluded from the dataset. The matching process provided a dataset consisting of 30,726 municipality-years. Further limiting the dataset was the restriction to municipalities with operating ratio across the full panel of 2009-2014, done to provide better estimates in the synthetic control model. In total, 2,135 municipalities (12,810 municipality-years) remain in the dataset out of a potential 6,194.[5]

The change in Minnesota's policy is an appropriate application of differences-in-differences (hereinafter "DiD") model, a policy change in one geography with a distinct pre and post period, and the ability to compare to numerous other municipalities without policy changes in the same period. As such, the treatment group consists of municipalities within Minnesota, while the potential pool of comparison municipalities being those which have assessment restrictions during the entirety of the evaluation period. The initial comparison group with assessment limits imposed consists of municipalities in: Arizona, Arkansas, California, Colorado, Connecticut, Florida,

[5] The large drop is a result of a large quantity of municipalities missing top line financial data between 2009 and 2011, effectively rendering their value null for inference over the period in question - whether employing the synthetic control or a standard DiD counterfactual.

Georgia, Illinois, Iowa, Maryland, Michigan, Montana, New Mexico, New York, Oklahoma, Oregon, South Carolina, and Texas.

The last restriction to the data is due to the use of a synthetic control model (explained later), which effectively limits the sample to only those municipalities with a fully balanced panel of observations. This last restriction is particularly compelling since many smaller municipalities were in the process of adopting GASB-34 standards, releasing and retaining access to Annual Reports at the outset of the study, limiting the balanced panel to 87 municipalities in Minnesota and 1,322 potential control municipalities in control states.

Methods

In this study, I employ a synthetic control design to construct an effective counterfactual. Although a simple DiD design is preferred for simplicity and clarity, the inability to fulfill parallel trends assumptions renders causal inference inconclusive. To begin I explain the development of the general model applicable to both the standard and synthetic control designs. I further explain the trends fulfillment and the benefits the synthetic model provides in this instance.

The change of policy in Minnesota requires a reversal of the typical DiD model in terms of treatment language because the treatment involves the removal of the assessment restriction policy; however, this change provides no substantive changes to the requirements of causal inference. The result is the rearrangement of a typical DiD to function as equation 1, but otherwise functions with the same statistical assumptions and capacity for causal inference. Because the regression effectively removes both Minnesota

effects and post-treatment year effects, estimations of the DiD coefficient of interest constitutes the average treatment effect.

Although the average treatment effect in the subject equation describes the effect of a rescinded policy, the variables in Equation 1 are coded typical of a standard difference in difference model. The Minnesota variable is coded as a 1 in all municipalities within Minnesota, to capturing latent effects of being in Minnesota. The Post Treatment variable is coded 1 in for all municipalities the years 2012-2014 to capture the latent effects of those years. As a result, the combined variable of Minnesota and Post Treatment is coded only as 1 during the years Minnesota does not have the assessment restriction in place and the municipality is in Minnesota. As such, the coefficients in Equation 1 can be interpreted as follows: β_1 is the coefficient for the value of the period post-reform, which for the untreated municipalities means continued assessment restrictions; β_2 is the coefficient for the value which is unique to the municipalities in Minnesota across all periods in the regression; β_3 is the coefficient of interest, which is the difference between Minnesota municipalities and the counterfactual during the pretreatment period and the value of Minnesota municipalities to the rest of the control municipalities during the post-treatment period; β_4 is the coefficient for the value of the controls employed in the regression intended to mitigate any potential unforeseen differentiation between Minnesota and the control groups generating omitted variable bias. The same right hand side of the equation is employed separately against the two outcomes discussed previously: municipal operating ratios and total revenues.

$$[1]\ (\text{Outcome})_{it}=\beta_1+\beta_2(\text{Minnesota})_{it}+\beta_3(\text{Post Treatment})_i+\beta_4(\text{Minnesota} * \text{Post-Treatment})_i+X'_{it}+\mathcal{E}_{it}$$

To test the fulfillment of the parallel trends assumptions, I employ three counterfactual groups: 1) the full complement of states using assessment limitations (Figures 1a and 1b); 2) a restricted sample consisting of Connecticut, Georgia, Iowa, Maryland, Michigan, New York, Oregon, and South Dakota (Figures 2a and 2b); and 3) a hand selection of states chosen expressly to fulfill the parallel trend (Figures 3a and 3b). The selection of the restricted sample was based on matches to the state TEL environment and the economic environment of Minnesota. For example, Massachusetts' large cities known for high education levels hew closely to Minnesota economically; however, the TEL environment of Massachusetts is so dissimilar (with rate caps and circumvention through special districts) that it is not an appropriate match. As an opposing example, Georgia is not geographically proximal to Minnesota; however, the economic drivers and TEL environment were closely aligned enough to warrant inclusion. To provide a cross-check to the trends assumption on the operating ratio that are provided in Figures 1a and 2a, Figures 1b and 2b provide trends of municipal revenues. Further, even where counterfactuals fulfill parallel trend assumptions, Jaeger, Kaestner, and Joyce (2018) raise concerns about match appropriateness when outcome levels are not consistent between subject and counterfactual groups.

Figure 1 (a-b) Assessment Restriction States and Restricted Sample (Maps)

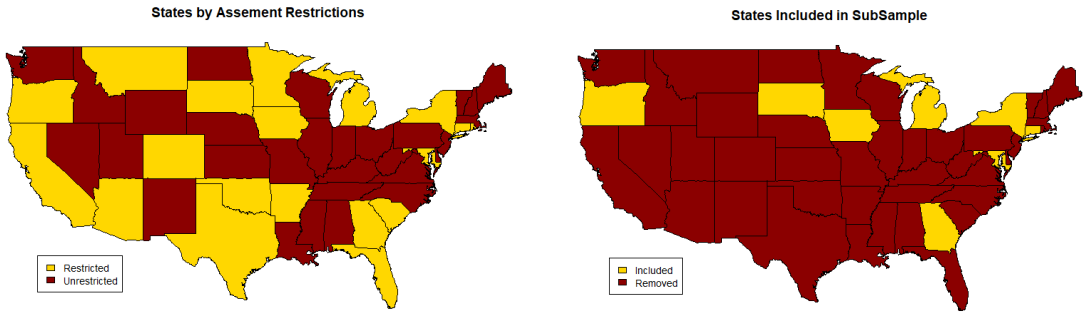


Figure 2(a-b) – Time series of parallel trends for all states with assessment restrictions against Minnesota

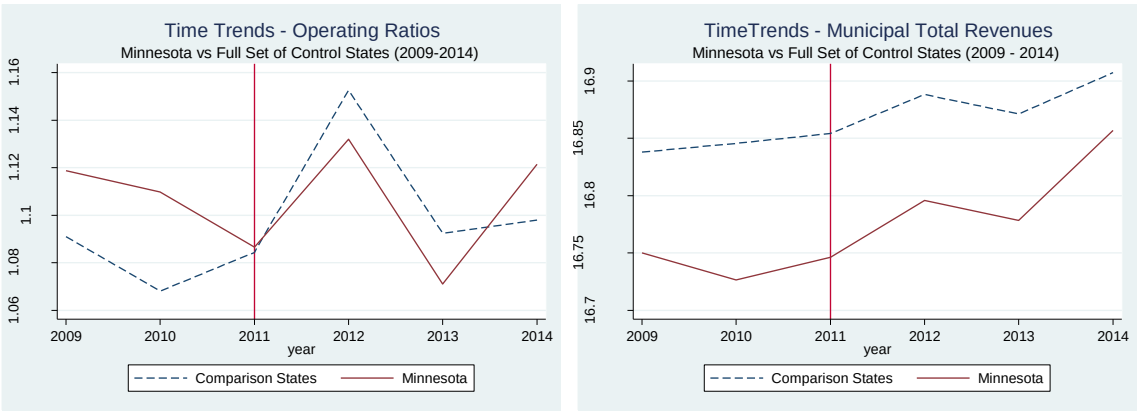
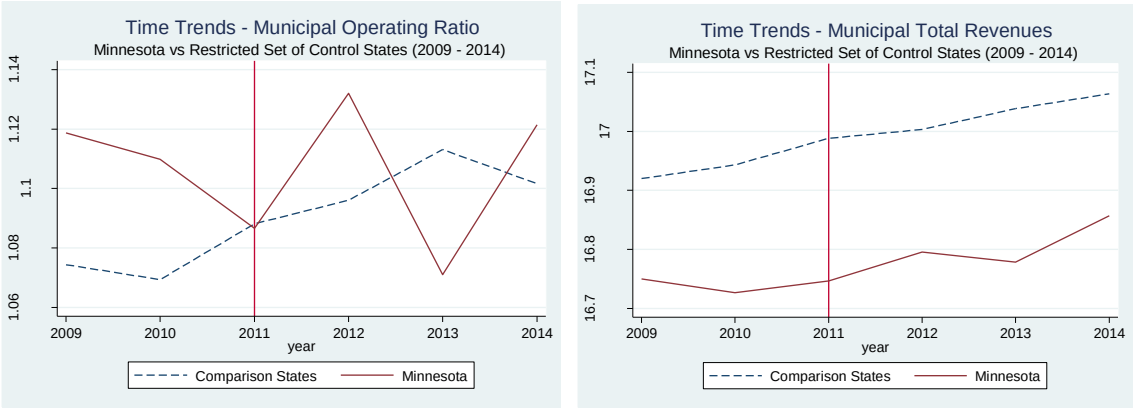
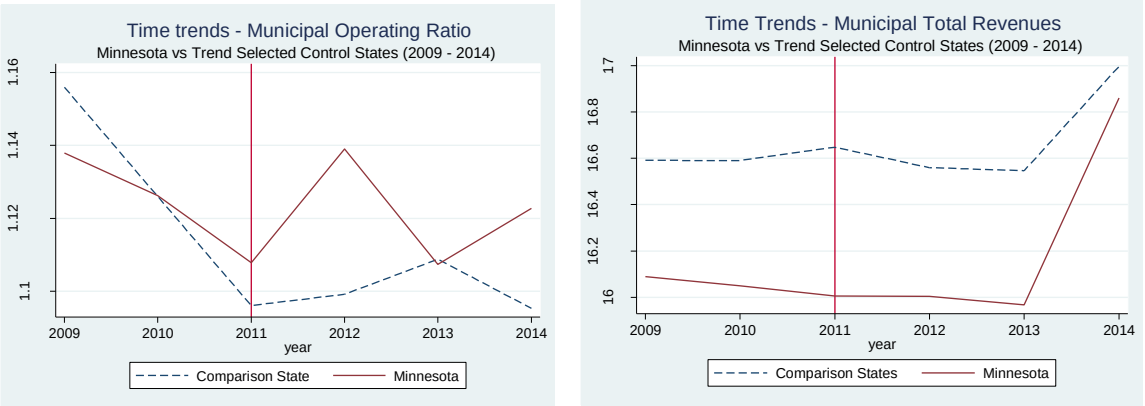


Figure 3 (a-b) – Coefficient plot of parallel trends assumption for limited set of assessment restricted states w/o controls



Because the parallel trends assumption is still unfulfilled, a tertiary selection process was also conducted choosing only states that expressly fulfill the parallel trends assumption. In this case the sample pool for counterfactual states consists of Arizona, Arkansas, Colorado, Florida, Iowa, South Dakota, and Virginia (Time trends provided in figures 4a and 4b). In this case, the parallel trends assumption is fulfilled for both operating ratio and revenue outcomes. However, the choice of states based on fulfillment of parallel trends has the potential to introduce unintended selection bias. This is especially the case because the selected states do not appear to meet alternative criteria for selection. As result, the synthetic control model is appropriate to fill those both the parallel trends assumption and automate selection criteria.

Figure 4 (a-b) – Coefficient plot of parallel trends assumption for limited set of assessment restricted states w/o controls



First, the method automates generation of the synthetic control from a donor pool and restricts the ability of the researcher to choose a favorable or unfavorable comparison group (Abadie, Diamond and Hainmuller, 2010). While researchers refrain from a biased choice in counterfactual group, completely ethical researchers are nonetheless prone to making subconscious choices impacting outcomes. Using the synthetic control method,

the mathematically generated control group eliminates potential researcher bias in counterfactual choice.

Second, the use of synthetic control matching reduces the likelihood of misinterpretation of exogenous factors on individual control groups as policy outcomes (Abadie, Diamond and Hainmuller, 2010). In a single case control method, unobserved exogenous influence on the sole control would bias results. By using a weighted sum of municipalities from the donor pool it restricts the potential exogenous impact of a single control to the weight of the specific pool contributor. For example, if a control municipality in typical case-control method had an unobserved economic shock, the change in municipal operating ratios would contribute to the causal association to the variable of interest. By amalgamating municipalities from the pool into a counterfactual, it strengthens the likelihood of fulfillment of the stable unit treatment value assumption.

Equation 3 provides a simplified form of the estimated treatment effect under a synthetic control where $\hat{\alpha}$ is the average treatment effect of the outcome of interest. The value of the Y_{1t} represents the treated municipality in Minnesota. The weighted average of the donor units is $\sum w_j Y_{jt}$, where w is the weight used in the pre-period to satisfy the parallel trends assumption of the DiD. By using within year comparisons at period $_t$, for treated and then untreated periods, estimates account for year fixed effects, and estimate yearly effects in post periods. Although the estimation provides some statistical ambiguity for the long-term treatment effects, it reduces concerns related to proper specification of non-linear policy outcomes such as diminishing or increasing returns typical of averaged DiD models. (See Abadie, 2005 or Abadie, Drukker, and Herr, 2004 for other non and semi-parametric alternatives)

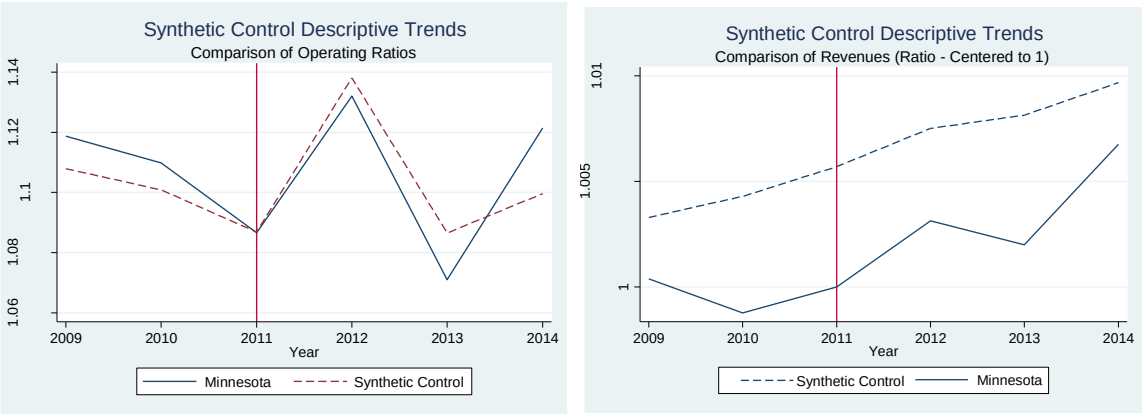
[3] $\hat{\alpha}_{1t} = Y_{1t} - \sum_{j \geq 2} w_j Y_{jt}$

Above the regular synthetic control, there are two reasons for employing the multiple synthetic control design developed by Cavallo, Galiani, Noy and Pantano (2010). First, aggregated data prohibits localized variation. Multiple variable sized municipalities within a state requires localized variation to avoid ecological fallacies – where events that have outcomes of interest at the municipal level could create either type 1 or type 2 errors if evaluated in average across the state. For example, if one large municipality experienced a large shift in operating ratio, in aggregate it could very well counteract numerous smaller municipalities experiencing a shift in the opposite direction. Second, unlike the use of synthetic controls for aggregated, or individualized data (Ross, 2017), the use of individual unit analysis allows for the development of pseudo P-values through placebo tests (Cavallo, Galiani, Noy and Pantano, 2010). The outcomes of the developed counterfactual balance against the outcomes of other potential counterfactuals to determine the degree to which the treatment group is statistically unique. As a result, unlike a single synthetic design, a multiple synthetic control matching technique for the DiD allows for determination of both effect size and statistical significance.

