

# Tax-Expenditure Limitations and Special-District Finance in Metropolitan Areas

*Working Paper*

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## **Abstract**

*In this study, I empirically investigate how tax and expenditure limitations are bypassed via special-district financing. The study exploits change in TEL provisions over decades to show how they invigorate financing arrangements through special districts: TELs motivate reliance on general user charges, intergovernmental transfers and, even, property taxes via special districts. I discuss the political and administrative dilemmas involved in circumventing the spirit of tax and expenditure limitations—originating from direct democracy initiatives—via special-district financing. I additionally take advantage of this opportunity to discuss threats to validity in TELs studies as well as recent strategies employed to address those threats.*

**KEY WORDS:** Tax and expenditure limitations, special districts, direct democracy

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# **Tax-Expenditure Limitation Initiatives and Special-District Financing**

## **1. Introduction**

This study empirically investigates how tax and expenditure limitations (TEs) have influenced special-district financing in the United States. While prior studies have documented the interrelationship between TEs and special district formation, I extend this literature by examining more specifically how TEs influence the revenue structure of special districts. Some literature, for example, suggests that special districts have been primarily intended as means for taking advantage of tax-exempt financing through the municipal bond market—a mechanism that has been severely constrained to municipalities due to fiscal limits. I document that TEs effectively motivate reliance on special-district financing. Special districts, consequently, have become allied institutions to general-purpose governments by often supplying mechanisms of finance that otherwise would not be available to governments under hard budget constraints.

An extensive literature documents a variety of effects of tax and expenditure limitations on the finance structure of subnational governments of the United States. In particular, TE initiatives have been intended to constrain the size of U.S. state and local governments by placing limits to their taxing and spending powers. Previous studies indeed show that TEs have effectively reduced local governments' reliance on traditional taxation—e.g. the property tax—while encouraging alternative budgetary arrangements such as user fees, charges, and miscellaneous revenues. While researchers may hold different views on the normative implications of TEs, they increasingly agree on the fact that TEs alter the revenue structures of U.S. subnational governments.

The TEs literature has primarily emphasized the direct effects of these limits on states and municipalities. An emergent strand, on the other hand, is suggesting that TEs not only affect revenue arrangements of these traditional governments. Rather, TEs seem to influence the institutional structure of U.S. fiscal federalism by shifting policy authority to other levels of government. Notably, TEs appear to be invigorating the creation of entire government jurisdictions, seemingly intended to circumvent the spirit of fiscal limitations. I extend this literature by investigating the effect of TEs on special-district financing in the United States. I assemble and investigate a multi-decade panel of TEs and special-district finances since 1970. I examine these my data through a range of alternative identifying assumptions.

The present study is organized as follows. In section 2, I take stock of the recent literature on special-purpose districts in the United States. In section 3, I describe the research design and data; and I introduce exploratory findings. In section 4, I develop an instrumental variables strategy in panel data to address threats to validity in estimation of TEs effects. I finally provide a range of sensitivity checks. In section 5, I outline conclusions.

## **2. Why Should TEs Influence Special-district Financing?**

In this section, I review major topics from the literature on special districts in the United States. I aim to highlight issues that have received attention from earlier studies with an eye toward areas that remain understudied. My discussion is necessarily selective and lay the ground toward the subsequent empirical analysis—the focus of this paper. I therefore do not pretend to develop here an exhaustive literature review on special districts, but rather a selective discussion of topics with an eye toward areas that remain to be investigated.

Even though special-purpose districts have a long history in the United States, their proliferation is a relatively recent phenomenon. Researchers particularly suggest that special districts have been growing since at least the Great Depression, when various fiscal limitations were introduced. Indeed, some tax and expenditure limitation initiatives were successfully established in the aftermath of the Great Depression, which have mostly lasted to the present. The most recent wave of TE initiatives, therefore, have largely strengthened—rather than created from scratch—the budgetary constraints on U.S. state and local governments.

An emergent literature documents the growth of special-purpose jurisdictions throughout the United States in the second half of the twentieth century. A range of empirical studies show, however, that special districts have proliferated since Proposition 13, when California voters introduced the first contemporary form of tax-and-expenditure limitation and similar initiatives followed in other states. This literature therefore brings suggestive evidence that special-purpose jurisdictions arise from circumvention attempts on the part of residents and political officials. Cross-local data, on the other hand, shows that formation of special districts varies widely in the states. Increasingly, researchers point to positive correlations among TELs severity and special-district formation.

Although raising suggestive evidence regarding the effect of TELs on special-district formation, the literature provides partial explanations about specific mechanisms in the TEL-districts effects. If special districts are created to circumvent tax-and-expenditure limitations, what specific mechanisms of finance do they supply? Motivated by this question, the present study investigates, specifically, the effect of TELs on revenue choices of special districts. This study focus on why and how TELs motivate special-district financing arrangements. In the empirical research that follows, consequently, I examine how distinct lines of revenue and debt are affected by the severity of TELs on traditional local governments.

Although suggesting positive correlations among TELs and special district formation, the literature is also not extensive on incentives involved in such incorporation processes. As previous research implies, special districts are fundamentally fiscal arrangements to circumvent tax-and-expenditure limits on traditional local governments. Still, we know little about how special districts specifically supply financial mechanisms to support public service provision. There is an unresolved debate in the literature, in particular, on whether special districts merely replace traditional taxation lost by municipalities and counties or, more intriguingly, whether special districts further exploit nontraditional fiscal sources such as intergovernmental aid and long-term debt.

Loss of democratic and budgetary accountability to special districts is thus raised in some recent literature, for example Berry (2009). He argues that special districts are created to exploit the “fiscal commons” and introduces some evidence regarding a positive correlation of special-district overlap on higher local taxation. Berry’s major warning, however, relates to the loss of political legitimacy due to special-district proliferation. He suggests, for example, that special districts are pervaded with low visibility and participation, for example voting, that in turn allows for capture of special-district politics by interest groups. TEL supporters seemingly did not foresee these issues when advancing TEL initiatives. Nonetheless, public debate on TELs now may benefit from consideration of the various unintended effects of such initiatives, not only on general-purpose governments, but also on other institutions of U.S. fiscal federalism.

An obstacle to empirical research on the effects of tax-and-expenditure limits is the use of different measures for TELs employed by different researchers. Earlier empirical studies used indicator (dummy) variables to proxy for the presence or absence of tax-and-expenditure limits. To overcome this restriction, Lindsay Amiel et al. (2009) document the most ambitious effort to date toward developing a TEL index for state and local governments. While this index may certainly be subject to future refinements, it advances the quality of TEL research for several reasons. First, the index is an ordinal variable that expand the measurement range of TELs severity beyond dummy variables. Second, the index is longitudinal as it measures change in TELs severity since 1969.

### ***Fiscal Rules in U.S. Federalism***

Tax-and-expenditure limitations are one set of rules for local government budgeting. In U.S. federalism, local governments, including special-purpose ones, derive their policy authority from state delegation. Still, there is great variety in the degree of policy delegation across the states. While some states give much leeway to their local governments, other states constrain their local governments in various policy areas. Fiscal rules, including TELs, are one set of rules characterizing the trust and deference from state deference to local autonomy.

