Why has growth of user fees and other nontraditional sources of local education revenues been so limited? Evidence from Colorado

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Abstract

Purpose – The purpose of this paper is to explore why school districts in the USA made so little use of local sources of non-tax revenues, even when faced with declines in traditional revenue as occurred during the Great Recession? The analysis uses the case of Colorado, where historically districts have made more use of alternative revenues.

Design/methodology/approach – Data for the analysis are drawn from the NCES's Common Core of Data with administrative data to create a panel of Colorado school districts. The paper presents estimates of traditional panel models, as well as spatial panel models, that give the correlates of variation in alternative revenue for education.

Findings – As is true nationally, in Colorado school districts made no increased use of non-tax revenues in fiscal downturns, while the presence of expenditure limits does increase use, though not as might be expected. Revenues from overrides of the limits and alternative local revenues appear to be complements. Further, there is no evidence of spatial relationships for the alternative revenue sources considered.

Originality/value – This paper uses richer data than has ever been used to explore the determinants of alternative revenues, making it possible to explore relationships others could not. In addition, synthetic cohort analysis is used to generate plausible instrumental variables for passage of an override of an expenditure limitation. Further, no existing analysis of nontraditional revenues considers the possibility that use of those revenues might be spatially correlated.

Keywords Tax and expenditure limitations, Education finance, Synthetic cohort analysis, User fees

Paper type Research paper

Introduction

In April 2016, Brockton Public Schools Superintendent Kathleen Smith submitted a budget proposal that included a recommendation that one school be closed, that $1.5m be cut from expenditures on administration and that another $1.5m be cut from athletics, after-school programs and technology (Papadopoulos, 2016). The budget problems facing Brockton, a large American school district near Boston, Massachusetts, are not unique. Even though the Great Recession nominally ended in 2009, revenues in many districts are still below their pre-recession levels. While property tax revenues were initially slow to decline during the Great Recession, reductions in state aid have forced most local governments to both reduce spending and seek alternative revenue sources. Increases in federal aid insulated school districts from these effects during the first part of the downturn, but school districts began to feel the full impact of the revenue declines in fiscal year 2011 (Kenyon and Reschovsky, 2014).

The Brockton example suggests a potential alternative to programmatic cuts. Athletics and after-school programs are examples of what Wassmer and Fisher (2002) called auxiliary services. In most states, schools are not mandated to provide these services. Further, the services included in this class benefit primarily or exclusively the consumer of the services.

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As a result, districts could gain revenue by charging for these services, and, some have argued, changes in consumption that result from such charges do not have impacts that extend beyond the family making the consumption decision. Whether school district decision makers subscribe to this view, or to the view that participation in athletics and after-school programs generates social benefits and reduces the likelihood of non-social behavior is a question we will return to later in this paper.

Fees for athletic participation are just one example of the ways in which schools can generate revenues outside of traditional sources; Table I gives other examples of these nontraditional sources. And, given the fiscal constraints facing school districts and the potential revenues from charging for auxiliary services, it is unsurprising that numerous popular press stories (Bergal, 2015; Conti, 2015; Mohl and Patel, 2015) comment on the apparent growth of these charges and other non-tax revenues in the face of budget deficits. But, while this apparent growth of fees and other nontraditional revenues[1] has been much noted, it has been far less extensive than these articles may lead the reader to believe, as shown by Downes and Killeen (2014).

What that earlier analyses failed to establish was why school districts have made so little use of these alternative revenue sources. That is the question that will be at the heart of this paper. In particular, we will use data from Colorado, a state in which districts have made relatively heavier use of fees and other nontraditional revenue sources, to shed additional light on the factors that correlate with variation in use of alternative revenue sources.

This line of study is driven by a compelling contrast in public finance. Outside of elementary and secondary education, fees and charges, in particular, have become an increasingly important source of revenue for local US governments in the post-tax revolt era (Fisher, 2016). There is a curious insulation of local school districts from what is typically expected in the budgeting and fiscal behavior of local governments in general.