Results

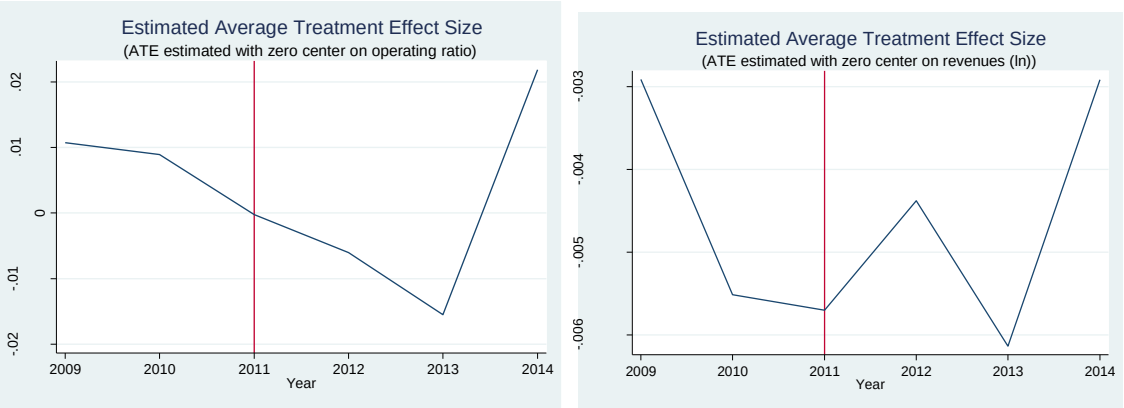
In this section, I estimate the average treatment effects from removing assessment restrictions in Minnesota against the counterfactual.[6] The results of the synthetic control model in time trend format are presented in Figures 5a and 5b. In the years 2009-2011, the graphs provide the match of parallel trends, divergence from 2012-2014 is the treated against the untreated effect developed from the synthetic counterfactual. Figures 6a and 6b simplify the treatment effects that are achieved by subtracting the treatment and nontreatment effects per equation 3 (or the trends in figures 5a and 5b). Both operating ratios and total revenues in Minnesota municipalities prior to the release from assessment restrictions fill the parallel trends assumptions required of the DiD design. While the break in 2012 between Minnesota and the counterfactuals on both outcomes portrays causal differentiation, there is no clear trend between the lifting of assessment restrictions in Minnesota against the synthetic counterfactual.

Figure 5(a-b) – Parallel trends of synthetic generated based on dependent variable trends



[6] Results from the standard DiD design are provided in Tables 4 and 5. As noted in the methods section, the results from the standard DiD estimations are flawed because the parallel trends assumption is unfulfilled. Disregarding the failure to meet the parallel trend assumptions, the results show consistency with results from the synthetic control model.

Figure 6(a-b) – Estimated Average Treatment Effects matched on dependent variable



The coefficients for the operating ratio noted in Table 6 provide that the operating ratio drops by 0.06 and 0.012 in 2012 and 2013, but returns with a growth in operating ratio of 0. 021 in the year 2014. The changes in operating ratio indicate a small, but noticeable drop after release from restrictions and a recovery in full by 2014. The down and up changes are certainly not confirmation of an express trendline indicating support for a relationship between operating ratios and the change of policy in Minnesota in 2011.

Table 4 – 6 Synthetic control estimates of operating ratio matched on dependent variable

	Coefficients	Direct P-Values	P -values – Pseudo T-Stats
2012	-0.006	0.951	0.994
2013	-0.012	0.486	0.575
2014	0.021	0.379	0.591

Coefficients are in terms of operating ratios

Table 5 – 7 Synthetic control estimates of revenue matched on dependent variable

	Coefficients	Direct P-Values	P -values – Pseudo T-Stats
2012	0.046	0.233	0.117
2013	0.004	0.922	0.510
2014	0.035	0.456	0.303

Coefficients are in terms of relative revenue ratios, 2011 position is treated as 1

Table 6 – 8 Synthetic Control estimates of operating ratio based on revenue trends

	Coefficients	Direct P-Values	P -values – Pseudo T-Stats
2012	0.025	0.243	0.072
2013	-0.067	0.029	0.077
2014	-.026	0.305	0.630

Coefficients are in terms of operating ratios

Table 7 – 9 Synthetic control estimates of operating ratio based on expenditure trends

	Coefficients	Direct P-Values	P -values – Pseudo T-Stats
2012	0.012	0.571	0.536
2013	-0.047	0.031	0.017
2014	-0.014	0.573	0.567

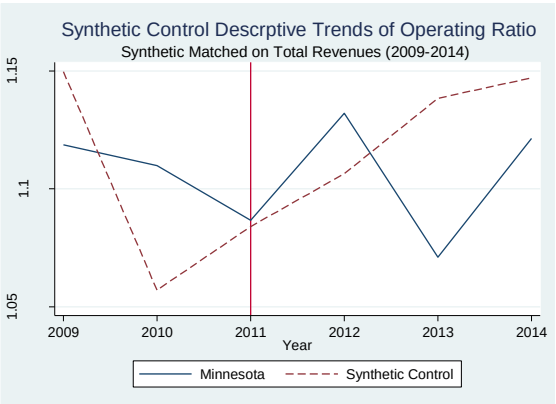
Coefficients are in terms of relative revenue ratios, 2011 position is treated as 1

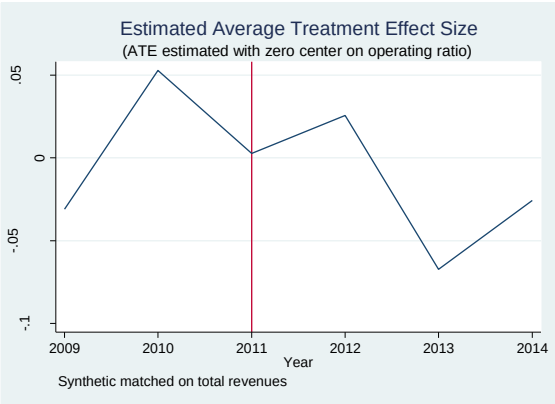
In contrast, the coefficients for revenues, provided in Table 7 provide an increase across all post-assessment restriction years, a slightly clearer image. Since they are centered on one at the last pretreatment point, the coefficients from synthetic model provides a percent point increase in revenues of 0.046 in 2012, 0.004 in 2013, and 0.035 in 2014. Although the results are consistently positive, coefficients still do not show a discernable trend. Once again, this provides a lack of support for the relationship between the change in policy in Minnesota and municipal revenues.

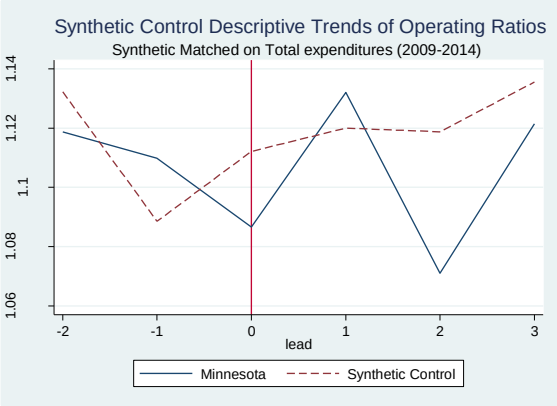
Problematic in both specifications is a complete lack of statistical significance in the p-values provided in Tables 6 and 7. The p-values for the operating ratio holds at 0.994, 0.575, and 0.591 during years 2011, 2012 and 2013. Providing an analogous picture p-values for total revenues are at 0.233, 0.922, and 0.456 in the years 2012, 2013, and 2014. Few of these values even approach statistical significance, which indicates no association exists. The same issue arises when pseudo p-values are employed. The pseudo p-values are developed weighting standard p-values against a measure of fit between the pre-treatment trends and the counterfactual pool.

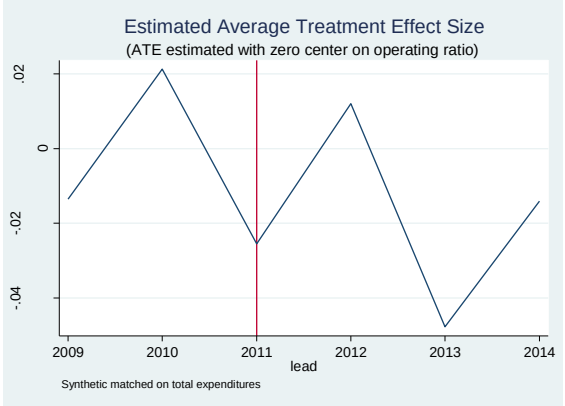
To allay concerns of overfitting on the dependent variable (Klößner, Kaul, Pfeifer, and Schieler, 2017), cross-validation is employed to as a robustness check on outcome consistency. To cross check, I cross specify the synthetic fitting variable as the alternative outcome measure (e.g., fit the synthetic control for the revenue outcome with an operating ratio match). In addition to providing a cross-check on the initial synthetic control results, the alternate specification provides a check on potential endogeneity. The potential endogeneity exists from a potentially recursive relationship in the operating ratio due to municipal expenditure responses. The results of the alternate specifications are provided in Tables 7 and 8, and illustrated in Figures 7(a-d). The lack of substantive significance using the covariate based synthetic controls developed from alternative outcomes reinforces that the lack of association found through the first synthetic control development is correct. Because the cross-specifications did not result in divergent results, it is not expected that strong endogeneity exists in the initial specifications.

Figure 7(a-d) Alternative matching for synthetic controls









All results from the synthetic model and the DiD model provide a consistently statistically insignificant relationship between either municipal revenues or operating ratios and the policy change in Minnesota in 2011. This provides evidence that there is a null relationship, supporting the theory that municipalities will not see strong revenue or fiscal health outcomes from assessment restrictions. The consistency in results across both analytical techniques further suggests that the lack of significant outcomes is not a result of the specific technique discussed here. Given null results across both models and multiple robustness checks, one is inclined to accept hypotheses 2a and 2b in favor of hypotheses 1a and 1b.

Discussion

The consistency in null results across several iterations of the model implies that there is simply not a relationship between the 2012 removal of the assessment restriction in Minnesota and a change in the operating ratio, or even total revenues, of subject municipalities in relation to comparable municipalities. The lack of substantiation in the relationship of interest indicates the relationship established in aggregate research on TELs does not hold for the assessment restrictions that previously enacted in Minnesota.

A potential explanation for these findings is that implementations of assessment restrictions minimize fiscal harm to municipalities, but nonetheless provides insurance against extreme property value growth, remaining consistent with Anderson (2006). Under this explanation, the placement of the assessment restrictions in Minnesota limit property tax only in particularly extreme cases, far beyond the growth seen coming out of the Great Recession during 2012-2014. As such, policy actors can claim a mantle of tax abatement, while simultaneously committing no substantive action on municipal budgets. Thus, the conclusion is that the policy of assessment restrictions as in Minnesota up until 2012 allows political adherence to tax constraint while providing no change in revenue outcomes – serves effectively as a "fiscal Potemkin village."

A second explanation is that municipal revenue offices are adjusting to revenue increases from full assessment as they occur. As a result, the municipalities may have fore-planned by adjusting levy rates in advance to accommodate the increase in assessments. The practice of limiting excess revenue to limit excessive slack resources would be consistent with Marlowe (2011) and the political untenability of large unrestricted fund holdings.

The most important implication of this research is in the way that the field evaluates TELs going forward. For decades, the field quantified TELs as a monolith, treating levy limits as though they have the same outcomes on a municipality as a limit on assessments. This current study provides a more nuanced story, suggesting that some TEL implementations not only have less impact than others, but may have null effects in certain situations. The foundational work by Ameil, Deller, and Stallman (2009) on TELs indicated that further testing was needed in order to confirm their assumptions; the

current study is the first to test those critical assumptions. Results showing the implications of individual TELs extend the value of TELs indices by providing backing to the underlying scoring on the TELs index.

Beyond the empirical implications, this research reiterates the need to provide relevance to policy actors. Policy actors looking towards TEL implementations may not see a complete, nuanced picture from previous TELs literature. For instance, if policy actors derived any information from prior research about the impact on fiscal health from removing assessment restrictions in Minnesota, it would be positive – while these findings show no impact. Even within this research shortcomings of nuance exist because only one level of assessment restrictions are evaluated. It should be apparent that researchers develop nuance in their estimation of TEL impacts. Discrimination by program appears necessary if we are to arrive at accurate conclusions.

Table 8 – Results of Standard Difference in Difference Models – First Restriction States

Dependent Variable:	Operating Ratio		Operating Ratio		Total Revenues (Ln)		Total Revenues (Ln)	
	β (se)	β (se)	β (se)	β (se)	β (se)	β (se)	β (se)	β (se)
Minnesota	0.013 (0.011)	-0.086 (0.506)	0.027** (0.011)	0.022 (0.018)	-0.320*** (0.055)	0.083* (0.043)	0.099 (0.061)	0.137*** (0.046)
Post Treatment	-0.239 (0.236)	0.370* (0.224)	0.002 (0.012)	0.009 (0.012)	0.092** (0.032)	0.076*** (0.019)	0.251*** (0.050)	0.129*** (0.036)
Minnesota * Post Treatment	-0.210 (0.231)	-0.267 (0.714)	-0.002 (0.018)	-0.007 (0.025)	-0.009 (0.082)	0.014 (0.060)	-0.167* (0.091)	-0.065 (0.060)
Total Population (Ln)		-0.020 (0.078)		-0.021*** (0.004)		0.838*** (0.007)		0.819*** (0.017)
% in Public K-12		1.202 (2.471)		0.090 (0.125)		-2.194*** (0.208)		-2.956*** (0.492)
% Black		-0.008 (0.008)		0.001* (0.000)		0.003*** (0.001)		0.003*** (0.001)
% Hispanic		-0.006 (0.007)		0.001 (0.001)		0.002*** (0.001)		0.008*** (0.002)
Median Income (ln)		-1.505*** (0.487)		-0.037 (0.025)		-0.452*** (0.041)		-0.127 (0.081)
% with Bachelor's Degree		0.013 (0.020)		0.003*** (0.001)		0.010*** (0.002)		0.008** (0.003)
% with Retirement Income		-0.005 (0.018)		-0.002** (0.001)		-0.002* (0.001)		0.007** (0.003)
% Unemployed		-0.037 (0.027)		-0.001 (0.001)		-0.018*** (0.002)		-0.026*** (0.005)
Median House Value (Ln)		0.494* (0.264)		-0.000 (0.015)		0.173*** (0.022)		-0.155*** (0.056)
Constant	1.111*** (0.007)	11.718*** (4.171)	1.096*** (0.007)	1.646*** (0.215)	16.367*** (0.022)	12.024*** (0.351)	15.947*** (0.034)	12.572*** (0.616)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Sample	All Asmt Restricted States		Ltd Set of Restricted States		All Asmt Restricted States		Ltd Set of Restricted States	
N	12812		5535		12812		5535	
Adj, R-Square	0.001		0.007		0.671		0.560	
* p<0.10, ** p<0.05, *** p<0.01								

Table 9 – Results of Standard Difference in Difference Models – Trend Selected States

Dependent Variable:	Operating Ratio		Operating Ratio		Total Revenues (Ln)		Total Revenues (Ln)	
	β (se)	β (se)	β (se)	β (se)	β (se)	β (se)	β (se)	β (se)
Minnesota	0.013 (0.011)	-0.086 (0.506)	-0.024* (0.011)	-0.015 (0.011)	-0.320*** (0.055)	0.083* (0.043)	0.060 (0.061)	-0.012*** (0.040)
Post Treatment	-0.239 (0.236)	0.370* (0.224)	-0.003 (0.013)	-0.036* (0.015)	0.092** (0.032)	0.076*** (0.019)	-0.563*** (0.066)	-0.337*** (0.051)
Minnesota * Post Treatment	-0.210 (0.231)	-0.267 (0.714)	0.024 (0.017)	0.021 (0.020)	-0.009 (0.082)	0.014 (0.060)	0.023 (0.097)	0.083 (0.060)
Total Population (Ln)		-0.020 (0.078)		-0.011*** (0.003)		0.838*** (0.007)		0.775*** (0.014)
% in Public K-12		1.202 (2.471)		0.228* (0.110)		-2.194*** (0.208)		-3.133*** (0.502)
% Black		-0.008 (0.008)		-0.001* (0.000)		0.003*** (0.001)		-0.008*** (0.001)
% Hispanic		-0.006 (0.007)		-0.001* (0.000)		0.002*** (0.001)		-0.004** (0.002)
Median Income (ln)		-1.505*** (0.487)		-0.070 (0.024)		-0.452*** (0.041)		-0.415*** (0.104)
% with Bachelor's Degree		0.013 (0.020)		0.001 (0.001)		0.010*** (0.002)		0.024*** (0.003)
% with Retirement Income		-0.005 (0.018)		-0.002** (0.001)		-0.002* (0.001)		0.015** (0.003)
% Unemployed		-0.037 (0.027)		-0.004** (0.001)		-0.018*** (0.002)		-0.010 (0.007)
Median House Value (Ln)		0.494* (0.264)		-0.012 (0.015)		0.173*** (0.022)		-0.310*** (0.068)
Constant	1.111*** (0.007)	11.718*** (4.171)	1.126*** (0.010)	1.877*** (0.212)	16.367*** (0.022)	12.024*** (0.351)	16.610*** (0.042)	18.451*** (0.836)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Sample	All Asmt Restrict. States		Trend Selected States		All Asmt Rest. States		Trend Selected States	
N	12812		3859		12812		5535	
Adj, R-Square	0.001		0.018		0.671		0.655	
* p<0.10, ** p<0.05, *** p<0.01								

3: DIRECT DEMOCRACY AND TAX AND EXPENDITURE LIMITATIONS

The Land Tax Where Preemption Meets Preference

Introduction

To the public, preemption only exists when governments disagree. While preemption exists at any point that a government restricts the policy choices of a subordinate government, it rarely garners public attention unless governments disagree. Because politics explains preemption more often than institutionalism (Fowler and Witt, 2019), examples of political disagreement in preemption are countless. From Republican states prohibiting the plastic bag bans of Democratic cities (A.R.S. 11-269-14), to Democratic states restricting Republican localities from enacting right to work legislation (AP, 2019), preemption takes many policy forms. In many senses, political disagreement generates press – for preemption, I argue it also generates effect.