Since at least the progressive movement, delegating powers from state to local governments has been an increasingly popular doctrine. States, consequently, have expanded the leeway local governments

can exercise in some policy areas. Several examples come to mind, including zoning, economic development, interlocal agreements, and alternative service delivery. Similarly, states have eased procedures for incorporation of local governments; in fact, this is one way to explain the proliferation of special districts in recent decades. Easier incorporation procedures, on the other hand, have counterbalanced stricter procedures for jurisdictional consolidation and annexation of unincorporated areas.

Fiscal autonomy is a key cornerstone of American local government. The ability to design tax-and-expenditure packages tailored to resident preferences is critical for several reasons. First, in the context of municipal fragmentation in metropolitan areas, taxes and expenditures play more than a fiscal role only. Tax-and-expenditure packages help, particularly, to internalize externalities associated with public goods provision. At the same time, fiscal policies play a role circumscribed to the jurisdictional configuration of municipalities. Second, and complementarily, taxes and expenditures can help municipalities to cooperate in solving region-wide policy problems. In theory, municipalities may also coordinate tax-and-expenditure choices in a way to minimize fiscal distortions across their jurisdictions.

Tax-and-expenditure limitations, however, alter local government roles. TELs affect especially roles that depend on reliable financing sources, such as property taxes. Furthermore, researchers argue that tax-and-expenditure limitations affect governments' ability to support infrastructure and other capital assets. Since those assets are linked to stable financing sources over time, TELs circumscribe capital projects and improvements. More generally, tax-and-expenditure limitations place a budget constraint onto localities which, otherwise, would be willing to expand government services.

Special-purpose districts, consequently, emerge as mechanisms to circumvent the constraining effects of tax-and-expenditure limitations. Although special districts have a long history, their popularity is recent and comes hand in hand with increasing TEL severity in the states. Special districts provide an increasing variety of services, most of them tailored to specific constituencies. As a consequence, special districts help those constituencies to circumvent, not only the budget constraint itself, but also policy decision-making out from city (or county) politics. It is in this respect that researchers point out the biases that could derive from shifting government roles to special-purpose jurisdictions.

### ***Revenue Structure in Special Districts***

Once incorporated, special districts are confronted to revenue choices. Like general-purpose governments, special-purpose districts can rely on a variety of revenue sources as circumscribed by law. Some states allow special districts to impose taxes and other traditional revenue sources. Nevertheless, special districts also take advantage of intergovernmental transfers as well as debt, especially when those districts are explicitly incorporated to provide for long-lived assets. Thus, similar revenue instruments are often available to special districts to those used by general-purpose governments. Special-district finance, however, differ from traditional municipal finance as a consequence of the restrictions placed on districts to support specific services. In contrast, general-purpose governments can allocate revenues to multiple ends through the general budget process.

The circumvention thesis suggests that localities get around tax-and-expenditure limitations via special districts, which, once incorporated, may enlarge fiscal burdens. In contrast to general-purpose governments, however, special-purpose districts lack the legal and political leeway to levy multiple taxes. Consequently, I should observe in practice that special districts raise revenues from specific sources more than others. Since they are incorporated to provide for specific functional services, one would expect special districts to rely on user charges and fees tied to those services. Researchers note, however, that special districts indeed take advantage of traditional revenues, such as the property tax. Since both general and special-purpose jurisdictions compete for revenues from the same fiscal base, scholars have expressed concerns regarding a potential "overfishing" effect on the local economy.

Although state law determines their taxing authority, special districts configure their revenue structure influenced by economic factors. Primarily, the size of a special district influences its revenue structure. Like other jurisdictions, size implies different fixed costs and scale economies. One should find different revenue structures in metropolitan-area districts compared, for example, to small districts.

Interestingly, scale factors may imply that metropolitan districts can choose among multiple revenue sources, while small districts are circumscribed to certain revenue sources, such as the traditional property tax. Tax-and-expenditure limitations should further shape revenue choices by asymmetrically affecting localities of uneven fiscal capacities (Mullins 2004).

Special districts providing business-like services may primarily finance their operations on user fees and charges. Special districts incorporated to supply utility services is one example. This form of special district is less controversial in policy debate because business-like districts accomplish a couple of useful purposes. First, these districts match service benefits to tax burdens by charging users based on service consumption levels. Business-like districts internalize costs and benefits of service provision and minimize externalities. Second, this form of special district performs service provision over large areas. Consequently, business-like districts may help localities to overcome municipal fragmentation by incorporating neighboring municipal jurisdictions into a common service district. This is a partial yet workable solution to specific problems derived from municipal fragmentation and competition in metropolitan areas.

Special districts are more controversial when they rely on traditional taxes and debt. By funding their operations on the property tax, for example, these districts more obviously circumvent the spirit of tax-and-expenditure limitations. TEL initiatives have historically targeted the property tax and, in theory, TEL provisions aim to circumscribe redistribution of local resources through this tax. By relying on the property tax, special districts externalize the costs of services that, otherwise, benefit specific consumers rather than an entire population. On the other hand, the impact of special-district debt depends on the uses and funding scheme tied to that debt. If debt matches the useful life of assets, special-district debt indeed benefit localities over that life time. If debt is supported by charges or restricted revenues rather than general taxes, special-district debt further benefit localities without creating externalities.

Finally, special districts often rely on intergovernmental transfers from both higher-level governments and other local governments. Special districts should naturally be supported by transfers insofar districts are incorporated to serve multiple municipalities and, perhaps, county areas. Higher-level and local transfers to special districts therefore are embedded to their originating purposes, that is, to take advantage of region-wide scale in service provision. Local transfers, therefore, may be part of the interlocal contracting process leading to special district formation. Federal and state transfers may further complement the revenue structure of special districts and, sometimes, become a sizable revenue component. In this latter case, special districts are additionally incorporated to take advantage of “free” resources from other government levels in U.S. fiscal federalism.

### ***Direct Democracy v. Administrative Autonomy***

In theory, direct democracy enhances the quality of democracy by placing policy decision-making on the hands of citizens. In practice, direct democracy is an imperfect institution that conflicts with other political values. For instance, some authors argue that direct democracy is subject to capture by special interests, which have the incentives and resources to exploit the initiative process in their favor. In relation to the subject of this study, direct democracy measures often express populist sentiments that, ultimately, may result in unintended consequences. Tax-and-expenditure limitations illustrate these contradictions by, on one hand, limiting the role of state and local governments and, on the other, motivating circumvention via nontraditional public finance schemes.

Though why direct democracy institutions varies across the states is not well understood (Matsusaka 2005), we understand better that direct democracy matters in terms of shaping public policy outcomes. Direct democracy states have effectively imposed more severe versions of tax-and-expenditure limitations. In these states, both voters and political actors have exploited the initiative process to successfully impose more stringent fiscal limitations. While voters often aim to stem redistributive policies, political entrepreneurs take advantage of TEL initiatives to advance additional agendas. It is not coincidence therefore that TEL initiatives are frequently promoted by organized interests.

Direct democracy conflicts with administrative autonomy because ballot initiatives are generally intended as constraints on policy discretion. Even beyond fiscal limitations, initiatives take over policy

making at the expense of public officials. Whether ballot initiatives or administrative autonomy is more conducive to better democracy possibly depends on the specifics of each policy on debate. At an intuitive level, however, ballot initiatives expand the depth of democracy simply by engaging the citizenry into the policy process. In contrast, some authors defend administrative autonomy on the grounds of specialization and knowhow in policy development.