Type of revenue	Mean across Colorado districts in 1991–1992	Mean across all districts in the USA in 1991–1992	Mean across Colorado districts in 2012–2013	Mean across all districts in the USA in 2012–2013
Fees and charges	$350.25	$89.29	$354.68	$171.10
Component: transportation fees	$0.39	$1.54	$3.13	$1.84
Interest earned	$255.89	$161.76	$33.51	$55.45
School lunch revenues	$154.64	$158.74	$123.12	$154.69
Tuition fees	$7.11	$20.28	$42.44	$34.77
Sales	$0.00	$10.34	$71.08	$71.73
Miscellaneous revenues[b]	$516.51	$176.32	$510.37	$377.08
Component: private contributions	–	–	$78.10	$31.32
Component: rents and royalties	–	–	$25.93	$22.63
Component: property sales	–	–	$17.05	$10.32
Component: fines	$0.00	$10.34	$71.08	$71.73
Component: other miscellaneous revenues	–	–	$318.21	$241.08
All non-tax revenues	$1284.38	$616.74	$1092.22	$831.87
Non-tax revenue as a share of total revenue	0.133	0.061	0.079	0.049

Notes: [a]All dollar figures are adjusted to 2017 dollars using the CPI-U; [b]private contributions, rents and royalties, and property sales became separate items beginning in fiscal year 2006

Table I. Types and distribution of local non-tax revenues (per pupil) (Colorado vs USA[a])

Popular discussion of the Colorado context (Great Education Colorado, 2016b) suggests that this insulation may not extend to one source of non-tax revenues, donations, which, it is argued, might perpetuate inequity because "higher income communities" can more easily raise significant funds through private fundraising. However, research on donations fails to document disequalizing effects (Brunner and Sonstelie, 2003; Nelson and Gazley, 2013). Brunner and Sonstelie (2003) suggest why this might be so; districts will turn to nontraditional sources such as contributions only when traditional sources become prohibitively expensive, as might be true when a TEL constrains local taxing ability. Downes and Steinman's (2008) findings for Vermont tend to confirm that. But, since TELs and school finance systems create heterogeneity in tax prices that cannot be quantified in national data sets, we turn to an individual state where tax prices vary and, potentially, are high for some districts.

With these general observations in mind, the next section of this paper provides more detail on the Colorado context in which the analysis in this paper is executed. Colorado provides a particularly apt setting for examining the use of fees and other alternative revenues. These revenue sources have been consistently utilized by most of the state's districts, and changes in the fiscal environment make it possible to examine whether alternative revenue sources are viewed as a replacement for traditional sources of revenue when access to those revenue sources is constrained. Voter approval in 1992 of the Taxpayer Bill of Rights (TABOR), one of the country's most stringent tax limitation measures, led to increased local reliance on non-tax revenues among non-school local governments. We discuss the details of TABOR and how we are able to take advantage of aspects of TABOR to explore the link between fiscal constraints and alternative revenue use among school districts.

After discussing the data used in our analyses, we review the models estimated to analyze the use of fees and other alternative revenue sources in Colorado. Among the models we estimate are some that allow for the possibility there is spatial correlation in fees and non-tax revenues because each district's use of alternative revenues depends upon the extent to which its neighbors use these revenue sources, a possibility that has been suggested by the literature on tax mimicking (Ladd, 1992). We also note recent critiques of these spatial models (e.g. Gibbons and Overman, 2012) and indicate how those critiques influence our empirical strategy.

We also observe that, while the fact that TABOR allows a school district's voters to override the revenue limits makes Colorado a particularly interesting case for analyzing alternative revenue use, the existence of overrides also creates complications. The endogeneity of overrides must be addressed to generate unbiased estimates, but we have no natural instruments in the data on the Colorado districts themselves. To solve this problem, we draw on the recent synthetic cohort literature, as exemplified by Eliason and Lutz (2018), to create synthetic controls for each of the districts in Colorado. We then use the synthetic controls to generate an exogenous measure of the TABOR-induced gap between each district's desired and actual spending, its shortfall, and instrument for overrides using this shortfall measure.

In the findings section, we review results from the estimates of the various models presented in the preceding section. We find that, while some of the results from Colorado duplicate those in our previous analysis, focusing on a single state does allow us to present a more nuanced picture of why the use of nontraditional revenues varies across school districts and over time. As was true nationally, we find that in Colorado districts made no increased use of non-tax revenues in the most recent two economic downturns. And, while there is a relationship between fiscal institutions and the use of nontraditional revenues, we are able to use our knowledge of the Colorado context to show that revenues from overrides of the TABOR limits and alternative local revenues are positively related. This suggests

that these revenue sources are not viewed as substitutes, either because they are truly complements or because districts maintain relatively constant shares of revenue from each source, with overrides enabling districts to expand use of all sources. Further, we find no evidence of a relationship between a district's taxable property wealth and its use of non-tax revenues, suggesting that the disequalizing impact of donations and other non-tax revenues is limited.