Tax and Expenditure Limitations (TELs) in the context of this paper are state-created limitations on the budgetary options of municipalities – classical preemption in an institutional sense. Per Goodman, Hatch, and McDonald (2020), tax and expenditure limitation laws fit squarely into the first era of preemption. Further, Goodman, Hatch, and McDonald (2020), following Mullins (2004) portray TELs as emerging from a small group of reformers – effectively foisted upon the state and its subordinate local governments, often hampering their ability to govern as they please. As such, TELs fill the formal minimum of preemption, a policy restricting the choice set of a legally subservient government.

Preemption does not have the same impact across all local governments within a state. Although preemption may be created with language that is impartial to locality – as all local governments are subject to the same set of rules. The impact of those regulations

are not borne equally. For example, a small town with a plastic bag manufacturer may perceive a ban on plastic bag restrictions as economic protection. Heterogeneous impact becomes particularly important when looking at policy impacts. A ban on local facemask regulations is not likely to change the policy landscape in a town broadly and vehemently opposed to facemasks. Similarly, TELs are unlikely to change the landscape of a town opposed to tax increases. The latent assumption that treatment homogeneity generates homogeneous outcomes has pervaded a wide swatch of existent tax and expenditure limit literature.

Further, local government responses to tax and expenditure limitations have been discussed primarily in administrative terms. While many decisions related to the financial management of local governments rest in the hands of professionalized non-political managers; the lack of influence by politics is often logical, one does not expect local politics to directly influence lower level managerial decisions. For example, fire fighter training routines are far more likely a managerial decision and are unaffected by the politics of the local constituency. However, I argue that tax and revenue preferences are antipodal to these non-political decisions.

With more polemical decisions such as taxes, the line between politics and neutral administration in public finance is increasingly understood as fuzzy (Jimenez, 2020; Tausanovich and Warshaw 2014). The long-held belief that all government actors attempt to increase their own budget revenues for the intent of job security (Wildavsky, 1964; 1988; Niskanen, 1971) may be falling by the wayside (Arapis and Bowling, 2020; Ryu, Bowling, and Cho, 2007). Instead, literature increasing intercedes that the same

mechanisms that serve to politicize city managers are the same ones that dictate local tax policy (Svara, 2016).

This article aims to provide evidence that not all local governments are equally impacted by state preemption and that municipalities tend to follow the political will of their constituencies in developing taxation levels. That political/taxation preferences of local governments are central to defining outcomes from tax and expenditure limitations, and one homogeneous treatment can generate large or null impacts depending on the alignment between the policy and the municipal trajectory. The paper continues as follows: Section 2 provides an outline of previous research and theoretical perspectives and extends to the theoretical perspective added by this article; Section 3 provides background on the Colorado's tax and expenditure limit, TABOR; Section 4 and 5 provide description of methods and robust checks used to test the theory; Section 6 provide the results of those tests; and Section 7 provides conclusions and a discussion resulting from those tests.

Prior Research and Theory

Typically, the impacts from TEL implementations are described through two theoretical lenses. The first lens is that the fiscal rules provide a necessary constraint on municipal actors. The constituency of the municipality uses a TEL to restrain elected local government actors from increasing taxes at a rate that is not desired by the constituents. Indeed, the majority of TELs research has focused on the theory of broad constraint on local government actors. If the TEL is not enacted, both unelected powers (typical PA actors) and elected officials will increase taxes based on their own beliefs and

to their own benefit. The counterfactual in the first theoretical lens assumes that the municipal actors were unconstrained by constituent preferences before TELs and would have been unconstrained in the absence of the TEL.

Within the perspective that TELs provide a constraint on municipal taxes, without which taxes will inevitably increase – owing to the theory presented by Wildavsky (1964) that a government will inevitably grow itself without outside constraint. Shedbegian (1998) provides that total revenues, property tax revenues, and total expenditures are effectively restrained by TELs; Plummer and Pavur (2009) show a decrease in both tax and non-tax revenues as a result of TELs adoption; Chapman and Gorina (2012) seeing potential endogenous processes employ a two-stage design and still observe a negative impact on own source revenues. Over time, the first lens begins to lose prominence and succinctness as more and more caveats were determined, such as Heone's (2004) determination that municipal charges and fees increase after TEL implementation; and Sun's (2014) perspective that own source revenues actually increase over time, even with property tax restrictions.

The second lens is one of institutional irrelevance, where the constraints are only designed to limit municipal actors in certain particular envelopes. Outside of the restrictive envelope, municipal actors make up for lost revenue in a host of ways. The institutional irrelevance perspective is illuminated by municipal responses in Massachusetts after the passage of the TELs package as part of Proposition 2½. (Carr and Farmer, 2011; Goodman and Leland, 2019). Although the state level restrictions limited revenue generation by municipalities, a de facto exception allows municipalities to generate revenue by the creation of special districts. Through the generation of special

districts, municipalities in Massachusetts subvert the TEL restrictions through new taxation under a special district. Effectively, the institutional irrelevance perspective provides that municipalities looking for new revenue are limited only in mechanisms, not levels of revenue production.

I offer a third lens. Introduced in part by Eliason and Lutz (2018), this view posits that the expected revenue changes resulting from TELs introduction rest on the tax preferences of the municipality itself. The third perspective rests on a simple proposition: if the TEL is consistent with the tax preferences of the subject municipality, there will be no impact on revenues, expenditures, or service delivery. Given that a constituency presents its views (in this case, via a TEL) that all tax increases are undesired, it is unlikely that a tax increase would occur without the TEL.

However, TELs are commonly enacted at the state level and all municipalities are subject to the restrictions do not have the same tax preferences. As a result, municipalities are bound to the preferences of their states, regardless of their individual preferences. Given that municipal preferences are not homogenous within a state - some municipalities will be averse to the restrictions, instead preferring to increase taxes beyond the restriction. In the situation where a municipality otherwise would have increased taxes, the TEL has the 'true' effect of restricting local tax and expenditures.

There are several assumptions provided in this perspective. First, decisions by municipalities on their budgets are inherently linked to the preferences of their constituencies. Prior research clearly links municipal taxation preferences (and broadly, municipal political leanings) and taxation levels (Tsaunovich and Warshaw, 2014). To the degree to which they have been upheld, both median voter theories and the Tiebout

model (Tiebout, 1956) point to a local government that follows the policy preferences of its constituency (Stevens and Mason, 1996; Gremlich and Robenfeld, 1982; Palus, 2010; Tsaunovich and Warshaw, 2014).

Second there is the point that local governments facing antithetical preemption will either be bound in entirety or subvert the preemption. However, local governments with a policy match with the preemption will not attempt to subvert the preemption. This is the case discussed by Park, Maher, and Ebdon (2018), where prior levels of taxation were used to predict Nebraskan local government tax increases – with mixed results obtained. Additionally, the idea of heterogeneous outcomes resulting from constituent preferences may explain and provide necessary nuance to many of the results of the institutional irrelevance perspective (the second lens noted above).

In conjunction with evasion or circumvention, it should be noted that even where local governments have broad authority to override, one would expect some frictional cost to exist. The argument for frictional costs has mixed results in prior literature (Kousser, McCubbins, and Rozga, 2009; Kioko, 2011, Park, Maher, and Ebdon, 2018); however, even with minimal frictional costs, heterogeneous effects are expected based on the same reasons outlined above. In fact, the style of frictional cost, whether the simple form of a broad tax referendum by a constituency or by the creation of special districts, could impact the structure of the heterogeneous outcomes. Many TEL overrides are related to single topics such as police spending (Park, Maher, and Ebdon, 2018), which can muddle the clear vision of constituent tax preference (Benton and Daly, 1992; Simonsen and Robbins, 2000). As a result, the clearest test of frictional costs would be in a situation where a vote for generic increases was not perturbed by spending allocation.

In sum, when a state preempts that local preference, the local government will be impacted to the degree to which the state policy preemption differs from the policy preference of the local constituents. In the case that a truly binding case of preemption exists, local governments under a strict tax and expenditure will deviate from their previous trend to the degree the preemption disallows. As a result, we are drawn to the first testable hypothesis of this perspective.

H1a: Strict tax and expenditure limits will limit local government revenue streams to a greater degree where constituencies oppose them to a greater degree.

The same question from the opposing end of the scale looks at local governments with constituencies highly in favor of Colorado's Taxpayer Bill of Rights Amendment. These local governments follow the will of their constituents in much the same manner. However, a key dissimilarity occurs when it comes to the application of preemption for these specific governments. Instead of preemption changing the course of policy within these local governments, it reaffirms previous intentions. In effect, the state preemption measure acts in much the same manner that a change to a local government charter would. The counterfactual operates with the assumption that a local government's actions are unchanged when the state preemption measure is in line with local governance preference.

H1b: Tax and expenditure limitations will have minimal impact on the revenue generation of local governments that have constituencies in favor of them.

Due to expectations inherent in each one of the hypotheses – a positive gradient association in H1a and a null expectation in H1b – two analytical strategies are employed to test each form. Expectations of H1b would dictate that a local government with particularly anti-tax sentiment will see null effects against similarly preferenced local government. Tests of each hypothesis are conducted following the background section.

Background

In 1992, Colorado voters passed the TABOR (Taxpayers Bill of Rights) as a constitutional amendment, still considered the most restrictive example of a local TEL in place (Eliason and Lutz, 2018). In 1988 and 1990, Colorado voted on versions of the Amendment, narrowly losing each time. By 1992, when a combination of anti-tax fervor and slightly less strict wording led to the inclusion of TABOR to the Colorado Constitution after 54% percent of the state voted in favor of the TABOR (Rueben and McGuire, 2006).

The public face of the TABOR has become particularly partisan, with two clear narratives pervading the public space. In one narrative, the TABOR has been blamed for dramatically decreased school funding, leaving Colorado schools at a clear disadvantage for funding (Lav and Williams, 2010, Martell and Teske, 2007). The same narrative provides that many counties are unable to provide funding for upkeep on existing capital projects preferred by residents (Martell and Teske, 2007, James and Wallis, 2004). In the opposing narrative the economic and population growth enjoyed by Colorado since 1992 has been the direct product of TABOR (New and Slavinski, 2005). At the same time, others have noted that the reduction of taxes is appropriate, that restricting taxation by

government allows for greater individual liberty. Given the potential for diverse impact for local governments I propose that multiple perspectives on the outcomes are potentially valid.

TABOR is particularly ripe for study in this case for two reasons. First, is the relative stringency of the measure. The policy elements restrain Colorado local governments in several ways that are not combined by other TELs implementation, allowing a cleaner interpretation of outcomes. Second is the ability to clearly view the tax preferences of local government constituents.

Policy Elements

Colorado's TABOR uses particularly strict language to restrict year over year growth in both property taxes and the development of alternative revenue streams based on total levy amount. The average results of the stringent nature of TABOR have been expressed in reductions to local government service delivery (Maher, Park, and Laio, 2019), fiscal capacity during recessions (Martell and Teske, 2007; James and Wallis, 2004), higher revenue volatility (St Clair, 2012) and increased local government aid from the state (Kioko and Martell, 2012).

Because TABOR's restriction is based on total local government levies, often considered the least malleable accounting of local taxation under TELs (Stallman et al., 2017). Additionally, TABOR applies to all Colorado municipal taxes, not just general fund or property taxes the way other TELs do (Joyce and Mullins, 1991). The combination of these elements allow us to assume that all (excepting fee for service) local government revenues in Colorado can be assumed to be constrained by TABOR and alternative streams of revenue will not be pursued simply because they are exempt.

Two additional factors contribute to an unavoidable restriction on revenue under TABOR. First, TABOR restricts tax creation generally unlike many TELs (e.g., Massachusetts's Proposition 2 ½, Nebraska's 1996 Limit) that restrict specific jurisdictions from increasing taxes (Park, Maher, and Ebdon, 2018). As a result, local governments do not have capacity to create special districts to 'offshore' their tax or service burdens. Lastly, Colorado's local governments are subject to oversight from the state regarding their strict adherence to the terms of TABOR. Local governments must return excess revenue above TABOR limits to taxpayers. This strict state oversight without caveats is counter to New York's limit which provides for several significant avenues for circumvention (Lincoln Land Institute, 2020).

This combination of rules included in TABOR dramatically reduce the risk of municipalities resorting to alternative tax streams (Kousser, McCubbins, and Moule, 2008; Wang, 2018) or creating special districts to take revenue streams or expenses away from the main government as seen in other states (Carr and Farmer, 2011; Goodman and Leland, 2019). In addition, the oversight provided by the Colorado Department of Local Affairs provides that the only means for circumventing regulation would be through fraud. The penalty is similarly stringent, requiring all revenues in excess of TABOR limits be returned to the taxpayers (Colorado Department of Local Affairs, 2019).

The avenues allowing growth or evasion by local governments are few: by rate of inflation and growth in taxable property value by improvement. Barring fraudulent reporting – the only avenue for local governments to alleviate themselves the restrictions

of TABOR is by override vote.[7] In the case that a local government places on the ballot a measure for exemption which passes, the local government is allowed to temporarily exceed the limits placed by TABOR.

Direct Democracy

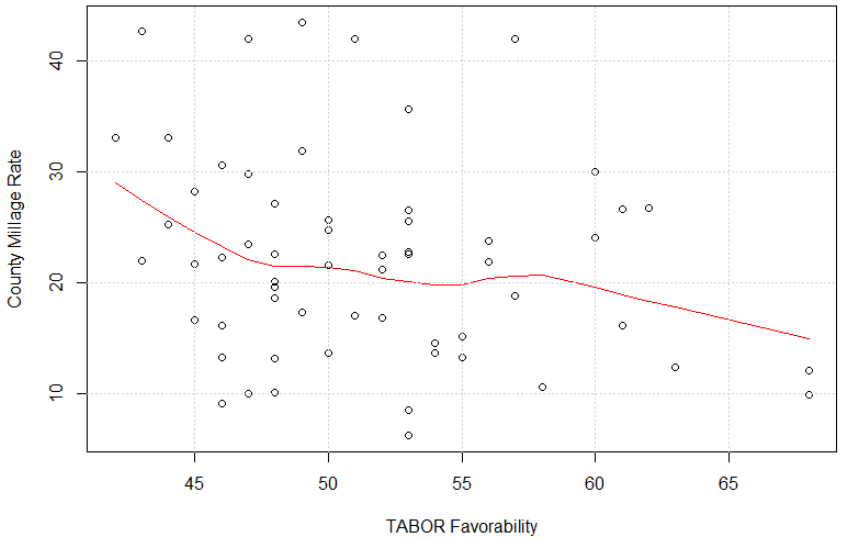
The second reason that TABOR situation is valuable is because it was enacted via direct democracy, thus generating two key elements of evidence. Second, we can see that TABOR favorability was highly heterogenous across the state. At the county level, the results varied from Mesa County, where over 65% of voters supported TABOR to Denver County, where only 44% of voters supported passage. Importantly, although support varies at the local level, the restrictions put in place with TABOR were nonetheless homogenously enforced. In effect, the local vote against the tax restrictions were simultaneously signaling favorability to tax increases – the exact thing prohibited through TABOR.

Additionally, Figure 8 (and Table 10) provides case in point evidence that Colorado's county tax levels were following constituent tax preferences prior to TABOR's enactment. The figure provides county millage rates prior to the 1992 vote on TABOR in association with the County's percent vote in favor of TABOR. There is a clear association between a preference for restriction and the level of taxation prior to the introduction of restrictive measures. While all municipalities voting against TABOR do not have high millage rates, the plot makes it clear that very few municipalities voting for

[7] The use of fees and penalties can only be raised if they are completely removed from regular tax revenue flows. Thus, if they were to bias the results in this case, it would be a downward bias. Further, this study guards against this problem by using property taxes directly, in lieu of business or fine and forfeiture revenues.

TABOR also have high millage rates. In effect, the municipalities with the highest favorability for TABOR are showing signs that they are already de facto restricted by their constituency. In concurrence, James and Wallis (2004) note that "Colorado was [already] among the most fiscally conservative states" when TABOR was enacted.

Figure 8 – TABOR Favorability and County Millage Rate (as of 1992)



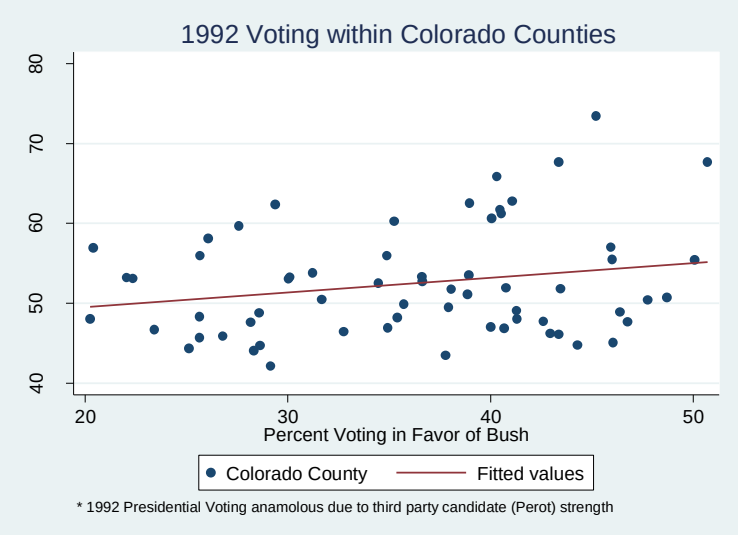
* Red line denotes a loess rolling average function.