Special districts illustrate the conflict between majoritarian democracy and self-governance. They arise, in part, to overcome fiscal constraints imposed by majorities. Special district allow for public service provision that, otherwise, would not be attainable under hard-budget constraints. Special-district financing removes fiscal policy from general-purpose governments and, in turn, may favor specific interests rather than entire populations. To some authors, special districts are conducive to better government in the sense of circumscribing the costs and benefits of public services (Wallis & Weingast 2008). To others, special districts exploit the fiscal commons by favoring specific interests (Berry 2008). In this context, tax-and-expenditure limitations play a crucial role in altering incentives and motivating reliance on such alternative forms of governance.

### ***Economies of Scale v. Public Goods Specificity***

The incorporation of special-purpose districts in place of, or as complements to, general-purpose governments raises the question of what the scope of special districts should be. In practice, there is great variety in both the jurisdictional and functional scope of special districts. The jurisdictional size of a special district varies dramatically from small, neighborhood-sized districts to large, metropolitan districts encompassing multiple counties. Small districts, on the one hand, are complementary organizations to neighborhood governance. This is one reason, for example, for the use of special districts by homeowner associations (McCabe & Tao 2006). In this instance, special districts typically help neighborhoods to provide for infrastructure assets by taking advantage of tax-exempt financing through the municipal bond market.

Instances of incorporation of special districts overlapping neighborhood associations may be more frequent than districts overlapping municipalities (Mallet 2006). In fact, the governing practices of special districts largely resemble those of existing neighborhood associations. As allowed by state laws, special purpose districts may rely on voting rights according to shares of property ownership (De Young 1982). Therefore, special districts have naturally become complementary institutions to neighborhood associations. Special districts may be particularly attractive for associations within established cities, where the alternative of adjusting municipal boundaries is nonexistent.

On the other hand, regional and multi county districts are incorporated to exploit economies of scale in service provision. In metropolitan areas, voters and public officials become aware of the potential benefits of scale resulting from coordinating service provision through a large district. Since there is less interest in consolidating city boundaries, due to the uncertainties implied by city (or county) consolidation, special districts become a politically palatable alternative. There, regional special districts are purposely designed to accommodate multiple municipal jurisdictions. This form of special district is increasingly popular for mass transportation, utilities, and other services where scale economies are potentially large.

By functional specialization, the variety of special districts is vast. Function of special districts ranges from basic service districts to multi-function districts. Basic district types are typically intended for neighborhood goods and their maintenance. Multi-function districts encompass a range of services that mimic that of general-purpose governments, even though districts are in principle circumscribed to specific functions. Tax-and-expenditure limitations curtail general-purpose governments' ability to take advantage of functional scope economies; localities may recover some of those benefits via incorporating a special district. Intergovernmental transfers to special districts can further expand the range of services provided by special districts, in spite of budget constraints.

At the local and regional levels, the scope of special-purpose jurisdictions is on debate. The principles of scale and subsidiarity are weighted against accountability and representation in multi-level government organization (Liesbet & Marks 2003). Special districts increasingly coordinate regional

policies and help to overcome municipal fragmentation (Carr & Feiock 2004). The jurisdictional scope of special districts is, however, influenced by both political and economic factors. Large jurisdictions economize on fixed costs in public service provision, yet large jurisdictions may recreate redistributive conflict typically associated with municipal jurisdictions. Overall, scale and scope economies are key factors influencing special-purpose jurisdictions in the context of budget constraints on general-purpose governments.

### 3. The Data and Preliminary Findings

To assess the TELs effect on special-district financing, I first estimate the following empirical model,

$$y_{it} = \beta x_{it} + \boldsymbol{\theta}' \mathbf{z}_{it} + c_i + d_t + \varepsilon_{it} \quad (1)$$

In equation 1,  $y_{it}$  denotes the dependent variable(s) of interest,  $x_{it}$  denotes a measure of tax-and-expenditure limitations,  $\mathbf{z}_{it}$  denotes a vector of control variables,  $c_i$  denotes individual-fixed effects,  $d_t$  denotes time-fixed effects, and  $\varepsilon_{it}$  denotes remaining errors. The subscripts  $i$  and  $t$  denote cross-section and time observations respectively. The coefficient of interest is  $\beta$ , which represents the effect of TELs on the dependent variables;  $\boldsymbol{\theta}$  summarizes effects from control variables. Equation 1 implies that I may consistently estimate the coefficient of interest by applying fixed-effects OLS, as long as the TELs measure is strictly exogenous to the error term (Wooldridge 2010, p. 301). Thus, I first compute estimates from fixed-effects OLS and, in subsequent sections, I allow for—and propose solutions to—possible violations of the exogeneity assumption.

#### Data

To operationalize the empirical model in equation 1, I need measures for 1) special-district finances, 2) tax-and-expenditure limitations, and 3) additional controls. Table 1 provides a detailed description of the data set assembled correspondingly. To examine the effect of tax-and-expenditure limitations over decades, I collect panel data for special-district finances since 1970. Following recent research, I sort these data by county (and county-equivalent) areas as defined by the U.S. Census of Governments. Although one would like to ideally analyze special districts themselves as units of observation, there are no available disaggregated data for special districts in the United States. This has been an obstacle to empirical studies, which have consequently analyzed special-district data aggregated, for example, at the state level. To overcome this obstacle as far as possible, I have assembled a data set for county (and county-equivalent) areas—the most disaggregation possible based on historical and current U.S. census archives.

<Table 1>

The dependent variables of interest include taxes, user charges, intergovernmental transfers, and debt as described in table 1. I am interested in assessing how TELs affect different mechanisms for current and over-time financing through special districts. In this respect, the preceding literature provides limited evidence on whether special districts are created for the purposes of direct taxation, user-based revenues, intergovernmental transfers, or tax-exempt financing associated with long-term debt. I consequently collect data on the aforementioned lines of special-district finance from the historical archives of the U.S. Census of Governments of 1972, 1982, 1992, and 2002.

The second key data source is the TEL index developed by Lindsay Amiel et al. (2009). The major advantage of this index is its longitudinal structure, as it accounts for the varying severity of TEL provisions since 1969. Additionally, this index aims to go beyond traditional proxies for TELs—dummy variables—used in earlier empirical studies. The index developed by Amiel et al. is, to my knowledge, the most ambitious effort to date toward measuring TELs through a longitudinal index.

Additionally, I incorporate control variables based on the U.S. Census of Population and Housing of 1970, 1980, 1990, and 2000. I control for population, density, median family income, minors (aged 18

and under), seniors (aged 65 and over), college graduates, ethnic diversity, poverty, homeownership, and unemployment. These measures control for socioeconomic factors that may be correlated to the dependent variables of interest. Although my focus is the effect of TELs on special-district finances, I will briefly discuss relevant results related to these control variables.

### *Preliminary Findings*

In table 2, I report summary statistics for the data analyzed in subsequent sections. In line with earlier research, the outcomes of interest—special-district finances per capita—grow over decades. For example, there is a substantial increase in average general charges from about 10 to 136 dollars per capita, an increase that is even masked by the wide dispersion of the data. As of 2002 for instance, general charges range from a minimum of 0 to a maximum of about 20 dollars per capita. There is also a sharp increase in federal and state-intergovernmental transfers to special districts. Other special-district revenues also grow considerably if we recall that these revenues are adjusted to constant dollars and the current population.