We also find limited evidence of spatial relationships for the revenue sources we consider. Estimates of traditional spatial models suggest that district revenue choices might be influenced not by geographic neighbors but by districts with similar per capita incomes or taxable property wealth per pupil. But, when we test for spatial relationships using reduced-form models, as suggested by Gibbons and Overman (2012), we only find evidence of the presence of spatial relationships for all non-tax revenues. But the impact of those spatial relationships appears to be limited; even traditional spatial models suggest the implied effects of the explanatory variables changes little relative to the implications of estimates that do not account for any potential correlation with neighbors. Thus, even though neighbors might matter, the impact on any districts revenue choices of the revenues of its neighbors is small, at most.

The paper concludes with a summary of key findings and discussion of the implications of those findings for policy. We argue that our results suggest that traditional empirics alone may be inadequate to tell us why these revenue sources have been little used. We suggest that state-level case studies like ours need to be complemented by work that asks actors on the ground to answer the why question.

Why Colorado?

To further explore the factors that affect school district use of fees and other alternative revenues, we began by selecting a state in which alternative revenues have been a consistent part of most districts' revenues. In addition, we wanted to select a state in which use of these revenue sources was not limited to a few districts. Table I lists the main components of nontraditional revenues and compares the use of these revenues in Colorado districts to their use nationwide[2]. Table II presents summary statistics for alternative revenues, fees and other revenue measures for Colorado school districts for selected years.

As Table I shows, nontraditional revenues have been a small but stable source of revenue for Colorado districts. A *Denver Post* story (Torres, 2013) draws out the widespread use of non-tax revenue in Colorado, highlights the burden placed on families by school fees and notes that these charges have become more pervasive and burdensome in the aftermath of the Great Recession. Among the fees cited in the article are an annual transportation pass of $150 in Douglas County, charges of up to $189 to take an advanced placement class in Jefferson County, as well numerous examples of charges for facility use, textbooks and instructional materials, and extracurricular participation like clubs and sports.

In our previous work, Downes and Killeen (2014) found that the use of fees was higher in districts in states in which districts were subject to TELs, but the authors could not determine the extent to which there was significant within-state variation in the use of alternative revenues in states with limits in place. Colorado gives us the opportunity to see if there is such within-state heterogeneity. In 1992, voters in Colorado approved the TABOR, one of the most stringent limits on the revenue-raising ability of the state government and local governments. TABOR limited revenues and spending for any school district to the previous year's amount plus inflation plus the percentage of growth in enrollment in the district. Spending or revenue growth in excess of what is permitted under TABOR could occur only if a jurisdiction's voters chose to override the limit.

Concern about the implications of TABOR for education spending led Colorado voters to pass Amendment 23 in 2000. Amendment 23 required growth in base and categorical

Table II. Yearly means (per pupil) for alternative revenues, fees, locally generated revenues and total revenues