In opposition, the direct vote totals show that TABOR preference is only weakly correlated with partisan voting (See Figure 9 and Table 10).[8] Prior studies using partisanship (Kousser, McCubbins, and Moule, 2008) or presidential voting behavior (Jimenez, 2017) as a proxy for local tax preferences may miss important variation. This is not to universally discount previous studies on most accords, but removing the

[8] By happenstance, the 1992 general election provides an additional reason that presidential voting behavior more broadly may be problematic – third party preference. Third party preference can vary dramatically between elections, and may shroud tax preferences.

comingling of policy preference under candidate voting and instead employing an unambiguous local tax preference – may add significant clarity.[9]

Figure 9 – TABOR Favorability Against 1992 Presidential Voting Behavior



Broad Local Government Applicability

In this article, I look at TABOR's impact both on municipal and on county revenues. Although counties and municipalities are not entirely congruent under the Constitution of Colorado, they are similar in their powers and duties (Colorado Legislative Council Staff, 2018). For purposes of governance in Colorado, the key distinction between counties and municipalities are in required duties and existence. In effect, there are no municipal service delivery requirements, but counties do have some minimum requirements. For the purposes here, both have the capacity to set taxation rates (outside of TABOR restrictions) and provide service delivery at levels of their choosing.

[9] Other implications on the analysis are discussed further in the methods section

Like many western states, the role of counties are relatively robust and mirror those of municipalities in eastern states.

Circumvention / Caveats

While the stringency of TABOR provides little opportunity for local governments to work around the components, temporary exemption is allowed with a vote of the electorate. In the period between TABOR implementation and 2000, 22 such exemptions have been successful. (Colorado Secretary of State). The exemptions from TABOR consist either of a reprieve from the spending restrictions or from the maximum rate of taxation and are limited to one to two years.

The greatest caveat in applying the theory to Colorado is the ability of counties to override TABOR restrictions by virtue of a vote. The thought is that municipalities with a higher tax preference can vote to relive themselves of the restrictions. For example, if Denver chooses to exceed the limits imposed upon it by TABOR, it can proceed with a public vote and enact property tax increases beyond previously allowed. At first, the ability to override TABOR by public vote gives intones that no shifts on municipal revenues should occur. However, a public vote does not come without cost. The ability to override TABOR requires that the county send explainers of the increase to voters, conduct votes, and tallied – all prior to action. Each of these steps presents non-zero costs to the municipality. Given that the vote allows an exemption there is still substantial cost to the municipality. In practice the cost becomes – at a minimum – a frictional cost to raising revenues introduced because of the vote provision of TABOR.

Additionally, local governments in favor of higher tax rates may not continue previous taxation trends prior to TABOR because of risk avoidance. Knowing that public

administrators tend to be relatively risk adverse (Chen and Bozeman, 2012; Bozeman and Kingsley, 1998), some counties may generally avoid TABOR exemption votes as the vote result is not guaranteed. Once again, counties, even those with constituencies perceived to be in favor of tax increases may forego the risk and reside within the bounds of TABOR. As a result, local officials will view any tax increase vote with uncertainty. And forego the chance of expenses (by way of additional election costs) with no return in revenues. Simply, a local government's attempts to exempt themselves of TABOR restrictions are not guaranteed success. The reasons for failure could be wide ranging (and additionally ripe for review), but it is sufficient here to assume that putting a tax increase to a vote is not without risk. Prior to TABOR, tax increases believed to be tenable may have passed without constituent affirmation.

On the opposing side, a shrewd official may choose to present a tax increase with expectation of failure – to explain a lack of subsequent service provision. If local administrators observe public desire for service delivery without funding, a vote may be presented to constituents as rationale for the lack of service provision. For example, 70% of Teller County voted in favor of TABOR, but constituents nonetheless expressed interest in a new aquatic center. The municipality of Woodland Park (within Teller County) put a TABOR override measure to voters, and upon rejection, returned to constituents that the aquatic center was unaffordable without additional revenues – effectively claiming their hands were tied (Colorado Secretary of State, Riley, R., 2018).[10]

[10] Of note, the Aquatic Center was eventually built with a bonding measure using dedicated funding within then-current revenue streams in 2014.

A key assumption is that the administrative reactions listed above are not heterogeneously distributed across municipalities; and further are not correlated with the taxation preferences of the local government. To evidence the assumption, Table 10 provides the correlation between the introduction of waiver votes and their success. The correlation figures for county-level votes support this assumption, with a correlation coefficient of only 0.016 between approval of a waiver and TABOR vote favorability. Further, the correlation coefficient between holding a waiver vote and TABOR favorability is only .215 indicating the presence of a vote is similarly uncorrelated.

Table 10 – Colorado County Attributes Correlation Table

	TABOR Favorability %	Rate Waiver Approved	1992 Presidential Vote % for Repub. Candidate	County Millage Rate
TABOR Favorability %	1.0000 n=1008			
Max Waiver Approved	-0.0177 (0.6767) n=504	1.0000 n=504		
1992 Pres. Vote % for Repub. Cand.	0.1788 (0.1057) n=252	-0.0591 (0.350) n=252	1.0000	
County Millage Rate	-0.1516 (0.235) n=63	-0.0745 (0.0932) n=504	0.0036 (0.9543) n=252	1.0000

* No waivers were voted on the TABOR voting year. Yearly TABOR vote is extended across all years granting the same effect as attributes under random effects or Hausman Taylor models.

Given broad the strictness and that overrides of TABOR restrictions appear homogeneously distributed, any changes away to new revenues under TABOR restrictions are similarly homogenously distributed. As a result, local governments in Colorado under a strict tax and expenditure limit scheme – even with the ability to vote to override – become subject to the same effective upper limits for revenue generation.

Additionally, if results are biased by the above caveats they will be biased towards null. Nonetheless, local governments with comparatively high revenue tolerance prior to the introduction of TABOR are surprisingly uniformly restricted to the tolerance levels of those municipalities with lower taxation tolerances.

Through the strict and relatively homogeneous application of the limitations, local governments with a tolerance for tax increases prior to the implementation of TABOR are restricted to tax increases at a rate consistent with local governments with a low tolerance. As an example of this effect – along with a rough application of median voter theory (Black, 1958) – I compare Denver and Arapahoe Counties between 1985 and 1992. Denver County increased per capita property taxes from $84 to $288. Arapahoe County (voting 51% in favor of TABOR) increased per capita property taxes from $123 to $132. During the same period, had Denver County had the same tax increase tolerance of Arapahoe County, taxes would have increased only 7% as opposed to 342%.

METHODS

Preface

The Methods section of the paper consists of a main analysis and a robustness check of the primary analyses. The two analytical techniques employ separate data sets and units of analysis. These analyses are designed as complements to remedy deficiencies of the other. In order to maintain clarity, I discuss the data, method, and results of the primary model (Hausman-Taylor) in entirety; I subsequently address the data, technique, and results of the robustness check (matching/synthetic model).

In beyond analytical techniques themselves, the use of a secondary data set also reduces the likelihood that outcomes are a byproduct of financial reporting requirements related to each data set – whether by outside audit or by non-audit state mandate. Further, I employ two local government types to reduce any likelihood that outcomes are the result of the type of local government, thus increasing generalizability.

The first analysis, a Hausman-Taylor analysis of all counties in Colorado, aims to determine the degree to which TABOR impacts county revenues – and most importantly, how the constituent tax preferences moderate that effect. While the Hausman-Taylor model has benefits over the matching design through increased sample size and clear scaling of the moderating variable, there are shortcomings. Specifically, a single observation of tax preferences in the 1992 TABOR vote is employed as a constant, eliminating the potential to measure heterogeneous shifts in preference by municipal constituents.[11] Additionally, employing a non-staggered treatment point for all counties at one point allows unseen treatments that interact with vote totals to introduce bias.

The robustness check serves two purposes. First, this analysis uses a matching technique that aims to remedy some analytical shortcomings of the Hausman-Taylor model, especially the issue of assumed time invariance of constituent policy preferences (See the following section). Second, the analysis serves as a methodological exemplum. The empirical design employs two matching strategies to illustrate how unobserved heterogeneous treatment errors can occur through covariate selection. The matching

[11] Subject covered at greater length in later sections

analysis most directly addresses hypothesis 1b, while also indirectly evaluating hypothesis 1a.

Main Analysis: Hausman-Taylor Design

Data – County Level

The main dataset consists of a novel combination of data from four separate sources of 63 Colorado counties from 1985 through 2000. The bookend dates are chosen for consistency in the property tax environment in Colorado. In 1984, Colorado passed the Gallagher Amendment (Const. of Colorado – Art. X § 3 and 15) requiring a strict ratio of levy between residential and commercial properties shifting the previous property tax landscape. The choice of end date is the result of the incorporation of Broomfield County, drawing land area and property taxes revenue from several surrounding counties, thus disrupting continuity of revenue streams for those surrounding counties.

Property taxes. The primary dependent variable is county level *property taxes per capita*, drawn from yearly reports of the Colorado Department of Local Affairs to the legislature. The reports contain total county-level property tax revenue, levy rates, and include split outs for dedicated revenue streams – total county-level property taxes not allocated to public schools or colleges is employed in the analysis. Reports are considered accurate because they are statutorily required reporting by counties to the state to determine compliance by Colorado local governments with TABOR and other property tax regulations.

Correlation between property taxes levels before TABOR implementation and TABOR favorability may be problematic (evidenced in Table 10 and Figure 2). As a result, *percentage change in property tax levy* is employed to reduce the impact of leveling issues from heterogeneous millage rates at the beginning of the panel period.

Table 11 – Descriptive Statistics for Main Data Set

Variable	N	Mean	Stan. Dev.	Min	Max
Total County Property Tax Revenues (in 000's)	1008	9,932.19	21,879.89	183.98	162,471.20
County Property Tax Revenues (Per Capita)	1008	255.25	150.7845224	47.75	908.95
TABOR Voting (% In Favor)	63	52.02	6.38	42	68
TABOR Year Implemented	1008	0.5	0.50	0	1
Total County Population	1008	57636.59	120073.37	453	556738
County Millage Rate	1008	21.23	8.67	5.46	47.5
TABOR Maximum Waiver Approved	1008	0.021	0.14	0	1
Per Capita Income	1008	19587.26	7023.11	7213	81326

* TABOR Voting is observed in 1992, but is treated as a constant observation across years for regressions

Population. *Population* is drawn from the State of Colorado's Department of Local Affairs Demography Office. The population numbers are employed because of increased yearly granularity extending back for the entire period of study. Two additional control variables are drawn from the Colorado State Department of Local Affairs, whether a TABOR spending waiver was approved in a given year (TABOR Spending Waiver Approved) and whether the a maximum rate waiver was approved (*TABOR Maximum Waiver Approved*). Both variables are coded binary with a positive as one and a negative as zero.

Per capita income. Controls for per capita income were drawn from the U.S. Bureau of Economic Analysis. Per capita income is used to control for potential tax base shifts. For instance, if income shifts present new growth within one county versus another – as was the case much later with the discovery of oil reserves in southwestern Colorado – per capita income is expected those trickle down revenue changes.

Rate of TABOR Approval. The key independent variable – from the third data source – is rate of approval by voters within each county for TABOR in 1992. The voting approval rate is drawn from reports of the Colorado Secretary of State. The variable is used as a strict percent of the vote on TABOR. The choice of this variable is valuable, as noted in the background section – however, it also presents a challenge because it is a single observation over time. Given the theorized alignment of voter taxation preferences at 1992, the correlation between year over year voter consistency within a municipality was balanced against the correlation between tax preference and party voting behavior as provided in by correlation Tables 1 and 3. Put simply, there is less change in voter preferences over time than there is spread between party vote and tax preference. Additionally, the implementation of a statewide TEL has been shown to reduce intra-state, inter-municipal residential mobility (Skidmore and Tosun, 2011). While neither measure is perfect, TABOR vote is preferred as it is assumed to more closely mirror taxation preference.

Table 12 – Correlation Table of Presidential Vote Percentage for Republicans by County

	1988	1992	1996	2000
1988	1.000 n= 63			
1992	0.8051	1.000		

	(0.000) n= 63	 n=63		
1996	0.9224 (0.000) n=63	0.9086 (0.000) n=63	1.000 n=63	
2000	0.8949 (0.000) n=63	0.8899 (0.000) n=63	0.9129 (0.000) n=63	1.000 n=63

* P-values listed in parentheses

Hausman-Taylor Methods

Although the dataset has a panel construction with repeated measures, a standard fixed effects model is unavailable because voting behavior is measured only once for the duration of the panel, and is treated as an unvarying attribute of the county. As a result, a Hausman-Taylor model is employed because it combines aspects of fixed and random effects models. The two-stage design allows for bias elimination provided by fixed effects models and the efficiency provided by random effects models, allowing for the broad use of both time-invariant and attribute variables (See Cornwell and Rupert, 1988 and Egger and Pfaffermayr, 2004). In the Hausman-Taylor model, the function of the two stages are not like a typical instrumental design where a regressor is assumed correlated with the error term. Instead, the instrument in the Hausman-Taylor design is assumed to be correlated with the individual random effects – exactly the case for the vote for TABOR approval. Additionally, while I report robust standard errors, they are consistent with bootstrap standard errors.

The main analysis includes an additional random effects model with year dummies to account for time effects for reference and comparison to the Hausman Taylor design. While the random effects model is preferred for both accounting for the effective time invariancy and simplicity in interpretability; there are significant drawbacks to its

use in this situation. Most importantly, the random effects model assumes that specific effects are uncorrelated with independent variables. The Breuch-Pagan test scores provided in Table 13 evidence that the assumption is not met. As a result, the results of the random effects model in not considered free of bias. The results of the random effects model are provided with robust standard errors.

Hausman-Taylor Results

The results section will be described in the order provided in the methods section. The results of the Hausman-Taylor model are provided in Table 13. The key independent variables in the non-interacted models are TABOR implementation which shows a decrease of property taxes by -11.95% and carries statistical significance at the 5% level. The value for TABOR favorability alone is -3.39% and carries a p-value of 0.27. However, in the full regression model, the interacted variably – the focus of the analysis looking at the combined influence of TABOR implementation and TABOR favorability within the county – the value is 0.23% and has significance at the 5% level. Additionally, statistical significance is reduced for both TABOR and voting behavior alone – a theoretically expected result.

Table 13 – Results for Hausman-Taylor and Random Effects Models

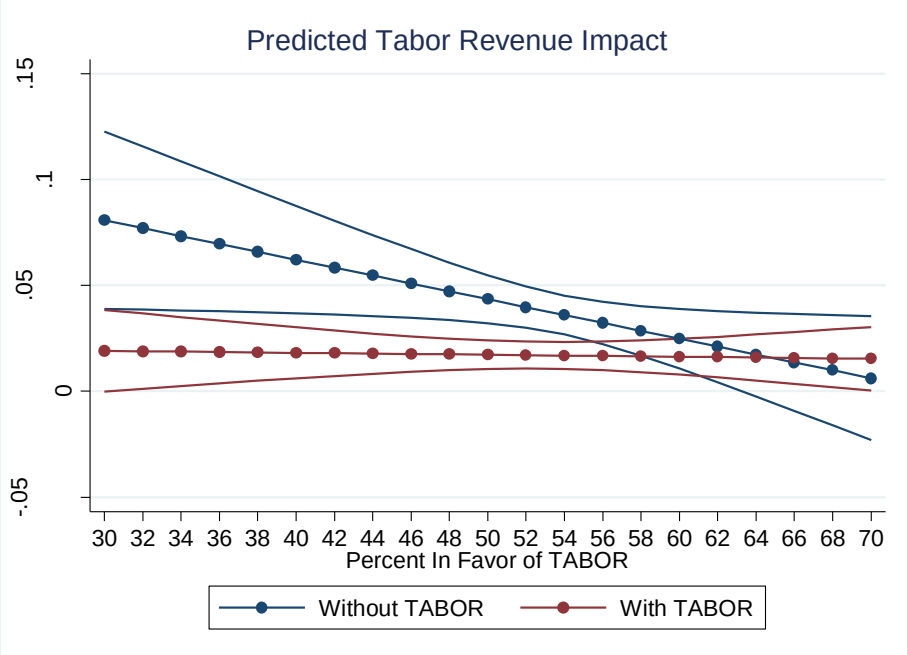
	(1)	(2)	(3)	(4)	(5)	(6)
	Base Hausman Taylor	Hausman-Taylor w/No Interaction	Full Hausman Taylor	Base Random Effects	Random Effects No Interaction	Full Random Effects
TABOR Alone	33.94***	-11.95**	-51.51*	91.24***	22.63*	-101.31***
	(3.85)	(4.13)	(21.99)	(7.78)	(10.53)	(23.23)
TABOR Vote %	-3.562	3.393	2.981	-5.921*	0.576	0.803
	(2.61)	(3.10)	(2.92)	(2.76)	(2.12)	(2.11)
Interaction			1.243**			1.535***
			(0.42)			(0.40)
Total Population (ln)	-30.79*	-109.9***	-97.88***		-76.39***	-71.11***
	(13.77)	(11.96)	(12.43)		(7.73)	(7.83)
Per Capita Income		0.00778***	0.00768***		0.00628***	0.00630***
		(0.00)	(0.00)		(0.00)	(0.00)
Spending Waiver		-5.353	-5.274		-6.183	-5.944
		(4.87)	(4.86)		(4.91)	(4.88)
Maximum Rate Waiver		29.91**	29.65**		32.09***	31.49***
		(09.50)	(9.47)		(9.45)	(9.38)
Population Density		0.0765*	0.061		32.09***	31.49***
		(0.0377)	(0.037)		(9.45)	(9.38)
Building Permit Count		-0.0018388	-0.009821		0.00168	0.00280
		(0.00257)	(0.00258)		(0.00)	(0.00)
Constant	715.9***	967.3***	878.6***	509.4***	798.0***	736.8***
	(145.6)	(163.0)	(155.7)	(144.7)	(112.7)	(113.6)
N	1008	1008	1008	1008	1008	1008
				B-P < 0.00	B-P Test < 0.00	B-P Test < 0.00

Standard Error in parentheses

* p<0.05, ** p<0.01, *** p<0.001

Notably, the result for TABOR alone is rather large, voting levels are small and require interaction. Some contextualization aids in understanding. For a simpler understanding of the outcomes on interacted terms, I provide predictive margins in Figure 10. As can be seen, the expected impact from TABOR implementation decreases with increases in TABOR favorability – as proposed. While confidence margins are wide, the point estimates nonetheless significant and distinct when comparing differences of voting behavior by 10% points or higher. Additionally, the results of the Hausman-Taylor model confirm the results of the synthetic model across a larger population of local governments and are consistent in scale.