<Table 2>

In table 3, I present preliminary findings regarding the effect of tax-and-expenditure limits on special-district finances. I estimate coefficients from two-way fixed-effects OLS, that is, controlling for both time and individual-fixed effects. The TELs effect is generally positive and statistically significant for property taxes, sales taxes, and state-intergovernmental transfers per capita. These first results, on the other hand, suggest that TELs may not have an effect on debt levels in special-purpose districts. According to these first results, consequently, special districts help to circumvent tax-and-expenditure limits primarily through own source revenues.

<Table 3>

I additionally incorporate several population and socioeconomic measures. Because they are intended as controls, I do not provide an extensive discussion of their effects. I briefly point, however, to some interesting results that suggest additional causal mechanisms for the use of special-purpose districts. For example, median family income is positively related to various special-district finances, such as intergovernmental transfers. Similarly, the percentage of college-educated residents is positively related to some special-district revenues. These results suggest that wealthier jurisdictions are particularly prone to using special districts for public service provision.

Additionally, I find that poverty is positively related to special-district revenues, particularly from own source. I also find that ethnic diversity is positively related to various special-district revenues. This latter result may be associated with the use of special districts in ethnically diverse metropolitan areas, where jurisdictional boundaries often follow the ethnic and racial configuration of local communities (Alesina et al. 2004). I interpret the positive effect of income and ethnic diversity as a consequence of fragmentation of local government jurisdictions, particularly in metropolitan areas.

## **4. Causal Inference**

Although the previous results provide a big picture of the effect of tax-and-expenditure limitations on special-district financing, they may not be necessarily interpreted as validating causal effects. More formally, if the TELs index is correlated to the error term in equation 1, the coefficients reported in the preceding section can be biased. Why should the TELs index lead to biased estimates in fixed-effects regressions? Even though Amiel et al.'s index notably improves on earlier efforts toward measuring tax-and-expenditure limitations, this index may still be subject to endogeneity threats due to several reasons.

First, TEL measures may be endogenous to fiscal outcomes due to simultaneity. Recent papers are increasingly aware of this issue (Shadbeigian 1999; Sun 2012). If voters from high tax-spending jurisdictions are more inclined to support TEL initiatives, regression estimates might result in counterintuitively positive correlations of high tax-expenditure burdens *and* more severe TEL provisions.

At a minimum, this simultaneity threat may bias TEL coefficients downward, even when finding statistically significant estimates.

Second, TEL measures may be endogenous to fiscal outcomes due to unobserved, omitted factors. Fixed-effects methods in panel data (partially) address this problem by controlling for constant, unobserved heterogeneity. Nonetheless, the most important omitted variables in multi-decade panels are not only constant but rather time variant. Unless relevant time-varying factors are modeled and controlled for, we may not conclude that regression estimates of TEL effects represent causal effects.

Third, and perhaps most crucially, TEL measures may be endogenous in regression analyses due to measurement error. Recent papers recognize endogeneity threats due to simultaneity among TELs and fiscal outcomes, yet they pay less attention to problems of time-varying omitted factors and measurement error. This latter threat is potentially the most serious, as earlier studies relied on dummy variables to proxy for the severity of tax-and-expenditure limitations. Amiel et al.'s index is arguably the most ambitious effort to date toward developing a TELs index that account for the changing severity of tax-and-expenditure limitations. Although Amiel et al.'s TELs index (2006) is a significant contribution, we should be aware that the index may still be subject to measurement error.

In short, all three classical sources of endogeneity may threaten the validity of conclusions from empirical studies regarding the effects of tax-and-expenditure limitations on U.S. subnational governments. The major issue of measurement error has been ameliorated thanks to Amiel et al.'s contribution and efforts by colleagues who have opportunely pointed out the potential sources of endogeneity in empirical studies on tax-and-expenditure limits. The present study builds on and extends this literature by further developing an instrumental variables strategy to assess TEL effects on U.S. special-district financing. The logic of an instrumental variables strategy is to bring measures that are correlated to the endogenous explanatory variable but exogenous to the error term in equation 1. An instrumental variables strategy thus involves two stages. In the first stage, the endogenous explanatory variable is regressed on the exogenous instruments. In the second stage, the predicted explanatory variable from the first stage is used in second-stage regressions. Formally,

$$x_{it}^* = W'_{it}\Omega + A'z_{it} + c_i + d_t \quad (2.1)$$

$$y_{it} = \beta x_{it}^* + \theta'z_{it} + c_i + d_t + \varepsilon_{it} \quad (2.2)$$

Equation 2.1 represents the first-stage regression of TELs on all instruments, and equation 2.2 represents the second-stage regression of outcomes of interest on predicted TELs and control variables. In the first-stage,  $W'_{it}$  represents the external instruments and  $x_{it}^*$  represents predicted TELs. In the second stage, I use predicted TELs to arrive at consistent estimates. This two-stage procedure implies that I need plausibly exogenous instruments for the TELs index.

I build on earlier efforts by colleagues who have suggested, in particular, the use of direct democracy initiatives as instruments for tax-and-expenditure limitations (Shadbegian 1999; Sun 2012). I use two measures: the number of initiatives and their approval rate across the states and over time. While an instrumental variables strategy can be crucial to uncover causal effects (Angrist et al. 1996), such strategy needs to be defended both substantively—that is, conceptually justifying the exogeneity of the instruments—as well as econometrically—that is, testing for the excludability of the instruments based on the data. I examine these two aspects in turn.

At the conceptual level, direct democracy initiatives should be plausibly exogenous to special-district financing to be useful instruments. In particular, direct democracy initiatives may be correlated to special-district financing via TELs provisions only, but not through other channels. Several authors, for example Shadbegian (1999) and Sun (2012), defend the validity of direct democracy initiatives as instruments on the grounds that they involve state-wide initiatives on a multiplicity of policy issues. In other words, more severe TEL provisions should be a byproduct of waves of initiatives in direct democracy states, but they should not be directly influence special-district financing. I thus build on this

logic; and I add that direct democracy initiatives are influenced by public opinion mood, at the national level, that can be accepted as exogenous to special-district financing.

At the empirical level, I test for excludability of the instruments, which is possible when having at least two instruments. I directly pursue this possibility by testing for overidentification through fixed-effect two-stage least squares (Wooldridge 2010). The logic behind these overidentification tests is that different instruments should help identify the same causal effect of interest, that is, they should not lead to substantively different results. I therefore investigate whether the two instruments indeed yield similar effects.

The data source for the instruments is the historical database on the number and approval rate of initiatives compiled by the USC Initiative & Referendum Institute (IRI). This database lists all state-wide initiatives appearing on the ballot since the first initiative in the state of Oregon in 1904. I collect data from 1960 to 2000 to operationalize the instrumental variables strategy. The two measures—number of initiatives and their approval rate—also give us two alternative instruments to identify and assess the effects of interest.

### ***Interrelationship between Initiatives and TELs***

An instrumental variable strategy is plausible based on two factors. First, the instruments should be (partially) correlated to the explanatory variable of interest—tax and expenditure limits—in first-stage regressions. Second, the instruments should be uncorrelated to the error term in second-stage regressions. In this section, I assess the first requirement and leave further discussion of the second requirement to later sections.