	1991–1992	1992–1993	1993–1994	1994–1995	1995–1996	1996–1997	1997–1998	1998–1999	1999–2000	2000–2001	2001–2002
Alternative revenues	1,284.38	1,133.57	928.99	1,098.33	1,130.71	1,124.52	1,084.05	1,063.52	1,135.61	1,159.33	1,088.19
	(1,666.76)	(1,576.78)	(801.53)	(735.48)	(959.71)	(1,068.75)	(1,026.52)	(541.12)	(648.01)	(551.05)	(616.42)
Fees	350.25	235.69	253.52	370.80	260.59	258.54	317.74	343.70	342.22	348.54	379.54
	(260.81)	(147.36)	(157.32)	(246.90)	(272.51)	(305.83)	(279.85)	(297.96)	(303.58)	(298.63)	(375.56)
Total locally generated revenues	6,140.29	5,548.00	5,085.40	5,584.26	5,413.99	5,320.04	5,427.11	5,491.90	5,598.28	5,537.76	5,766.44
	(4,152.12)	(3,704.67)	(3,210.18)	(3,281.02)	(3,305.40)	(3,067.13)	(3,096.28)	(3,002.29)	(3,007.25)	(2,882.43)	(3,308.28)
Total revenues	11,184.93	9,882.99	9,318.04	11,000.57	10,906.36	11,011.84	11,274.67	11,560.69	11,754.32	11,808.39	12,571.91
	(3,850.90)	(2,618.47)	(2,217.69)	(2,993.82)	(2,999.63)	(3,083.51)	(2,998.87)	(2,946.45)	(3,150.77)	(3,012.59)	(3,456.35)
Districts	176	57	57	176	176	176	176	176	176	176	178
	2002–2003	2003–2004	2004–2005	2005–2006	2006–2007	2007–2008	2008–2009	2009–2010	2010–2011	2011–2012	2012–2013
Alternative revenues	1,098.56	1,019.89	1,157.26	1,281.30	1,310.78	1,476.04	1,255.64	1,049.20	1,029.30	1,001.29	1,092.22
	(652.90)	(562.80)	(877.47)	(1,620.56)	(954.62)	(2,048.59)	(974.29)	(636.76)	(588.12)	(590.16)	(968.37)
Fees	377.53	373.39	372.53	370.06	372.43	366.36	357.41	345.24	350.34	351.02	354.68
	(326.62)	(300.52)	(302.94)	(300.64)	(295.28)	(304.29)	(292.13)	(272.90)	(278.92)	(278.68)	(268.72)
Total locally generated revenues	5,762.95	5,722.31	5,758.17	6,525.64	6,047.93	6,506.37	6,495.38	6,818.03	6,469.00	6,500.01	6,769.29
	(3,073.67)	(3,080.36)	(3,197.74)	(7,430.74)	(3,233.92)	(4,254.21)	(3,886.88)	(4,338.14)	(4,146.19)	(4,538.49)	(4,447.82)
Total revenues	13,195.19	13,312.09	13,241.61	13,895.41	13,539.42	14,049.56	14,165.51	14,857.77	14,558.04	15,153.17	14,422.58
	(4,010.93)	(3,771.52)	(3,894.90)	(7,898.88)	(3,815.02)	(4,660.59)	(4,252.63)	(5,983.62)	(8,964.79)	(12,546.49)	(9,415.06)
Districts	178	178	178	178	178	178	178	178	178	178	178

Note: Standard deviations are in parentheses

funding to equal inflation plus 1 percent[3]. In addition, the amendment established a state education fund, financing of which was exempt from TABOR. These funds were intended to supplement existing state funds for education, establishing a growth rate for state general fund contributions exclusive of the funds from the newly established education fund (Martell and Teske, 2007).

In spite of this relatively favored status of education funding, the stringency of TABOR appears to have constrained local school districts. And the effects of TABOR have been accentuated by the Gallagher Amendment, which was passed in 1982. The intent of the Gallagher Amendment was to maintain a constant ratio between business and residential property taxes. That ratio is maintained by reducing the assessment rate on residential property whenever the growth in the value of residential property is faster than the growth in value of business property (Great Education Colorado, 2016a). Since TABOR limits growth of nominal tax rates, Gallagher and TABOR together constrain each district's effective tax rate and, thus, its revenue growth.

Both before and after TABOR, state aid in Colorado was determined using a foundation formula. Aid is determined by the difference between the state-established foundation amount and the amount of revenue each district is required to raise. While foundation amounts vary across districts, in every district the marginal dollar is raised locally, either from property taxes or from non-tax revenues (Dallas, 2018).

TABOR imposes limits on all revenues, including nontraditional revenues. In other words, for any district that is at its TABOR limit, total revenues can only be increased if the voters approve an override. So, for districts at the TABOR limit, non-tax revenues can be increased if tax revenues or intergovernmental aid are reduced or if an override is passed making it possible to increase revenues from any source. In that sense, non-tax revenues are like property tax revenues. Nevertheless, non-tax revenues are treated differently under TABOR than are taxes. Any tax rate changes must be approved by voters, even in districts not bound by their revenue limit. The Colorado Supreme Court has ruled that no such voter approval is required for non-tax revenues (*Barber* v. *Ritter*, 196 P.3d 238, 241-242, 250-251 (Colo. 2008)). As a result, for districts that are not at their revenue limit, increasing non-tax revenues is easier than increasing property tax revenues.

Further, in contrast to limits imposed in other states, TABOR applied to special purpose governments. As a result, the incentive to create special purpose governments, which frequently are financed with user charges and fees, was much weaker in Colorado (St. Clair, 2012). Thus, the extent to which fees and other alternative revenues have been utilized by overlaying governments has been less in Colorado than in other limit states, potentially giving school districts more scope to take advantage of these revenue sources.