Figure 10: Predictive Margins of TABOR Outcomes Based on TABOR Favorability



Additionally, the results for controls aid in determining veracity of the overall model. Although controls are limited, they are consistent with theorized positions. First and most obviously expected, the approval of a maximum tax waiver increases property

taxes by 19% per year with significance at the 1% level. Less intuitively, the approval of the less discussed spending waiver decreases taxes by about 1% in a year and does not have significance. The small scale and insignificance is expected as spending waivers are usually offered after levy miscalculations that would otherwise be returned to taxpayers. Anecdotally, the amounts are small, unplanned, and accidental.

The last set of results within the first analysis are those for the random effects model. As can be seen in Table 13, the results are relatively consistent with the results provided in the previous two models. The variable of interest has a clear but small impact on the outcome of county property tax revenue, showing a .196 percent decrease in revenues per year, per percent voting against TABOR. Although the Breusch Pagan score does not allow the results to stand on their own, consistency across models nonetheless buttresses their results.

Robust-Check Analysis – Match Case Design

Data – Municipal Level Data

To further test the hypothesis, I employ a municipal level dataset to conduct case matching based on the strategy developed by Abadie and Gardeazebal (2003), and termed synthetic control analysis (Abadie, Diamond, and Hainmueller, 2010; Abadie and Gardeazabel, 2003). The secondary dataset is drawn from the Fiscal Policy Space Data Portal compiled by the University of Illinois at Chicago's College of Urban Planning and Public Affairs. The dataset itself is drawn from seven diverse sources including individual municipal Audited Annual Reports, Lincoln Land Institute's *Property Tax at a Glance*, U.S. Census of Governments Intergov Attribute Index, U.S. Bureau of Economic Analysis, The Decennial Census, and a range of policy attributes assembled by Fiscal

Policy Space itself (Fiscal Policy Space Portal). The result is a panel dataset with multiple research applications

To fulfill requirements of the synthetic control estimation procedure, I removed municipalities that lack observations in the variables of interest during the entirety of the pre or post period (defined as 1984 to 2003, for the same reasons as the main analysis). The remaining 100 U.S. municipalities of the total skew towards larger municipalities with longer terms of robust accounting measures. Municipalities located in Colorado (Denver and Colorado Springs) constitute the treatment group, while the rest of the dataset (98 municipalities) comprises the counterfactual pool as potential matches. In total there are 2,000 observations in the data set, although the panel is strongly balanced, not all observations for all variables contain values. Descriptive statistics for all variables employed are provided in Table 14.

Table 14 – Descriptive Statistics for Municipalities included in the Robustness Check

Variable	Obs.	Mean	Stan. Dev.	Min	Max
Total Population	2000	516,381	875,875	42,830	8,214,426
Property Tax Levy (000's)	1998	150,988	163,925	441	915,800
Property Tax Per Capita	1998	361.14	312.62	5.55	1703.96
Income Per Capita	2000	19,257	5,985	9,258	45,453
% Municipality Voting for Democratic Candidate	500	54.44	11.99	22	87

The synthetic control analysis focuses on property tax per capita and remains untransformed for the analysis. Controls drawn from the Fiscal Policy Space Portal consist of per capita income, unemployment percentage, and yearly population for each municipality. In addition, a key differentiating variable – existence of a TEL in the

municipality during the given year is drawn from the dataset. The determination of a TEL in existence is based on the factors put forth by Mullins and Wallin (2004) and is binary coded.

Robustness Check Analysis Methods

The robustness check employs synthetic control methodology not only as a robustness check, but also a methodological example and as a tool for further elucidation of the theoretical precepts of this paper – that politics matter when evaluating assumed taxation trends in counterfactuals. Synthetic controls serve to bolster the evidence presented by the Hausman-Taylor and random intercept models above by providing case study evidence of trends between the two largest municipal areas in Colorado against 98 other municipalities in the US. The two largest municipalities in Colorado (Denver and Colorado Springs) have dramatically different political landscapes and property tax preferences.

The synthetic control methodology operates similar to coarsened exact matching; wherein the treated municipality is matched with alternative untreated municipality and the difference assumed as the causal impact of the treatment. In the case of the synthetic control, the matched untreated municipality is a weighted agglomeration drawn from a potential pool of untreated municipalities. Per Ross (2018), the weighted agglomeration intends to replicate "a counterfactual [municipality] that is structurally the same as the state that actually adopted the [treatment]."

With the untreated 'synthetic' municipality mimicking a treated municipality, a typical matched difference in difference design is followed. The equation is as follows:

Equation 1:

$$\hat{\alpha}_{1t} = Y_{1t} - \sum_{j \geq 2} w_j Y_{jt}$$

Where Y_{1t} is the outcome of interest in the treated municipality in year t; while $\sum w_j Y_{jt}$ serves as weighted average of the donor units where w is the weight used in the pre-period to satisfy the parallel trends assumption of the DiD. As a result, $\hat{\alpha}$ becomes the average treatment effect for each year. In simplified form, ATE is the result of subtracting observed outcomes in the weighted average "match" from observed outcomes in the treated municipality.

The synthetic control methodology provides two major advantages to the use of single matched case. The first advantage is that the quality of trend matching is dramatically increased. By weighting multiple municipalities, the aggregate trend is more likely to be consistent in pre-trends than by an individual match. Second, the likelihood of impact of interpreting unobserved simultaneous events as the causal effect is reduced. For example, with a single match, any event in the counterfactual municipality will bias the entire counterfactual. Using a synthetic counterfactual the unobserved simultaneous event will be limited to the weight of the municipalities with the unobserved event. For example, if the counterfactual municipalities were drawn from a state where new accounting rules were implemented simultaneous to the treatment, that impact may be misinterpreted as treatment effect. Part of the purpose of a synthetic control is to mitigate

that potential. It should be noted that the synthetic control does not contravene all problems with unobserved treatments and only partially mitigates them. In fact, the following methodological choice describes one way the synthetic is still subject to this flaw.

Further, the use of synthetic control provides a methodological example where the design allows one to see clear implications of choosing matching variables. Some literature employing synthetic controls argue that using identification variables that minimize the MSPE of the outcome variable is most appropriate (Dube and Zipperer, 2015; Ferman, Pinto, and Possebom, 2020). Ferman, et al. (2020) propose that using matching variables selected in an automated process to alleviate the potential for "cherry picking" variables that produce statistically significant outcomes. While the data driven approach is enviable in reducing the ability of investigators to make choices desirable to favored outcomes (i.e., those outlined by Brodeur, Cook, and Heyes, 2020), there are significant drawbacks to the automated approach. I argue the data-based approach is inconsistent with the belief that matching and controls should be theoretically driven when developing causal inference (Spector and Brannick, 2011). Specifically, the automated approach may prove problematic when variables are endogenous or are multicollinear – in the same way they are problematic in other regression settings – generating the potential for biased results.

To provide evidence of the disparity between the two matching perspectives, I employ each perspective in a pair of synthetic models. The first two models match municipalities on property tax trends, population, and median income, representing the data-driven approach favored by Dube and Zipperer (2015) as the RMSPE for the main

outcome is most closely matched. The first pair of synthetic control models are expected to be consistent with previous models determining TEL impacts where political preferences are not the focus or are omitted entirely (Ross, 2018; Zhang and Hou, 2020 for matching designs, Skidmore and Tosun, 2011 for within state variation).

The second pair of models match Denver and Colorado Springs based on typical variables, plus the addition of presidential voting behaviors to synthetic municipalities (the theoretically pertinent independent variable in this case). This second set of models illustrate how the municipalities in Colorado compare to municipalities with equivalent political leanings but that did not have restrictive TELs put into place.

Tables 15 and 16 provide information on municipal match quality and weights using determined with the matching process described above. Table 15 provides RMSPE – the root mean square percent difference between the treated and untreated municipalities during the pre-treatment period. The RMSPE values show a lower match quality when requiring a match on the additional presidential voting behavior variable to match on. Although including variables for voting behavior reduces match quality in both cases, a visual inspection of the pre-trends in Figures 11a, 11b, 12a, and 12b reflect that an extremely high match quality is maintained. Table 16 provides the weights attributed to each municipality to achieve the pre-trend match. The provided weights allow transparency and additionally show that matches pass the eye test. For the sake of concision, I do not show municipalities with weightings of less than 1%. The different results show that tax preference can heavily influence the counterfactual for TELs research and the potential for omitted variable bias from if it is not included without intention.

Table 15 – Synthetic Control Match Quality

	Property Tax and Population		**Property Tax, Population, and Presidential Votes**	
	Colorado Springs	Denver	Colorado Springs	Denver
Overall RMSPE	.0077124	.0285829	.0747129	.0843124

Table 16 – Synthetic Control Match Composition – Top Contributing Municipalities

	Property Tax and Population		**Property Tax, Population, and Presidential Votes**	
Treatment Municipality	Matching Municipality	Weight	Matching Municipality	Weight
Colorado Springs	Mobile, AL	.414	Lubbock, TX	.449
	Columbus, OH	.247	Orlando, FL	.424
	Augusta, GA	.178	Phoenix, AZ	.127
	Cape Coral	.015		
Denver	Columbus, OH	.083	St Louis, MO	.395
	Los Angeles, CA	.024	Little Rock, AR	.124
	San Diego, CA	.018	Philadelphia, PA	.090
	Phoenix, AZ	.014	Santa Rosa, CA	.011
	Cleveland, OH	.012		

* Weights below 1% contribution are omitted

Robust Analysis Results

The results of the secondary analysis are presented as a set of two comparisons between matching strategies. Table 17 shows the outcomes for Denver and Colorado Springs by year when the match was conducted using a politically neutral approach. Both Denver and Colorado Springs show similar statistical significance as a result of the changes in 1992 and continue through 2000. Denver begins with an immediate 10%[12] drop in per capita revenue compared to the synthetic counterfactual which raises to a 16%

[12] The effect sizes in the first year appear particularly large and I spoke with multiple county finance employees about the issue. Two separate county employees intonated that local governments may have pushed for higher tax rates prior to TABOR implementation with the intent of alleviating immediate stress from restrictions.

percent drop by the year 2000. Results for Colorado Springs actually show a larger initial impact, at 25% which increases to 29% by 2000.

Table 17– Synthetic Control Effect Sizes and Significance

	Property Tax and Population		**Property Tax, Population, and Presidential Votes**	
Year	Denver	Colorado Springs	Denver	Colorado Springs
1992	-0.106	-0.251	-0.339	-0.014
1993	-0.124	-0.225	-0.299	-0.023
1994	-0.133	-0.245	-0.304	-0.023
1995	-0.142	-0.263	-0.322	-0.029
1996	-0.150	-0.271	-0.330	-0.027
1997	-0.151	-0.280	-0.344	-0.027
1998	-0.159	-0.291	-0.353	-0.025
1999	-0.162	-0.246	-0.305	-0.034
2000	-0.165	-0.295	-0.346	-0.028

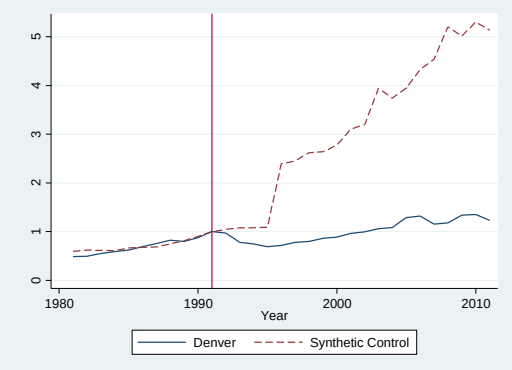
* Note that results provided in figures 11(a-b) and 12(a-b) extend the results beyond 2000

The results of the second set of synthetic control designs carry much larger results for Denver than for Colorado Springs, consistent with theorization for a municipality with higher than average tax preferences. Again, Denver shows significance starting in 1992 and continuing through 2000 and much higher impact, with a 33.9% drop in per capita revenues increasing very slowly to 34.6% in 2000.[13] The first year of implementation However, the results for Colorado Springs run contrary to the results in the first model. Although Colorado Springs separates from the synthetic counterfactual in 1992, the effect size is comparatively minimal with only 1.4% change in tax rates. In addition, statistical significance in not carried through all of the out years after the implementation of TABOR and only occurs in 1992 and 1999. Even at the peak of

[13] As noted in the discussion section, economic conditions on a year by year basis in municipalities with less strict TELs may heavily influence outcomes.

influence, Colorado Springs only saw a 3.4% reduction in the per capita revenues in comparison to a municipality with similar population growth and political disposition.

Figure 11(a-b): Denver/Colorado Springs matched on unemployment, pop, and income



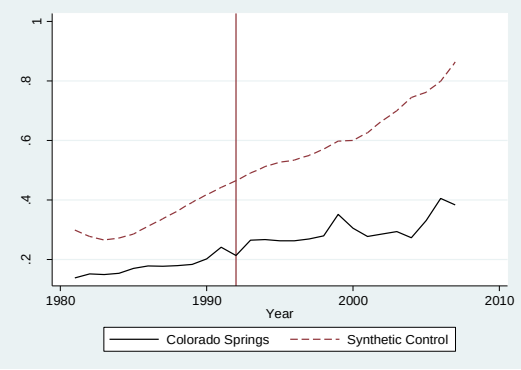
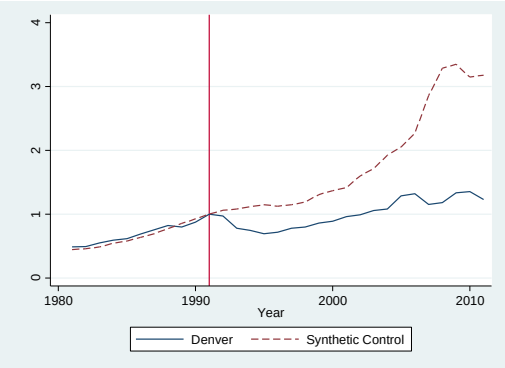
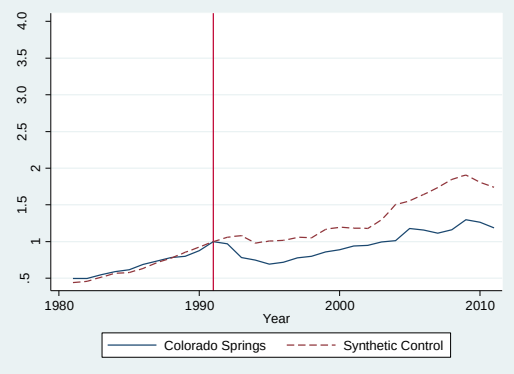


Figure 12(a-b): Denver and Colorado Springs Matched on Prior and Presidential Voting





While the use of synthetic control methodologies for causal inference are well regarded, there are nonetheless two significant drawbacks to the use of synthetic controls in this instance. First, as previously noted, presidential voting behaviors – while correlated – are not perfect parallels of constituent tax preferences. Secondly, the small number of municipalities included in the first analyses opens the door for alternative causal effects being at play. While unlikely, the combination of these two issues relay that outcomes could be the result of alternative causal explanation or unobserved

exogenous influence. As a result, a secondary analysis is conducted to remedy any concerns that the exploratory analysis does not.