Table 4 reports first-stage regressions of the TELs index on the two direct democracy measures—the number of initiatives and their average approval rate. These first-stage regression incorporates the full set of control variables (Wooldridge 2010). There is a strong first-stage as implied by the significance level (lower than 0.001) of the  $F$  tests. Both the number of initiatives and their approval rate are positively related to the TELs index and are statistically significant at the 0.001 level. In short, the first stage suggests that both initiative measures may be valid instruments to the extent that are correlated to the TELs index, even after controlling for other relevant covariates.

<Table 4>

Column 1 shows that the number of initiatives is indeed correlated to the TEL index, even after controlling for other factors. Column 2 shows that the approval rate of initiatives is also correlated to the TEL index. Column 3 shows a horse race where the TELs index is regressed on both the number of initiatives and the approval rate of initiatives as well as other on control variables. In short, all three regressions show that direct democracy measures are (partially) correlated to the TELs index, whether included separately or jointly into the regressions.

The first-stage results suggest that either the number of initiatives or the approval rate of initiatives may be valid instruments for tax-and-expenditure limitations. Nonetheless, I will primarily rely on the number of initiatives since it is the strongest instrument as implied by the  $F$  statistic (496.95) in column 1. I do use, however, both initiative measures to check for overidentification, that is, whether using different instruments lead to (potentially) different results on second-stage regressions.

### ***Main Results***

Table 5 reports results from second-stage regressions using the number of initiatives as instrument. Column 2 reports results for property tax per capita as dependent variable. The TELs index has a positive effect of 0.192 on property tax per capita, which is statistically significant at the 0.01 level. This partial effect is larger than the preliminary estimate from fixed-effects OLS. The IV result therefore suggests fixed-effects OLS estimates were likely biased downward. Such bias, to some extent, might have resulted from simultaneity; nonetheless, another source of bias may be measurement error. Although measurement error is ameliorated by using a TEL index rather than dummy variables, any left measurement error can be aggravated by fixed-effects transformations (Griliches & Hausman 1986). The

effect of TELs on sales tax per capita is no longer statistically significant compared to fixed-effects OLS, as column 3 shows.

<Table 5>

Column 4 identifies a positive effect of TELs on federal-intergovernmental transfers per capita that was not statistically significant by fixed-effects OLS. This implies that TELs invigorate transfers to special districts, a finding that is line with the circumvention hypothesis. Column 5 similarly shows that TELs have a positive effect on state-intergovernmental transfers, an effect that becomes stronger through fixed-effects IV estimation. I do not find evidence, on the other hand, regarding the effect of TELs on local-intergovernmental transfers.

Column 7 reports IV estimates for general charges per capita as dependent variable. I confirm the positive effect of TELs on general charges, and this effect becomes stronger by fixed-effects IV estimation. This estimate implies that, by increasing TEL severity from the lowest to the highest measure in the sample, general charges per capita increase by almost 50 percent. This is the largest effect among all dependent variables examined. Such an effect suggest that tax-and-expenditure limitations primarily encourage reliance on user charges for a variety of special-district services. Similarly in column 8, the effect of TELs on utility charges per capita is now positive and statistically significant, as compared to preliminary fixed-effect OLS estimates.

Column 9 reports fixed-effects IV results for full-faith-and-credit debt per capita. Compared to preliminary findings, I now identify that TELs have a positive, statistically significant effect on full-faith-and-credit debt. This estimate implies that, by increasing TEL severity from the lowest to the highest measure, full-faith debt increases by 11 percent. Finally, the effect of TELs on nonguaranteed debt is statistically significant but slightly negative.

### ***Sensitivity and Robustness Checks***

To assess the robustness of previous results, I use both direct democracy measures—the number and the approval rate of initiatives—in overidentification tests. Having two initiative measures means that I can check for identification of TEL effects based on alternative instruments. Consequently, I test for overidentification by incorporating both instruments into two-stage procedures and check for sensitivity by isolating key subsamples.

In table 6, I examine whether the preceding findings are sensitive to alternative identification assumptions. I find that fixed-effect IV results are generally robust to alternative assumptions, except for some coefficients discussed in turn. In panel 5.1, I re-estimate coefficients by using the sample of initiative states only. I apply the same fixed-effects IV procedure maintaining the number of initiatives as first-stage instrument. Isolating the initiative states subsample is useful for two reasons. First, my identification strategy rests upon initiatives as exogenous measures historically correlated to TELs but not (directly) correlated to special-district financing. Second, when focusing on initiative states only, similar estimates reassure us that preliminary findings are not merely a consequence of having non-initiative states along initiative states.

<Table 6>

I find that, when isolating initiative states, estimates remain similar to those using the full sample. TELs have a positive, statistically significant effect on per capita property taxes, federal intergovernmental transfers, general charges, and utility charges. In contrast, the effect of TELs loses statistical significance for state-intergovernmental transfers and long-term debt per capita. Interestingly, the effect of TELs becomes now positive and statistically significant for local-intergovernmental transfers per capita. Finally, the effect of TELs on sales tax per capita is not significant, which is consistent with preliminary findings.

In panel 5.2, I re-estimate coefficients by using the sample of metropolitan areas only. It is useful to assess the effect of TELs on metropolitan versus non-metropolitan counties since TELs may have asymmetric effects based on distinct socioeconomic structures in U.S. counties (Mullins 2004). In line

with this reasoning, I find that estimates differ when isolating these two different subsamples. Notably, the effect of TELs on property taxes per capita is not significant for metropolitan counties, while such effect remains significant for non-metropolitan counties. These opposing results show that TELs indeed have asymmetric effects: while metropolitan jurisdictions may be able to circumvent TELs through alternative revenues, non-metropolitan jurisdictions—possibly due to lower scale economies—need to rely on property taxes raised by special districts.

In contrast, these subsample analyses suggest that metropolitan jurisdictions are more likely to rely on federal intergovernmental transfers, while non-metropolitan jurisdictions are more likely to rely on state-intergovernmental transfers. These contrasting results suggest that intergovernmental transfers play complementary roles: federal aid seemingly favors densely populated areas, whereas state aid assists small and rural localities. Local-intergovernmental transfers per capita are significant only for metropolitan counties, which may result from greater interlocal fiscal arrangements among governments in metropolitan areas.

I also find that TELs have a positive, statistically significant effect on general charges per capita for both metropolitan and non-metropolitan jurisdictions. Similarly, TELs have a positive, statistically significant effect on utility charges per capita for both subsamples. Interestingly, general charges seem to play a greater role for non-metropolitan counties, while utility charges play a greater role for metropolitan counties. Finally, neither sales tax nor long-term debt appear to be significant financing sources via special districts as a result of tax-and-expenditure limitations.

## 5. Conclusions

The present study documents how tax-and-expenditure limitations are bypassed via special-district financing. I extend the preceding literature, in particular, by investigating the effect of TELs on special-district financing. A growing literature suggests that the formation of special districts could be largely explained as an unintended consequence of severity of tax-and-expenditure limitations. Voting citizens and political actors are increasingly relying on financing schemes through special districts as a means to alleviate the constraints derived from TELs across the U.S. states. I empirically investigate this thesis by unbundling the revenue structure of special districts and find positive evidence. Nevertheless, I find that TELs asymmetrically influence distinct lines of revenue, generally user charges and, in some analyses, property taxes and intergovernmental transfers.