Discussion and Conclusions

The clear, consistent results across multiple settings and analytic techniques provide strong empirical support for the first hypothesis. It is clear that a restrictive set of TELs are far more binding for municipalities that both have a prior preference *against* tax restrictions and have a history of allowing tax increases. It is important to note, however, that these results do not dispute aggregate results of previous studies such as Martell and Teske (2007). It merely lends evidence as to where the aggregate effect is – distributed narrowly. As such, evidence suggests that H1a is supported and the negative revenue impacts of TABOR increase with county opposition to the TABOR.

It is important to similarly note the small effect size being seen across the results. An immediate reaction is that a difference of fourteen cents may not seem large. However, the change is per year per percent TABOR favorability becomes large rather quickly. Using Denver's 44% approval (6 % below the mean) as an example, a 0.14 percent per year change starting in 1992 and continuing for 8 consecutive years equals a $19.35 change in property taxes per capita.[14] The effect size may appear minimal, but the analysis portrays a loss of over $10.3 million of revenue in just 2000 in Denver and

[14] This is based upon the results of the full Hausman-Taylor model. The synthetic control model would indicate a 16% revenue decrease, amounting to a $46 per capita (and $24.5 million total) property tax revenue loss per year in 2000 for Denver.

compounds to a 3% decrease in overall revenue in 2000 and an 11% decrease in revenue by 2010.

The synthetic control model bolsters the intuition of H1a and provides evidence that H1b is correct. In particular, the results in column 4 of Table 8 (and Figure 12b) provide evidence that after accounting for political leanings, Colorado Springs saw minimal changes to tax tendencies after the implementation of TABOR. The analytical choice further exemplifies the need to consciously decide on appropriate matching variables. In the case that the impacts of TELs in a politically neutral (or 'at means' politically) situation are desired, then the results portrayed in the first regression are appropriate. However, if one wishes to develop a counterfactual with a clear "but for" approach, politics within the local government should be accounted for as in the second set of models. Additionally, the results of the synthetic control method employed here give rise to greater generalizability, as they apply the same principles seen in the results for Colorado Counties to a pair of municipalities within the state.

Although this paper can conclude that Colorado local governments faced different shifts in revenue growth after TABOR – based on their taxation preferences, generalizability beyond Colorado becomes more challenging. As noted, other states often have more leeway for local governments to work around TELs restrictions. Given the results of this article, one would expect that local governments in other states would work around their restrictions at rates consistent with the tax preferences of their constituents. However, those elements are currently untested.

Further, it is posited that the consistent positive overall effects of TABOR alone are consistent with the findings of Wu and Yu (2020). In Wu and Yu (2020),

municipalities hoard during good times as they are unable to increase taxes during economic downturns. Figure 10 provides evidence in the present case, where pre-TABOR local governments not only increased property taxes during economic downturns but were willing to decrease taxes during economic upswings. In the post-TABOR landscape, it appears that local governments are unwilling to lower taxes during positive economic periods. Thus if the period in question could be considered an overall positive economic period under that consideration, TABOR would manifest itself as an overall increase in revenues.

Returning to the initial thrust of the article and the relation to preemption, the results indicate that public perception of preemption may not be without merit. Both the definition and academic understanding of preemption is clear - that it exists at any point that a state or federal government restricts the choice set of a local government. However, what is less clear is the degree to which choice the preemption limits the effective choice set of the municipality. The results of this paper show that while preemption policy may limit the agenda universe for some local governments, it may leave the range of systemic agenda items untouched. It could be argued that - for example - Colorado Springs' systemic agenda remained unmoved by TABOR, while the same amendment fundamentally restricted the systemic agenda of Denver.

4: MUNICIPAL FORM AND FORECASTING BIAS

Introduction

At the heart of public administration lies tension between bureaucratic expertise and democratic representation (Maier, 1997, Meier and O'Toole, 2006, Keiser, 2010, Park, 2013, Marvel and Resh, 2015, Resh and Zook, 2016, Riccuci and Van Ryzin, 2017). Expertise is required to deliver a host of public services, but responsiveness to the public's wants necessitates some form of representation. The overarching debate plays out in many places, including the structure of municipal government where mythical stories of mismanagement at the behest of political whim led to the creation of the council-manager system. This system, which places professional staff in charge of the administrative responsibilities of government, was presented as the "antidote to the corruption in big-city machine politics" (Nelson and Alfonso, 2019).

The question still remains whether the system has matched its promise (Carr, 2015). In this paper, I examine whether and how the structure of a municipal government affects its ability to accurately forecast its revenue streams. The short-term gains to political actors influencing revenue projections are clear. The partisan goal of funding a political pet program or painting a rosy picture of forthcoming economics (Frant, 1996) becomes feasible with a strong revenue forecast. In opposition, a negative forecast places economic gloom and appearances of mismanagement at the feet of a political operative. Clearly, political actors have reason to pressure revenue forecasts that would not appear as obvious with municipal managers.

While the short-term political gains are clear (Krause and Corder, 2007), there are also clear long-term repercussions to poor revenue forecasts. Without a reasonable estimation of how much revenue will be coming in, the municipality is beset with an

inability to commit to long term planning. Removing the incentive for political opportunism in forecasting - and instead gaining risk averse accuracy, allows a host of benefits for a municipality – increased credit worthiness, solvency, and rainy-day funding (Marlowe, 2007; Marlowe, 2012). In short, revenue forecasting appears to provide a keystone opportunity for the increased professionalism of the council-manager form to shine.

Although several works lead up to the question of whether municipal form of government impacts forecasting, the linkage in question is still pending (Mikesell and Ross, 2014; Jimenez, 2020). The ramifications for inaccurate or biased revenue forecasts – misappropriations, decreased credit ratings, lack of appropriate reserves – are obvious dangers to municipal well-being. Further, given an expected universal preference (by constituents) for more transparency in the quality of forecasts and a similar preference for accuracy; if this article can illuminate one way to structurally increase revenue forecast accuracy.

This article aims to resolve whether the structure of municipal government impacts the accuracy and bias of revenue forecasting. The remainder of the essay is as follows: the second section provides background on the issue; the third section provides literature and theory, culminating in hypotheses; the fourth section provides an explanation of the data employed; the fifth section provides the analytical techniques employed; the sixth section provides the results; and, finally the seventh section provides conclusions and a discussion.

Theory and Related Literature

This article combines two areas of literature with extensive backgrounds: revenue forecasting and municipal management form. I begin by discussing the role of forecasting accuracy and bases and extend to the wide range of governance impacted by municipal governance form. I end with the confluence of the two and proposition of hypotheses.

Determinants of Municipal Forecasting Bias and Accuracy

Revenue forecasting in the context discussed here, consists of a municipal government's estimation of revenues in the forthcoming year. Municipalities typically rely on these revenue estimates as a part of the budgeting process. Municipalities face a wide range of options for revenue forecasting from the use of complex econometric techniques to estimates provided through in house expertise (Reddick, 2004). The choices of forecasting technique and the potential bias of estimators come from a wide range of institutional factors (Reitano, 2018). Revenue forecasting is the expectation of the funding for the next years' budget; leading to cuts based on expected budget shortfalls, or program expansions based on expected windfalls. While the long-term value of biasing revenue forecasts would be debatable, the desire to do so may not be so debatable.

It is important to note that accuracy and bias are not equal. While the two are associated and consistently run into each other, they cannot be equivocated. Accuracy speaks to the ability of a forecast to hit the intended mark. For instance, Hou (2006) provides evidence that states regularly underestimate their revenue streams to mitigate the risks associated with overestimated revenues (Bretschneider, et al., 1989; Rogers and Joyce, 1996). In contrast, some governments create more accurate (closer to actual values) because they are subject to balancing between multiple forms of bias (Krause and

Douglas, 2006), including those that are looking for additional funding (Bretschneider et al., 1988; Krause, Lewis, and Douglas, 2006; Krause and Corder, 2007).

The greatest determinant of forecast accuracy is governmental resources, such as a specialized employee capital, computing capacity, and funds (Forrester, 1991; Reddick, 2004; Kong, 2007; Reitano, 2018). As a result, it is expected that smaller municipalities have a diminishing capacity to provide accurate forecasts (Kong, 2007; Reitano, 2019). As a result, any empirical work must take into account for the resources available to the municipality. However, an important counterpoint by Lawrence et al. (2006) provides that the accuracy of judgment-based forecasts (or naïve forecasts) are comparable in value to statistical techniques – with the significant caveat that the forecast dependability is highly related to the forecasters' biases. As by Williams and Calabrese (2016) note in their comprehensive review on revenue forecasting "literature shows that top decision-making preferences, whether managerial or political, influence the point estimates creating systematic error" (p. 131-132).

While more complex and involved modeling is important for forecasting accuracy, structural impacts such as ensemble forecasting[15] are similarly – if not overarchingly – important to explaining forecast bias (Bretschneider, et al., 1989; Mikesell and Ross, 2014). Importantly, Bretschneider, et al. (1989) provide insight about state level influences on forecast accuracy, including ideology, and the need for consensus building. The focus on consensus building for forecasting could have two competing explanations. First, it could attributed to the "wisdom of the crowds," where

[15] The use of multiple forecasts from a diverse set of governmental participants.

averaging the input from more diverse perspectives produces a higher quality forecast - akin to a carnival weight guessing game (Murr, 2011). Alternatively, the quality of consensus forecasting can be attributed to the need to accede from ideological preference or bias – the position hinted at by Bretschneider et al. (1989) and further elucidated by Krause and Douglas, (2013). Given the persistence of results indicating risk aversion - but not inherently accuracy - from single party domination, the latter appears more convincing in a municipal government (Bretschneider, et al. 1989; Chapman and Gorina, 2012).

Further, there is significant reason to believe that another factor may be involved in municipal budget forecasting, the structure of the government itself which leads us to the next section.

Why Managers and Mayors May Behave Differently

Per Nelson and Svara (2008), the mayor-council system primarily differs from a council manager system by the accountability and position of the individual granted administrative authority. In the mayor-council form, the administrative authority rests with the mayor; while the administrative authority rest between the council and manager in the latter form. For the purposes of this investigation, the foci of administrative power translates itself in two important ways. The first difference is by their intrinsic preferences and motivation: mayors are far more motivated to return to the private sector after service while managers carry far higher levels of innate desire to serve the public in the long term (Enikopolov, 2014; Miller and Whitford, 2007). Second, the mayor and manager differ in who they are an agent of: politicians tend to have short term goals for

obtaining reelection (Maskin and Tirole, 2004), while career bureaucrats tend to have longer-term goals associated with sustainable governance and service to the public (Miller and Whitford, 2007; Enikopolov, 2014).

Prior literature makes predominant use of principal-agency theory to argue that public managers differ from private sector and political actors (Frant, 1996; Miller and Whitford, 2007; Jimenez, 2020). In effect, public managers face different terms of service, where the greater lengths of service lower risk propensity and reduces appetite for short term opportunistic gains (Frant, 1996; Bozeman and Kingsley, 1998; Feiock, Jeong, and Kim, 2003, Hessami, 2018). Fundamentally, the appetite to commit to short-term rewards – in this case, forecasting higher revenues to maximize the budget over the course of an election cycle – is dramatically lower in a career manager than it is for a political actor.

Frant (1996), in comparing politicians to public administrators, notes that political opportunism is a driving factor for political actors and removing that motivation is expected to increase allocative efficiency. In this case the expected result is an increase in quality - interpreted here as accuracy. From Bozeman and Kingsley's (1998) assessment that public administrators are more risk averse, public administrators associate with longer-term goals such as the length of their career (Frant, 1996; Feiock and Kim, 2001), and with service to the public interest (Miller and Whitford, 2007). Further, evidence that mayors and managers use municipal employment strategies to enable their policy preferences (Alesina, Baqir, and Easterly, 2000) indicating that the mayor or manager pervasively influences the municipality.

In the alternative, the design of the council-manager system was not likely done for the purposes of creating a clear divide between politics and administration (Montjoy and Watson, 1995). Taking the argument of Montjoy and Watson (1995) a step further, one could claim that the vision of a city manager as non-political actor is merely a figment in the mind of academia. Nelson and Svara (2015) present a middle ground, where managers serve primarily as strategic advisors to their councils.

Adding heft to the argument that municipal managers are politically driven, Svara (2001) provides that the strictly dichotomous split between politics and public administration is a myth. Further, Svara presents evidence specifically pointing to municipal managers as susceptible to politicization (1999, 2016); and Garmann (2015) advances the argument that politics often blurs the line between manager nominations and mayoral elections. To the same effect, Burnett and Prentice (2018) provide that partisanship dictates hiring of managers at the county level, a clear nod that managers are far from apolitical.

Relationship Between Municipal Structure and Finance

Dealing directly with the question at hand, is the confluence of the two elements, municipal form and financial outcomes for municipalities. As of current, the literature stands somewhat divided on closely related direct effects. While some early research found no difference between municipal forms, more recent literature shows disparate spending behaviors between municipal forms (Lineberry and Fowler, 1967, McDonald, 2008, Coate and Knight, 2011, and Chapman and Gorina, 2012). Further, both Compton, Gore, and Glick (2017) and Eistein and Glick (2018) provide that municipal compensation is dictated by executive style and structure. With the addition of Jimenez

(2020) and Wei (2020) showing differences in fiscal conditions; a growing consensus is appearing. Several fiscal behaviors appear to differ between mayor-council and council-manager forms. As such, the budget process is impacted by the underlying structure of the government.

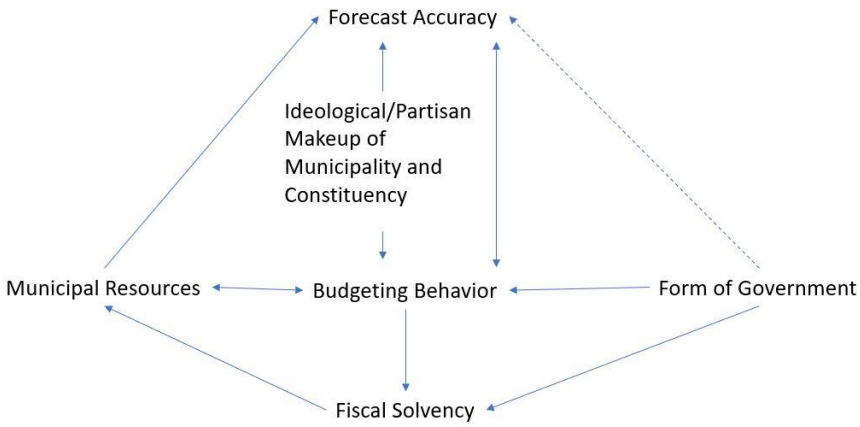
Two pieces of recent literature relate most closely to the subject at hand. Mikesell and Ross (2014) provide evidence that political pressures do, in fact make it into the budgeting process. Further they bolster the point provided by Boyd, Dadayan, and Ward (2011), Chapman and Gorina (2012), and later reaffirmed by Reitano (2018; 2019) - that elected officials often have a clear motivation to paint a rosier picture of revenues than do unelected officials. Because so many institutional factors influence forecast accuracy, it suggests that budgetary preferences may also influence the forecasting process (Bretschneider, et al., 1989; Chapman and Gorina, 2012). In sum, there may be a bicausal mirroring of Bretschneider, et al. (1988)'s position – and Forrester's (1991) restatement - that forecasts inherently influence budgetary choices. In fact, Reitano (2019) begs the question at hand by noting that elected officials in smaller local governments may express preference for - and have a greater ability to be - more judgmental and less rigorous in forecasting.

Although Mikesell and Ross (2014) discuss the implications of politics in the public budgeting process, there are two fundamental gaps between their work and the aim of this research. First, they discuss forecasting at the state level instead of the municipal levels. In this regard, the bridge between state and municipal is not large - as political ideology clearly trickles down to the municipal level (Tausanovitch, and Warshaw, 2014) and impacts municipal spending and choices (Connolly and Mason, 2016, Dynes and

Martin, 2019, Martin, Lopez, and Olsen, 2019). Second, the remedy illustrated by Mikesell and Ross (2014) is for consensus forecasting. Importantly, the ability to use consensus forecasting may be dramatically limited in a large number of municipalities where creating more than one budget oriented office is cost prohibitive.

Second in the central liturgical areas is the case presented by Jimenez (2019), where the fiscal well-being of municipalities is dependent on the structure of their governments. While Jimenez (2019) examines the impacts of form of government on municipal solvency, many of the same tenets apply here: a principal-agent theory argument is based on mayoral and managerial accountability - that incentives for managers foster forecast accuracy and that fiscal professionalism increases with a manager. The fiscal professionalism embodied by a manager suggests that a manager's mission in the forecasting process, as well as most fiscal choices is that of cautious accuracy.

Figure 14: Relationships between Form of Government and Forecast Accuracy (Proposed Relationship with Dotted Line)



Notably, an important factor in budgetary solvency is the ability to produce a budget that accurately represents actual revenues (Justice, et al., 2019). If the municipalities routinely overestimate revenue, the municipalities will face decreased reserves and suffer lower credit ratings (Marlowe, 2011). Municipalities routinely underestimate future revenues to maintain some fiscal slack (Marlowe, 2012; Williams and Onochie, 2013; Su and Hildreth, 2018), and ostensibly, solvency. The underestimation of revenues results from broader risk aversion – as above, linked to public administrators to a greater degree than it is political actors. As a result, uncovering differences in revenue forecast accuracy may provide insight into the underlying reasons for divergent fiscal outcomes based on form of governance.

In sum: Forecasts produced by knowledgeable professionals tend to prevail in accuracy against even well-trained modeling techniques (Reitano, 2019); and political pressures influence forecasting behaviors (Bretschneider and Gorr; 1989; Chapman and Gorina, 2012). Municipalities with less political influence will be expected to have more

accurate forecasts. Because council-manager municipalities are designed to distance executive authority from political opportunism (Carr, 2016), they are expected to be more accurate than those with mayor-council forms. As a result:

H1: Budget forecasts from municipalities with council-manager structures will be more accurate than those produced by municipalities with mayor-council governments.

Career administrators (as opposed to political actors) expect to have longer term rewards and motivations than political actors (Frant, 1996; Feiock, Kim, and Jeong, 2003), and they are more likely to be risk-averse (Bozeman and Kingsley; 1998). Forecasters often make less aggressive forecasts to alleviate the risk of over-forecasting revenues, reducing creditworthiness and decreasing rainy day funds (Marlowe, 2009). Because political actors tend to have shorter-term goals of political opportunism, retaining rainy day funds for after their term provides lower value. In turn, the revenue forecast that is created by a municipality expected to be less insulated from political pressures (council-manager) would have downwardly biased errors compared to that of mayor-council systems. As a result:

H2: Budget forecasts from municipalities with council-manager structures are expected to be downwardly biased compared to those municipalities with mayor-council governments.

Methods

Data

The data set used for this analysis comes from a novel combination of five different sources: municipal financial statements; municipal charters/websites, mayoral

websites; the American Ideology Project, and the Census Bureau's American Community Survey. The complete dataset consists of 495 municipal-year observations from the 100 largest municipalities in the United States.[16] Years under investigation are 2014 through 2018. While the main dataset consists of all 100 largest municipalities, restrictions to the full panel serve as robustness check sets. For example, due to population scale compared to the other municipalities , removal of the largest four municipalities) provides an additional robustness check.

The independent variable of interest is form of government, which takes the value of 1 if form of government is mayor-council and 0 if form of government is council-manager[17]. Portland, Oregon, Orlando, Florida, and Wichita, Kansas score 0 for council-manager form under the broader 'reformed' government umbrella because they employ a commission format. As a robustness check, the final model is rerun with those observations removed. Form of government was drawn from independent review of municipal charters and municipal websites.

The dependent variable which denotes forecast quality, is constructed in two forms, similar to those employed by Mikesell and Ross (2014). First is the budgeted revenue amount minus the actual revenue amount divided by the actual revenue amount, providing the percent by which the budgeted amount differs from the actual (MPE). The values indicate the degree to which municipal budgets under or overestimated the

[16] Due to irregular reporting, Newark, New Jersey is not included in the sample.
[17] This procedure is consistent with delineations by Jimenez (2020). While additional nuance may be valuable in many cases, (see Nelson and Svara (2012)) the number of elements creates unneeded complexity.

revenues for the year: measuring bias. Table 18 provides summary statistics for key variables in the dataset.

Table 18: Summary Statistics

	mean	sd	min	max
Form of Government (1=Mayor/Council)	0.51	0.5	0.0	1.0
Total Revenue MPE	3.14	9.0	-66.0	81.5
Total Revenue MAPE	4.37	8.4	0.0	81.5
Total Population	641932.29	962,571.8	212,067.0	8622698.0
Median Income	56,003.07	16,533.6	24,701.0	136,718.0
Mayoral Party (1=Dem/0=Rep)	0.66	0.5	0.0	1.0
Observations	495			

The second outcome employs the absolute value of the value derived in the first case (MAPE). The values indicate the degree to which the municipality's budget accurately predicted the actual revenues received in the following year. Since the absolute value is being employed, there is no distinction whether the values are over or under the actual values and as a result, the values are interpreted as a simple measure of accuracy.

Manucipal Annual Financial Reports serve as the source for MPE and MAPE - completed by municipalities in the interest of fulfilling the auditing requirements of the Government Accountancy Standards Board (GASB). Importantly, GASB Statement No. 41 requires that governments present budgetary comparison schedules within the Required Supplementary Information portion of the Annual Financial Reports (Statement 41 of the Governmental Accounting Standards Board). The components parts of total revenue MAPE and MPE are municipalities' total initial budgeted revenue and the final actual budgeted revenue.

Annual Financial Reports serve as the source for individual revenue streams during the period 2016 through 2018. Individual revenue streams consist of six major sub-streams: total taxes, investment earnings, license and permits, shared revenue, service charges, and fines and forfeitures. Consistency drove the selection from a diverse set of revenue sources reported by municipalities. For instance, while Philadelphia reports revenue from only 5 highly aggregated sources, Chicago reports revenues from 65 different sources. As a result, the revenue streams for Chicago summed to larger categories in the final data set. The miscellaneous category served as refuge for any revenue streams that did not fit clearly into other categories.

Several control variables serve to reduce potential omitted variable bias. First,ideology scores provided by Tausanovitch, C., and Warshaw, C. (2014) and updated under the American Ideology Project, provide scores of ideological preference for municipalities in the United States. This score serves as an updated form of the municipal party control variables used in previous work such as Chapman and Gorina (2012). Continuing to account for the tendencies of party affiliation at the municipal level (de Benedictis-Kessner and Warshaw, 2016), mayoral party is also included for each of the municipalities in the data set. Mayoral party is coded as a 1 if a mayor is associated with the democratic party, while mayors associated with the Republican Party are coded as negative 1. The use of a negative number for coding - although unorthodox – is for purposes of interactive effects and does not disturb results otherwise.

Importantly, variables for population, median income, and unemployment serve as proxies for municipal resources. The source of variables is the Census Bureau's American Community Survey one-year estimates for each municipality. The inclusion of

a these proxies for municipal resources is important as municipal resources are a key determinant of forecasting accuracy (Forrester, 1991; Reddick, 2004; Reitano, 2018).

Sampling Motivation

Selected ubject municipalities for three reasons. First, the 100 largest cities -when taken by themselves - have a relatively even distribution of form of government across size. Within the sample municipalities 51% are Mayor-Council, 44% are Council-Manager, 3% are Commission form, and 2% are hybrid structures. The second reason for using the 100 largest municipalities is the result of employing a power analysis based on information from previous literature. Using the difference of operating ratio between mayor-council and council-manager (0.992 to 1.023) found by Jimenez (2019), a sample size of 186 would be necessary to observe the same size difference at the five percent level of statistical signifcance. Using Chapman and Gorina (2012), the difference in direct municipal expenditures is 8.3% lower in council-manager forms than in mayor-council forms. Using the power calculation from Chapman and Gorina would enable the same sample size of 200 observations to show a difference at the one percent level. To account for data shortages (missing audited financial statements and uneven distribution among sample groups), provide for significant oversampling, and to maintain power across several robust check subsample analyses, the total sample aimed to obtain 500 observations.

Analytical Technique

The main model consists of a pooled ordinary least squares regression with year fixed effect to determine the association between revenue prediction error and forms of municipal government. The use of this technique is consistent with Wei, 2020, Park and Park, 2018, Nelson and Svara, 2012, Giroux and McClelland, 2003; or the limited dependent variable forms used by Feiock, Krause, and Hawkins, 2017, Connolly, 2017; Coyle McCabe, Ponomariov, and Estrada, 2018. Although the dataset is a repeated measures panel, fixed effects are not available as form of government for the municipalities are effectively time invariant during the sample period. Over the last decade only one municipal government within the largest 100 is known by the author to have changed their structure. Both a lack of variability over time and no temporal interventionresults in the selection of a non-causal, correlative model. In addition, cluster robust errors based on state are employed to account for state level effects such as accounting measures.

The equation presented here describes the structure of the model:

$$Projected.Error_{it} = MunicipalForm_{it} + Ideo_{it} + (Ideo_{it} * MayoralParty_{it}) + MayoralParty_{it} + Ln(Pop)_{it} + Income_{it} + \varepsilon_{it}$$

Of note, the form presented includes measures for mayoral party and ideological score for the municipal that are included both independently and interacted. Resulting from the structure of the two dependent variables, the interacted variable describes a relationship

whereby observations with the highest and lowest values represent the most agreement between mayoral party and municipal ideology.

Because this data set – like most municipal fiscal data sets, contain numerous and prominent outliers, the primary dependent variables are Winsorized at 98% (Nyitrai and Virag, 2019). The choice to use Windsorized models mimics the effect generated by using MdAPE - minimizing outstanding values (Bretschneider and Gorr, 1989). The intent of Windsorizing the data is to limit the clear outsized effect of a few outliers on the regression results, making sure that the results are not driven by or reduced by outliers to the data – similar to other methods, such as Cook's Distance. Non-standardized data models, with results included in the Appendices, are consistent with standardized models.

I additionally provide three alternative structures to the main model as robustness checks. First, are the models describing revenue subcategories. Because revenue streams may be subject to politically driven manipulation (see, Mughan and Nicholson-Crotty, 2020; Mughan, 2020), each revenue stream has individual results. In effect, if some particular revenue streams are politically driven, their projections may be subject to bias specific to those sources. The same model structure used for the final evaluation of total municipal revenues is applied.

Further, I attempted a two-stage instrumental variable approach. A complete description and results are included within the appendix. In the two-stage instrumental approach, while the instrument of municipal incorporation dates meets exclusion criteria, the instrument is extremely weak, providing little functional benefit (Hahn and Hausman, 2003, Stock and Yogo, 2005). As a consequence, those results are in the appendix for

inspection in lieu the main body of this article. Further, the implementation of numerous other instruments both individually, and in combination with each other: municipal incorporation date, municipal founding date, region, state admittance date, and portion of the municipality consisting of water, were similarly inadequate.

Results

The results for total revenue projection error do not appear to approach statistical significance in any of the model iterations with control: both absolute percent error and generalized precent error in the 2% and 98% Windsorized model do not show results approaching significance (the non-standardized model provides similar results in the appendix).

Table 19 provides results of the step-wise building of model for total revenue MPE, concluding with two additional robust check models: one that precludes the Commission form municipalities and a model precluding the largest municipalities. Table 20 again provides a parallel display of the results of the models with total revenue MAPE as the dependent variable. The only significant results for total revenue for the independent variable of interest are for the models without controls for municipal resources in any form.

Table 19: Summary of Empirical results (MPE)

	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se
Form of Government (1=Mayor/Council)	-0.64*	-0.64*	-0.01	0.04	0.11
	(0.38)	(0.38)	(0.39)	(0.38)	(0.40)
Ideology Score			-0.58	-3.79**	-3.32**
			(0.84)	(1.60)	(1.54)
Mayoral Party (1=Dem/0=Rep)=1			0.34	0.53	0.16
			(0.53)	(0.54)	(0.49)
LnPopulation			-0.58**	-0.56**	-0.44*
			(0.23)	(0.23)	(0.22)
LnIncome			3.70***	4.01***	3.06***
			(0.95)	(0.98)	(0.93)
Mayoral Party (1=Dem/0=Rep)=1 × Ideology Score				4.09**	2.99*
				(1.89)	(1.80)
Constant	3.09***	3.40***	-29.77***	-33.22***	-24.59**
	(0.28)	(0.49)	(10.42)	(10.68)	(10.49)
N	493.00	493.00	493.00	493.00	468.00
R-sq.	0.01	0.02	0.07	0.08	0.06
Year Fixed Effects	No	Yes	Yes	Yes	Yes
Commissions Incl.	Yes	Yes	Yes	Yes	No

* p¡0.10, ** p¡0.05, *** p¡0.01

Table 20: Summary of Empirical Results (MAPE)

	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se
Form of Government (1=Mayor/Council)	-0.61*	-0.61*	-0.30	-0.30	-0.29
	(0.36)	(0.36)	(0.39)	(0.38)	(0.41)
Ideology Score			-1.17	-0.80	-0.27
			(0.88)	(1.37)	(1.31)
Mayoral Party (1=Dem/0=Rep)=1			-0.59	-0.61	-1.04**
			(0.51)	(0.54)	(0.45)
LnPopulation			-0.41*	-0.41*	-0.28
			(0.23)	(0.22)	(0.22)
LnIncome			2.10**	2.06*	0.81
			(1.04)	(1.05)	(0.96)
Mayoral Party (1=Dem/0=Rep)=1 × Ideology Score				-0.47	-1.82
				(1.70)	(1.56)
Constant	4.06***	4.42***	-12.87	-12.48	-0.73
	(0.26)	(0.48)	(11.12)	(11.21)	(10.61)
N	493.00	493.00	493.00	493.00	468.00
R-sq.	0.01	0.01	0.04	0.04	0.04
Year Fixed Effects	No	Yes	Yes	Yes	Yes
Commissions Incl.	Yes	Yes	Yes	Yes	No

* p¡0.10, ** p¡0.05, *** p¡0.01

Importantly for the veracity of the model the controls remain consistent and statistically significant with results from prior research. For instance, population (the main proxy for municipal forecasting resources) is consistently negatively associated with error values municipal forecasts. Further, mayoral party and municipal ideologies

are significant and consistent in the non-absolute percent error model forms. Further, Figure 15 details the marginal effects of mayoral party and municipal ideology.

Figure 13: Marginal Effects of Municipal Ideology and Mayoral Party of Forecast Accuracy

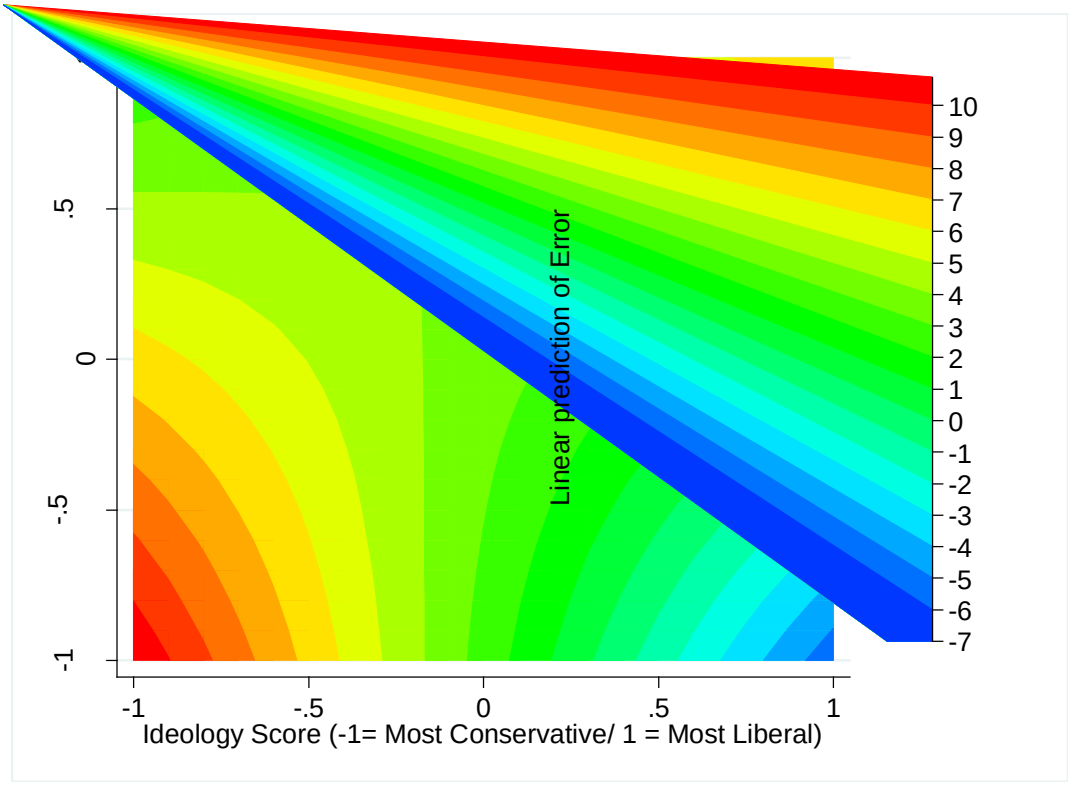


Table 21 provides results for the mean percent error model across all subcategories of revenue regularly provided by municipalities.[18] Within the main total revenue MPE category, only median income and unemployment are correlated. Concentrating on municipal form, the only subcategory holding statistical significance in the shared revenue category seeing a 9.05% lower revenue estimation in council-manager municipalities.

[18] Income tax is not provided as all municipalities within the sample employing an income tax (only 11) are Mayor-Council form.

Table 22 provides results for the MAPE model across all subcategories of revenue regularly provided by municipalities. Again, the only independent variables carrying significance on the total revenue MAPE outcome are median income and unemployment. For the outcome of interest, form of government, property taxes are .85% less accurate in council-manager municipalities, while licenses and permits are 3.11% more accurate in municipalities with council-manager municipalities.

Again, the general lack of statistical significance on the independent variable of interest stands through the non-Winsorized models (results provided in the Results Appendix), and indicates that the results are less likely subject to concerns over Type II errors.