Even though I emphasize the revenue structure of special districts, the proliferation of these government units in the United States raises additional concerns as highlighted in the earlier literature. Notably, researchers point out the loss of accountability and political representation resulting from circumventing traditional municipal finance via special-district mechanisms. Similarly, researchers point out the budgetary problem resulting from overlapping jurisdictions taxing a local economic base. In short, tax-and-expenditure limitations not only may be ineffectual in constraining the size of governments but, rather, create loss of efficiency (and accountability) by encouraging financing outside traditional municipal budgets.

The data presented here suggests that, indeed, tax-and-expenditure limitations invigorate special-district financing. Aware of the various threats to validity associated with TELs measures, I test TEL effects through competing identification assumptions. Specifically, I use historical data on direct democracy initiatives as instruments for TELs. My analysis suggest that, indeed, fixed-effects OLS estimators may result in biased estimates, possibly originating from measurement error as well as other sources of endogeneity. I arrive at alternative effects after incorporating initiative measures to instrument for TELs. For example, I estimate that, when increasing TEL severity from the minimum to the maximum score in the data, property taxes increase about 20 percent and general user charges per capita increase almost 50 percent.

The present study also illustrates the role played by systemic data refinement and replication. The TELs index introduced by Lindsay Amiel et al. (2009) is a dramatic contribution to this end. In particular, the index provides a longitudinal tool for testing TELs effects in the United States by looking at such effects over meaningful time frames. I exploit this TELs index to examine how tax-and-expenditure

limitations influence special-district financing as a means to circumvent such limitations on general-purpose governments. Cumulative work in this field will further refine our understanding of the role of fiscal institutions on U.S. fiscal federalism and, in that spirit, data from the present study is readily available to other researchers for replication and extension efforts.

**TABLE 1**  
**Data**

Variable	Description	Source
Property taxes	Are taxes on property ownership and measured by its value. They include taxes on real, personal, tangible, and intangible property. They are reported in thousands of U.S. dollars.	U.S. Census of Governments
Sales taxes	Are taxes on sales and gross receipts from transfer of goods or services. They are reported in thousands of U.S. dollars.	U.S. Census of Governments
Federal IGR	Is intergovernmental transfers from the U.S. Federal Government to special districts. It is reported in thousand of U.S. dollars.	U.S. Census of Governments
State IGR	Is intergovernmental transfers from state governments to special districts. It is reported in thousand of U.S. dollars.	U.S. Census of Governments
Local IGR	Is intergovernmental transfers from local governments to special districts. It is reported in thousands of U.S. dollars.	U.S. Census of Governments
General charges	Are fees, assessments, reimbursements, and charges on sale of commodities and services to users (excepting utilities). It is reported in thousands of U.S. dollars.	U.S. Census of Governments
Utility charges	Are charges on sale of utility commodities and services to users. It is reported in thousands of U.S. dollars.	U.S. Census of Governments
Full-faith-and-credit debt	Is long-term debt issued for which the full taxing power is a special district is pledged. It is reported in thousand of U.S. dollars.	U.S. Census of Governments
Nonguaranteed debt	Is long-term debt issued for which a restricted revenue source of a special district is pledged. It is reported in thousand of U.S. dollars.	U.S. Census of Governments
TELS index	Index of TELS severity, originally to range from 0 to 38, but normalized to range from 0 to 1 in this study. Measurement procedures are explained in Lindsay Amiel et al. (2009).	Lindsay Amiel et al. (2009)
Number of initiatives	Counts the number of ballot initiatives in the states from the previous decade. E.g. it counts the total number of initiatives from 1960 to 1969 to instrument for TELS in 1970.	USC Initiative & Referendum Institute
Approval rate of initiatives	Averages the annual approval rate of initiatives in the states from the previous decade. E.g. it averages approval rates from 1960 to 1969 to instrument for TELS in 1970.	USC Initiative & Referendum Institute
Population	Counts the total population.	U.S. Census of Population
Density	Is the ratio of population per square mile.	U.S. Census of Population
Ethnic diversity index	Is the probability that two randomly drawn individuals belong to different races. This formula draws on Alberto Alesina et al. (1999).	U.S. Census of Population
Median family income	Is the median family income in a county. It is reported in U.S. dollars from the year preceding a census year. E.g. the median family income from 1999 is reported in the 2000 census.	U.S. Census of Population
% Homeownership	Is the percentage of owner occupied housing units over total occupied housing units.	U.S. Census of Population
% College	Is the percentage of the population having attained a bachelor's or higher degree.	U.S. Census of Population
% 18 Years old and under	Is the percentage of the population aged 18 years and under.	U.S. Census of Population
% 65 Years old and over	Is the percentage of the population aged 65 years and over.	U.S. Census of Population
% Poverty	Is the percentage of the population below the poverty level.	U.S. Census of Population
% Unemployment	Is the percentage of the population unemployed. It is based on the civilian population aged 16 year and over.	U.S. Census of Population

**TABLE 2**  
**Summary Statistics**

Variable	1970		1980		1990		2000	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Property tax, per capita	2.46	(6.71)	7.79	(19.79)	16.31	(46.20)	33.51	(77.40)
Sales tax, per capita	0.01	(0.48)	0.13	(2.85)	0.53	(6.71)	0.68	(10.63)
Federal intergovernmental revenue, per capita	1.89	(6.33)	8.86	(28.05)	16.70	(58.84)	32.24	(80.31)
State intergovernmental revenue, per capita	0.65	(3.33)	3.10	(13.23)	6.62	(30.56)	23.18	(72.92)
Local intergovernmental revenue, per capita	1.28	(8.46)	3.08	(16.40)	5.26	(26.27)	13.68	(47.74)
General charges, per capita	10.28	(24.37)	40.24	(91.61)	79.87	(186.11)	136.07	(338.35)
Utility charges, per capita	5.24	(52.09)	23.70	(265.17)	49.08	(405.42)	75.17	(533.92)
Long-term full-faith-and-credit debt issued, per capita	5.48	(27.08)	24.37	(411.13)	29.54	(303.06)	36.67	(219.92)
Long-term nonguaranteed debt issued, per capita	0.42	(5.63)	0.24	(7.02)	7.41	(290.46)	6.15	(85.40)
TELS index	0.18	(0.14)	0.31	(0.20)	0.37	(0.22)	0.44	(0.25)
Number of initiatives, total in previous decade	1.64	(3.58)	3.23	(5.67)	4.09	(7.81)	5.90	(11.88)
Approval rate of initiatives, average in previous decade	0.10	(0.23)	0.14	(0.24)	0.20	(0.30)	0.18	(0.27)
Population	64697	(228271)	72217	(236191)	79182	(263813)	89596	(292462)
Density	214.71	(1,707.45)	214.41	(1,533.88)	220.01	(1,431.52)	243.03	(1,667.47)
Ethnic diversity index	-0.21	(0.98)	-0.06	(0.98)	0.02	(0.99)	0.25	(1.00)
Median family income	8,604.30	(1,936.37)	19,410.41	(3,780.03)	33,923.54	(8,130.82)	51,455.96	(11,882.27)
% Homeownership	0.71	(0.08)	0.73	(0.08)	0.72	(0.08)	0.74	(0.08)
% College	0.16	(0.07)	0.25	(0.09)	0.35	(0.11)	0.43	(0.11)
% 18 Years old or less	0.35	(0.04)	0.29	(0.04)	0.27	(0.04)	0.26	(0.03)
% 65 Years old or more	0.12	(0.04)	0.13	(0.04)	0.15	(0.04)	0.15	(0.04)
% Poverty	0.17	(0.10)	0.12	(0.06)	0.13	(0.07)	0.11	(0.06)
% Unemployment	0.05	(0.02)	0.07	(0.03)	0.07	(0.03)	0.06	(0.03)

*Note:* The table presents means and standard deviations (in parentheses) for unadjusted data to be analyzed in subsequent tables. Special-district finances data are collected by county (and county-equivalent) areas from the U.S. Census of Governments (COG) of 1972, 1982, 1992, and 2002. TELS data are collected from Lindsay Amiel et al. (2009). Initiatives data are collected from the Initiative & Referendum Institute (IRI) (2013). Other covariate data are collected from the U.S. Census of Population and Housing (CPH) of 1970, 1980, 1990, and 2000.