Table 21: Revenue Subcategory Results (MPE)

	(1) Total Taxes b/se	(2) Investment Earnings b/se	(3) Licenses and Permits b/se	(4) Shared Revenues b/se	(5) Service Charges b/se	(6) Fines and Forfeits b/se
Form of Government (1=Mayor/Council)	-0.15	-1.34	-3.70*	-9.05**	0.50	3.06
	(0.53)	(9.53)	(1.99)	(3.55)	(1.61)	(3.78)
Ideology Score	0.08	-12.81	2.67	1.98	-10.57	-11.95
	(1.65)	(35.05)	(7.29)	(11.99)	(7.14)	(12.71)
Mayoral Party (1=Dem/0=Rep)=1	-0.10	-10.52	-1.53	-8.93**	5.46***	-1.48
	(0.63)	(11.67)	(2.15)	(3.73)	(1.91)	(4.15)
Mayoral Party (1=Dem/0=Rep)=1 × Ideology Score	-1.86	-16.14	-6.33	-7.05	15.47*	13.56
	(2.00)	(38.56)	(8.15)	(12.82)	(7.90)	(14.49)
LnPopulation	0.03	-2.86	2.18*	4.05*	-0.55	2.56
	(0.32)	(5.87)	(1.17)	(2.07)	(0.93)	(2.15)
LnIncome	2.03**	-18.83	-10.11***	-2.90	7.51**	10.43
	(0.93)	(17.74)	(3.38)	(5.95)	(3.45)	(6.63)
Constant	-21.20**	279.57	89.59**	-5.68	-76.10**	-151.45*
	(10.49)	(196.85)	(37.94)	(67.95)	(37.43)	(82.21)
N	183.00	179.00	187.00	184.00	186.00	178.00
R-sq.	0.07	0.03	0.07	0.09	0.08	0.04
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Commissions Incl.	Yes	Yes	Yes	Yes	Yes	Yes

* p¡0.10, ** p¡0.05, *** p¡0.01

Table 22: Revenue Subcategory Results (MAPE)

	(1) Total Taxes b/se	(2) Investment Earnings b/se	(3) Licenses and Permits b/se	(4) Shared Revenues b/se	(5) Service Charges b/se	(6) Fines and Forfeits b/se
Form of Government (1=Mayor/Council)	0.22	0.80*	0.80*	11.03	-3.34**	-4.00
	(0.49)	(0.48)	(0.48)	(7.30)	(1.53)	(4.30)
Ideology Score	2.11	0.58	0.58	-30.72	1.53	-22.49
	(1.47)	(0.86)	(0.86)	(31.12)	(4.66)	(15.02)
Mayoral Party (1=Dem/0=Rep)=1	0.33	0.63	0.63	2.98	-1.85	-14.22***
	(0.55)	(0.46)	(0.46)	(8.05)	(1.65)	(4.66)
Mayoral Party (1=Dem/0=Rep)=1 × Ideology Score	-2.09	-1.47	-1.47	33.13	-10.19*	18.14
	(1.82)	(1.30)	(1.30)	(32.73)	(5.33)	(15.79)
LnPopulation	0.13	-0.27	-0.27	-9.05**	-2.09**	1.63
	(0.27)	(0.22)	(0.22)	(4.40)	(1.02)	(2.28)
LnIncome	1.40	-0.01	-0.01	28.20**	-4.25*	0.98
	(0.90)	(0.86)	(0.86)	(13.24)	(2.53)	(6.85)
Constant	-14.31	4.45	4.45	-142.65	86.13***	-7.07
	(9.90)	(9.48)	(9.48)	(147.02)	(29.16)	(80.08)
N	183.00	129.00	129.00	179.00	187.00	184.00
R-sq.	0.03	0.11	0.11	0.07	0.09	0.09
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Commissions Incl.	Yes	Yes	Yes	Yes	Yes	Yes

* p¡0.10, ** p¡0.05, *** p¡0.01

Conclusion and Discussion

Withstanding the shortcomings outlined below, municipal form of government is not associated with the under or over estimation (MPE) of revenue forecasts. Further, the adherence of the results of the control variables to previous literature and theoretical expectation upholds the value of the models. Error scores for controls such as ideology, population, mayoral party, and income are all consistent with posited values.

An important contribution that this article is outside the original focus:The marginal results indicate that (in the upper-left and lower-right corners of Figure 13) where the mayoral party is in opposition to the ideological preferences of the municipality, revenue forecast accuracy is increased. However, where previous literature described only competition or agreement, and party control as independent objects – this figure incorporates the two components simultaneously. A compelling conclusion is that agreement between ideology and mayoral party is not equal. While a Democratic mayor in a highly democratic municipality shows some upward bias in forecasts; a Republican mayor in a highly conservative municipality shows much higher upward bias, indicating that party agreement is not equal between the two parties.

Further, there is no consistent correlation across models for form of government is in the revenue categories. The only significant measure for MPE is the shared revenue category, where the potential for correlation between state factors and alignment with form of government, the potential to be a remnant of state apparatus is high – likely the same remnants seen by Wei (2020). For example, if all large municipalities in a state such as California were mayor-council form and subsequently faced a shortfall from state revenue the correlation is unavoidable .

There are some significant results of interest for the MAPE score are on property tax, and license and permit revenue. As those results are all at the 10% level, this indicates two routes for further investigation rather than a clear conclusion. First, these revenue streams could be a product of true differentiation as the forecast for those revenues streams are the only one meaningfully different under different forms. While potentially valid, this result is unlikely. More likely, however, is the possibility that the statistical significance is a result of the multiple tests. Additionally, no outcomes would retain significance after Bonferroni corrections (Holm, 1979).

There are two significant shortcomings of this article which may influence outcomes. First is the inability to draw causal inference from available information. The use of case studies or other qualitative work may illuminate whether changes to form of government direct the quality or lack thereof forecasts. Second is the lack of information on revenue forecasting techniques. This article relies – like many others to date – on either an equal disbursement of forecasting techniques based on municipal resources or the proposition by Lawrence (2005) that individual bias far outweighs bias by technique.

Additionally, constraints of the time period limits the article ; But this is a shortcoming pervasive to all forecasting work . Inevitably the quality of the forecast is bound not just to the forecast itself, but also the ‘quality’ or ‘bias’ of the year in which it was presented. For example, if municipal revenues a struck with a particularly negative shock of which could not have been reasonably foreseen, an evaluation of accuracy will simply pull the lowest forecast and deem it the most accurate (the type expected at the outset of the Covid Pandemic). On the opposing side, a positive shock will simply show the most bullish forecast as the most accurate. As any real forecast must be judged

against its own context, it is an unavoidable shortcoming which needs to be more adequately addressed across the literature.

In addressing divergent results between those obtained by Jimenez (2020) and the finding in this article, two explanations may hold. First, the motivation for pushing forecasts towards higher bias may not be a clearly political one. Politicians looking to present more positive results may not feel forecasts provide the best forum for pushing revenues, as the politicians themselves are ultimately held in account when revenues do not match forecasts. A political actor, while not professionalized in the way a manager would be, still recognizes the potential for eventual negative outcomes if an overly rosy picture is portrayed and revenues do not match. The second reason is a clear next step in research. The combination of thisarticle and Jimenez (2020) indicate that spending patterns may be generating the results seen in Chapman and Gorina (2012).

Appendix 1: Two-stage instrumental variable approach

Description of Method

In addition to the main models, an instrumental variable approach is attached here. The two-stage instrumental model is intended to assuage concerns of potential endogeneity between the municipal government form and structural contributors such as long running cultural factors within a municipality. For instance, a municipality governed by a reform (or council-manager) municipal form may be culturally more risk adverse than municipalities with a mayor-council form. In order to extricate potential endogeneity, I attempt to make use of a two-stage design.

Important to the successful running of a two-stage model is the selection of the instrumental variable. In order to fill the exclusion requirements, the variable must be correlated with the endogenous independent regressor, while remaining unassociated with the dependent regressor. In order to fulfill those requirements, the choice of instrument was absolute value of the incorporation year of the municipality in relation to 1908. The incorporation year would be associated with governmental form because the council-manager form was not introduced until 1908 and those governments newly incorporating after 1908 had the manager council-form readily available to them. Because the manager council form became a trend shortly after 1908 (Nelson and Svara, 2008), newly incorporated governments are far more likely to be manager-council form. On the opposing side, municipalities incorporated shortly prior to the introduction of the manager form would be reluctant to change form immediately after doing it once.

Results

The results of the 2-stage instrumental variable approach are provided in appendix table 1. Importantly, while the exclusion requirement is met with a F-score of 3.20 and a probability of F at 0.07 for the MPE outcome; it should be noted that instrument does not meet even particularly minimal scores as a strong instrument. Under the Stock and Yogo (2005) test of weak instruments, the instrument (F-score of 5.53 at 25%) potentially has bias above 25%, thus nullifying any value that the 2-stage instrumental design would have over the main model designs. The F-scores for the MAPE model are identical, indicating that the same issue pervades that model.

Although the validity of the results are in question, it should be noted that the model retains a lack of significance across both main outcomes; however, the reliability also comes into question with prohibitively large standard errors.

	(1) Total Revenue MPE b/se	(2) Total Revenue MAPE b/se
Form of Government (1=Mayor/Council)	16.85	-1.22
	(15.10)	(9.61)
Ideology Score	5.61	-1.32
	(8.24)	(5.60)
Mayoral Party (1=Dem/0=Rep)=1	3.07	-0.75
	(2.55)	(1.58)
Mayoral Party (1=Dem/0=Rep)=1 × Ideology Score	7.69	-0.66
	(5.76)	(2.57)
LnPopulation	-2.04	-0.33
	(1.44)	(0.86)
LnIncome	14.90	1.47
	(10.12)	(6.67)
Constant	-139.03	-6.73
	(98.50)	(65.68)
N	493.00	493.00
R-sq.	-2.66	0.03
F-Score	3.20	3.20
F-Score p Value	0.07	0.07
Stock-Yogo Bias Level	¿ 25	¿ 25

* p¡0.10, ** p¡0.05, *** p¡0.01

Appendix Pt. 2: Robust Check Results

Non-Standardized Main Results

	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se
Form of Government (1=Mayor/Council)	-2.02**	-2.02**	-0.66	-0.53	-0.35
	(0.81)	(0.81)	(0.79)	(0.75)	(0.78)
Ideology Score			0.04	-8.28**	-6.02**
			(1.81)	(3.40)	(3.01)
Mayoral Party (1=Dem/0=Rep)=1			1.26	1.75	0.58
			(1.28)	(1.34)	(0.61)
LnPopulation			-1.06***	-1.01***	-0.51*
			(0.37)	(0.37)	(0.27)
LnIncome			6.99***	7.78***	3.43***
			(2.21)	(2.36)	(1.30)
Mayoral Party (1=Dem/0=Rep)=1 × Ideology Score				10.60***	5.58*
				(3.90)	(2.88)
Constant	4.17***	4.09***	-59.13***	-68.08***	-28.40**
	(0.70)	(1.23)	(21.40)	(22.95)	(14.13)
N	493.00	493.00	493.00	493.00	468.00
R-sq.	0.01	0.02	0.06	0.07	0.05
Year Fixed Effects	No	Yes	Yes	Yes	Yes
Commissions Incl.	Yes	Yes	Yes	Yes	No

* p¡0.10, ** p¡0.05, *** p¡0.01

	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se
Form of Government (1=Mayor/Council)	-1.46*	-1.46*	-0.12	-0.10	0.07
	(0.76)	(0.76)	(0.71)	(0.69)	(0.71)
Ideology Score			0.53	-0.85	1.41
			(1.73)	(2.94)	(2.61)
Mayoral Party (1=Dem/0=Rep)=1			-0.10	-0.02	-1.24**
			(1.24)	(1.33)	(0.52)
LnPopulation			-0.73**	-0.72**	-0.22
			(0.35)	(0.34)	(0.24)
LnIncome			5.26**	5.39**	0.95
			(2.16)	(2.30)	(1.09)
Mayoral Party (1=Dem/0=Rep)=1 × Ideology Score				1.76	-3.31
				(3.55)	(2.42)
Constant	5.11***	5.90***	-41.98**	-43.46*	-2.66
	(0.67)	(1.17)	(20.65)	(22.17)	(11.82)
N	493.00	493.00	493.00	493.00	468.00
R-sq.	0.01	0.01	0.04	0.04	0.03
Year Fixed Effects					
Commissions Incl.					

* p¡0.10, ** p¡0.05, *** p¡0.01

5: DISSERTATION CONCLUSION

In two essays of this dissertation, the results of the new investigation are not statistically significant. In the other, the introduction of particularly stringent tax and expenditure limit has almost no effect on some municipalities. However, it is far from the case that the dissertation lacks new evidence for local public budget and finance. Further, the results may intone a flavor of institutional irrelevancy, but that also is not the case.

Selznick (1951) took a negative tone in noting that institutions are extremely vulnerable to political manipulation (while also noting that politics is an institution itself); however, we can alternatively interpret the ability of institutions to cater to political will as a feature, rather than a bug. If institutions insulated themselves entirely from political intentions, they would rightly be subject to criticism and effectively ground the fears of a technocracy. Instead, institutional forms may simply set a preferred framework while maintaining the deeper social contract. The third essay, may indicate a deeper concern though, where the intentioned veil to separate politics and the administrative is willfully abridged.

While it may be a cliché for local governments, states, and even the national government to place their constituents at the top of their organizational chart, the clear dominant force in two of the three essays are constituents and the political perspective they bring. When speaking to representation of democracy in public bureaucracies; the outcomes included here provide that politics in are ever dominant. In much the same way that many interpret the Constitution as a living document, the malleability of institutions is their living element.

While I give deference to the credibility of the results I assert, as I believe they are correct in the span they encompass. I similarly do not believe that they are above reproach. At the most basic level, all essays suffer from the myopicity of positivism. While, a perfect textualization of each essay's topic may be valuable in many regards, the ability to theorize broadly is lost. With all due deference to Proust, not all is lost in the stride towards reductionism. In a nod to the satisficing nature of positivism, Moses and Knutsen (2019) draw from Shapiro (2009) "the methods of science hold out the best possibility of capturing [the buried truth's] true character."

Further elaborating, limitations to generalizability is likely the greatest concern that pervades all three papers. All three papers rely on the special circumstances of their sampling frame to provide theoretical insight. For instance, the use of an 'unbreachable' TEL in Colorado; that same sampling frame limits the ability to directly generalize to all municipalities. Similarly, the use of the 100 largest municipalities to evaluate forecasting bias allows a much greater ability to assume forecasters themselves are roughly equivalent, whereas individual forecaster capacity is more likely to be the sole dominant force in much small municipalities. In preserving the ability to provide a test of theory with increased sensitivity to non-forecaster tendencies, the ability to generalize to smaller municipalities is diminished. The trade-off in favor of theoretically based outcomes was conscious and considered beneficial, considering there are clear extensions to practice in each circumstance.

Speaking to essays individually, the first essay evaluates the impacts of a removal of an assessment restriction indicates that many previous tactics for evaluating the effects of tax and expenditure limits. Results indicate that assessment restrictions, in this

particular case, may have little to no impact on municipal solvency. Previous measurement such as binary indicators, and unvalidated scoring systems – may miss important detail. The next steps for research are two-fold: First is to either develop a validated scale or some other nuanced measurement that more appropriately classifies the nuance involved in tax and expenditure policy. Second, the results warrant investigations into the equity of these measures. In line with some of the research on Prop 13 (Sexton, et al, 1999), assessment restrictions may indicate a more defined impact on tax equity than overall revenue.

By way of the second essay, we come to understand that important heterogeneous effects result even with the uniform implementation of a tax and expenditure limit across a state. The localities that were most in favor of the tax restrictions saw the least change in revenue trends. Further, localities finding them less desirable saw the largest shift in revenue trends resulting from the restrictions. The results imply that the service preferences of municipalities who find the restrictions unfavorable may not be met by their government. The test of that measure is the clear next step for this stream of research and meeting the preferences of residents.

The third chapter concludes that municipal form of government is not associated with different levels of bias in forecasting. Given that information, we can expect that forecasting bias is not the root of disparate fiscal outcomes noted in Jimenez (2020). As a result, the list of potential underlying rationale narrows, and other avenues should (such as borrowing intentions) be closely appraised. In practice, while the potential move of Austin, Texas from a council-manager system to a mayor-council form could be

considered problematic or preferable for a number of reasons, impacts to forecasting bias would not be expected.

Municipalities are creatures of their ecosystem, culture, and regimentation. Local governments are beholden to abilities their budgets endow them. The same objects that influence budgets, influence local government, and influence governance. This dissertation evaluates new ways and nuance in which institutions do and do not influence the budgets of local governments. Through the evidence provided by this research, local governments and their states have more information at their disposal for policy choices that they make. In turn, those governments are better able to suit the needs of their populations.

It may be enticing to frame this dissertation as a confirmation of institutional irrelevancy; and in a traditional sense it may be. The institutions of focus in the Second and Fourth Chapters are without statistical impact, and the extremely constraints applied in Chapter 3 only change outcomes in certain jurisdictions. However, Chapter 2 delineates a particularly weak institutional form from other forms have shown strong influence; Chapter 3 shows considerable influence on the local governments with the strongest preference for tax increases; and Chapter 4 shows the subsuming of one institution by the politics that engulfs it. Rather than pushing institutional irrelevance, it instead narrows the scope and provides nuance to where institutions are expected to be dominant forms.

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