**TABLE 3**  
**Effect of TELs on Special-district Finances: Fixed-effects OLS**

<u>Dependent variables</u>	log per capita Property Tax	log per capita Sales Tax	log per capita Federal IGR	log per capita State IGR	log per capita Local IGR	log per capita General Charges	log per capita Utility Charges	log per capita Long-term Full Faith-Credit Debt	log per capita Long-term Nonguaranteed Debt
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<u>Explanatory variables</u>									
TELS index	0.075*** (0.015)	0.005** (0.002)	0.013 (0.012)	0.045*** (0.013)	-0.006 (0.010)	0.053* (0.030)	-0.005 (0.016)	-0.021 (0.023)	-0.005 (0.006)
log Population	0.043 (0.043)	0.031 (0.036)	-0.129 (0.096)	0.046 (0.034)	0.017 (0.025)	0.061 (0.084)	-0.027 (0.093)	0.009 (0.060)	-0.014 (0.015)
log Density	-0.052 (0.039)	-0.033 (0.036)	0.083 (0.094)	-0.051 (0.031)	-0.041* (0.022)	-0.143* (0.079)	0.057 (0.090)	0.021 (0.054)	0.019 (0.014)
log Median family income	0.043 (0.039)	0.013*** (0.005)	-0.020 (0.032)	0.056** (0.022)	0.043** (0.021)	0.034 (0.062)	0.043* (0.026)	0.134*** (0.050)	-0.004 (0.011)
Ethnic diversity index	0.005 (0.007)	0.006*** (0.002)	0.004 (0.006)	0.003 (0.005)	0.010** (0.005)	0.057*** (0.014)	0.003 (0.007)	0.024** (0.011)	0.001 (0.002)
% 18 Years old or less	0.183 (0.140)	-0.015 (0.016)	-0.273** (0.114)	-0.146 (0.104)	-0.157** (0.080)	0.541** (0.258)	0.281** (0.118)	0.122 (0.217)	0.203*** (0.058)
% 65 Years old or more	0.224 (0.171)	-0.012 (0.021)	-0.093 (0.134)	0.219 (0.140)	-0.196** (0.097)	0.622** (0.314)	0.028 (0.155)	0.271 (0.209)	0.091 (0.059)
% College	0.233*** (0.057)	0.010 (0.010)	0.106** (0.054)	-0.009 (0.052)	0.058 (0.040)	0.211* (0.123)	0.000 (0.067)	0.003 (0.098)	-0.017 (0.030)
% Poverty	0.232*** (0.074)	0.042*** (0.012)	0.040 (0.073)	0.283*** (0.053)	0.010* (0.058)	0.237* (0.140)	0.082 (0.073)	0.295*** (0.110)	0.058 (0.040)
% Unemployment	-0.212** (0.090)	0.027* (0.014)	0.376*** (0.104)	0.227* (0.121)	0.045 (0.124)	-0.382* (0.204)	-0.402*** (0.126)	-0.227 (0.164)	-0.007 (0.042)
% Homeownership	0.127 (0.082)	-0.009 (0.011)	-0.173*** (0.067)	-0.018 (0.052)	0.020 (0.044)	0.119 (0.137)	0.112 (0.084)	0.029 (0.120)	0.041 (0.040)
F-test (joint covariates) p- value	{0.001}	{0.050}	{0.001}	{0.001}	{0.001}	{0.001}	{0.001}	{0.001}	{0.001}
Number of observations	11908	11908	11908	11908	11908	11908	11908	11908	11908

*Note:* The table reports coefficients from two-way (individual and time) fixed-effects OLS and robust standard errors (in parentheses) clustered at the county level. Outcome variable data is collected from the U.S. Census of Governments of 1972, 1982, 1992, and 2002. TELs data is collected from Lindsay Amiel et al. (2009). Control variable data is collected from the U.S. Census of Population and Housing of 1970, 1980, 1990, and 2002.

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

**TABLE 4**  
**First-stage: Initiatives-TELS Correlation**

<u>Endogenous variable</u>	TELS index		
	(1)	(2)	(3)
<u>Instruments</u>			
Number of initiatives	0.009*** (0.000)	... ...	0.008*** (0.000)
Approval rate of initiatives	... ...	0.192*** -0.009	0.103*** -0.009
log Population	0.031*** (0.005)	0.040*** (0.004)	0.029*** (0.005)
log Density	-0.021*** (0.004)	-0.029*** (0.004)	-0.018*** (0.004)
log Median family income	0.028 (0.025)	0.048* (0.028)	0.050** (0.026)
Ethnic diversity index	-0.037*** (0.004)	-0.036*** (0.004)	-0.036*** (0.004)
% 18 Years old or less	0.230** (0.093)	0.131 (0.092)	0.175* (0.090)
% 65 Years old or more	0.345*** (0.087)	0.358*** (0.085)	0.292*** (0.085)
% College	-0.025 (0.043)	0.071* (0.043)	-0.059 (0.042)
% Poverty	0.299*** (0.064)	0.372*** (0.067)	0.365*** (0.064)
% Unemployment	0.723*** (0.102)	0.895*** (0.105)	0.657*** (0.100)
% Homeownership	-0.006 (0.043)	-0.051 (0.043)	0.008 (0.043)
Time fixed-effects	yes	yes	yes
<i>F</i> -test (joint covariates) score	496.95	341.47	425.56
<i>F</i> -test (joint covariates) <i>p</i> - value	{0.000}	{0.000}	{0.000}
Number of observations	12552	12552	12552

*Note:* The table reports coefficients from pooled OLS and robust standard errors (in parentheses) clustered at the county level.

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

**TABLE 5**  
**Second-stage: Effect of TELs on Special-district Finances**

<u>Dependent variables</u>	log per capita Property Tax	log per capita Sales Tax	log per capita Federal IGR	log per capita State IGR	log per capita Local IGR	log per capita General Charges	log per capita Utility Charges	log per capita Long-term Full Faith-credit Debt	log per capita Long-term Nonguaranteed Debt
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<u>Explanatory variables</u>									
TELs index	0.192*** (0.039)	0.014 (0.012)	0.140*** (0.033)	0.063** (0.031)	0.031 (0.025)	0.489*** (0.077)	0.174*** (0.045)	0.110* (0.065)	-0.038*** (0.010)
log Population	0.035 (0.040)	0.031 (0.035)	-0.138 (0.098)	0.044 (0.034)	0.014 (0.025)	0.028 (0.084)	-0.040 (0.094)	-0.001 (0.060)	-0.011 (0.015)
log Density	-0.051 (0.036)	-0.032 (0.036)	0.084 (0.096)	-0.050 (0.031)	-0.041* (0.022)	-0.139* (0.079)	0.059 (0.091)	0.022 (0.055)	0.019 (0.014)
log Median family income	0.058 (0.040)	0.014*** (0.006)	-0.003 (0.032)	0.058*** (0.023)	0.047** (0.021)	0.091 (0.063)	0.066** (0.027)	0.152*** (0.051)	-0.009 (0.011)
Ethnic diversity index	-0.004 (0.008)	0.005*** (0.002)	-0.006 (0.007)	0.002 (0.005)	0.007 (0.005)	0.023 (0.015)	-0.011 (0.007)	0.013 (0.011)	0.003 (0.002)
% 18 Years old or less	0.201 (0.139)	-0.014 (0.017)	-0.253** (0.114)	-0.143 (0.104)	-0.151* (0.080)	0.609** (0.262)	0.309*** (0.120)	0.142 (0.217)	0.197*** (0.057)
% 65 Years old or more	0.246 (0.171)	-0.010 (0.020)	-0.069 (0.134)	0.222 (0.139)	-0.189* (0.097)	0.703** (0.316)	0.061 (0.157)	0.295 (0.211)	0.085 (0.059)
% College	0.207*** (0.058)	0.008 (0.011)	0.078 (0.055)	-0.013 (0.052)	0.050 (0.040)	0.117 (0.126)	-0.039 (0.069)	-0.025 (0.099)	-0.010 (0.029)
% Poverty	0.231*** (0.073)	0.042*** (0.012)	0.039 (0.073)	0.282*** (0.053)	0.100* (0.058)	0.233* (0.141)	0.080 (0.074)	0.294*** (0.110)	0.058 (0.040)
% Unemployment	-0.231** (0.091)	0.026* (0.014)	0.355*** (0.105)	0.224* (0.121)	0.040 (0.124)	-0.452** (0.208)	-0.430*** (0.127)	-0.248 (0.166)	-0.001 (0.043)
% Homeownership	0.108 (0.082)	-0.011 (0.010)	-0.194*** (0.067)	-0.021 (0.052)	0.014 (0.044)	0.048 (0.139)	0.082 (0.084)	0.008 (0.120)	0.047 (0.040)
F -test (joint covariates) p- value	{0.000}	{0.000}	{0.000}	{0.000}	{0.000}	{0.000}	{0.000}	{0.001}	{0.000}
Number of observations	11857	11857	11857	11857	11857	11857	11857	11857	11857

*Note:* The table reports coefficients from two-way (time and individual) fixed-effects IV estimation and robust standard errors (in parentheses) clustered at the county level. The TELs index is instrumented using the number of initiatives and control variables in the first stage.

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

**TABLE 6**  
**Sensitivity and Robustness Checks**

<u>Dependent variables</u>	log per capita Property Tax	log per capita Sales Tax	log per capita Federal IGR	log per capita State IGR	log per capita Local IGR	log per capita General Charges	log per capita Utility Charges	log per capita Long-term Full Faith-credit Debt	log per capita Long-term Nonguaranteed Debt
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<b>Panel 5.1—Subsample of initiative states only</b>									
<u>Explanatory variables</u>									
TELS index	0.145*** (0.048)	0.015 (0.012)	0.115** (0.047)	0.044 (0.040)	0.055* (0.031)	0.620*** (0.099)	0.184*** (0.057)	0.055 (0.080)	-0.007* (0.004)
<i>F</i> -test (joint covariates) <i>p</i> -value	{0.000}	{0.154}	{0.000}	{0.000}	{0.000}	{0.000}	{0.000}	{0.002}	{0.447}
Number of observations	5224	5224	5224	5224	5224	5224	5224	5224	5224
<b>Panel 5.2—Subsample of metropolitan areas only</b>									
TELS index	-0.036 (0.043)	0.030 (0.031)	0.218*** (0.053)	-0.029 (0.039)	0.158*** (0.044)	0.390*** (0.104)	0.229*** (0.077)	0.140 (0.096)	-0.048* (0.025)
<i>F</i> -test (joint covariates) <i>p</i> -value	{0.000}	{0.012}	{0.000}	{0.000}	{0.000}	{0.000}	{0.000}	{0.018}	{0.000}
Number of observations	3206	3206	3206	3206	3206	3206	3206	3206	3206
<b>Panel 5.3—Subsample of non-metropolitan areas only</b>									
TELS index	0.311*** (0.052)	0.000 (0.000)	0.106** (0.044)	0.128*** (0.040)	-0.031 (0.026)	0.546*** (0.105)	0.133** (0.058)	0.081 (0.088)	-0.026*** (0.009)
<i>F</i> -test (joint covariates) <i>p</i> -value	{0.000}	{0.992}	{0.000}	{0.000}	{0.000}	{0.000}	{0.000}	{0.000}	{0.384}
Number of observations	8651	8651	8651	8651	8651	8651	8651	8651	8651
<b>Panel 5.4—Full sample: Fixed-effects IV (two instruments)</b>									
TELS index	0.193*** (0.039)	0.014 (0.012)	0.139*** (0.033)	0.065** (0.031)	0.031 (0.025)	0.493*** (0.077)	0.175*** (0.045)	0.111* (0.065)	-0.038*** (0.010)
<i>J</i> -test (overidentification) <i>p</i> -value	{0.464}	{0.684}	{0.092}	{0.004}	{0.180}	{0.005}	{0.099}	{0.125}	{0.953}
<i>F</i> -test (joint covariates) <i>p</i> -value	{0.000}	{0.015}	{0.000}	{0.000}	{0.000}	{0.000}	{0.000}	{0.001}	{0.000}
Number of observations	11857	11857	11857	11857	11857	11857	11857	11857	11857
<b>Panel 5.5—Full sample: First-differences IV (two instruments)</b>									
TELS index	0.199*** (0.032)	0.006 (0.008)	0.113*** (0.034)	0.050* (0.029)	0.017 (0.023)	0.315*** (0.066)	0.054* (0.033)	0.140** (0.064)	-0.024*** (0.008)
<i>J</i> -test (overidentification) <i>p</i> -value	{0.000}	{0.525}	{0.365}	{0.008}	{0.492}	{0.001}	{0.000}	{0.013}	{0.994}
<i>F</i> -test (joint covariates) <i>p</i> -value	{0.000}	{0.012}	{0.000}	{0.000}	{0.000}	{0.000}	{0.000}	{0.000}	{0.000}
Number of observations	8810	8810	8810	8810	8810	8810	8810	8810	8810

*Note:* Panels 4.1, 4.2 and 4.3 report coefficients from FE-IV using the number of initiatives as instrument. Panels 4.4 and 4.5 report coefficients from FE-IV and FD-IV respectively, incorporating the number of initiatives and the approval rate of initiatives as instruments. All regressions control for two-way fixed effects and covariates from table 3.

\*\*\**p* < 0.01; \*\**p* < 0.05; \**p* < 0.1

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