Inclusive of the special conditions associated with TABOR mentioned above, the most observable ways in which school districts have responded to the constraints imposed by TABOR has been by seeking overrides and by pursuing nontraditional revenues (Teske, 2005). Our expectation is that differential success in pursuing overrides could translate into differential reliance on nontraditional revenues. Colorado thus provides a natural case for exploring within-state heterogeneity in the response to local TELs.

Additionally, Colorado is a good setting for exploring the spatial relationships in the use of fees and other alternative revenue sources. While school districts in Colorado are geographically large, school district boundaries in the state have been relatively stable over our period of analysis. In the 1991–1992 school year, the first year for which we have financial data, there were 176 school districts present in the state. That number increased to 178 in the 2001–2002 school year, when two school districts split into four. The number of districts in the state has remained at 178. This geographic stability allows us to explore whether changes in nontraditional revenue usage by neighboring school districts are associated with commensurate changes in usage by districts of focus.

Why not fees? How can we deepen our understanding of the use of nontraditional revenues

In our previous paper on fees and other nontraditional revenues, we offer a relatively exhaustive review of the sparse literature on school district use of non-tax revenues. One of the critical take-ways from that review is that there is potentially considerable scope for school districts to expand their use of these revenue sources (Wassmer and Fisher, 2002). The other critical lesson, both from that literature and from our own analysis, is that, in general, school districts have not expanded their reliance on these nontraditional revenues, even when faced with declining receipts from traditional sources. The one potential exception to this lesson is that districts in states with state and local revenue or expenditures limits might rely slightly more heavily on locally generated, non-tax revenues.

Analyses of school district responses to the state aid cuts that occurred during the Great Recession have confirmed that property tax increases were insufficient to compensate for state aid reductions (Alm *et al.*, 2011; Chakrabarti *et al.*, 2014). Nontraditional revenues would seem to be a natural mechanism for closing some of the remaining gap as they have been for local government entities (e.g. special districts, cities, towns, etc.) since the 1970s. But we (2014) and others (Nelson and Gazley, 2014) have found little to no evidence that these revenues increased among school districts in the wake of either of the last two recessions[4]. The challenge, then, is to improve our understanding of why the use of nontraditional revenues varies across districts and of why there are changes in some districts in the share of revenues from these sources. While the literature (Brunner and Sonstelie, 2003; Downes and Steinman, 2008; Downes, 2016) suggests major institutional changes such as TELs and school finance reforms can lead to shifts to nontraditional revenues by changing tax prices, are there other key drivers of growth?

Brunner and Sonstelie (2003) lay out the empirical strategy on which the analysis in this paper builds. The theory that motivates the empirical work in that paper implies that the primary determinants of revenue choices will be the attributes of the district that influence the public choice process, including the district's resources and its demographics, as well as the determinants of each family's demand for school quality.

Further, a growing empirical literature has suggested that among the factors that influence the local public choice process and voters' demand for school quality are the choices of its neighbors. Brueckner (2003) provides an excellent review of the basic theoretical models that frame empirical studies of how the revenue choices of a locality relate to those of its neighbors. As Brueckner notes, the theoretical models lead to the same basic empirical model, even if they imply slightly different interpretations of the estimated parameters. That basic model takes the form:

$$z_{it} = \alpha_i + \rho \sum_{j \neq i} \omega_{ij} z_{jt} + X_{it}\theta + \tau_t + \varepsilon_{it}, \tag{1}$$

where z_{it} is a component of local revenue in school district i in period t, ω_{ij} are nonnegative weights assigned to the local revenue in school district j, ρ is the spatial correlation in revenue between districts, α_i is a school district-specific effect, τ_t is a time-specific effect, X_{it} are characteristics of school district i that could influence that district's revenue choices and ε_{it} is an error term. When estimating spatial relationships of this kind, researchers have drawn heavily on Anselin's (1988) work. As that work and the work that has followed have made clear, among the choices that researchers must make are how the weights are determined, how this inherently endogenous model is estimated and whether the error terms are also spatially related.

More recently, several authors, most notably Gibbons and Overman (2012), have argued that in most contexts the parameters of this model are not well-identified. Recent work by

Lyytikäinen (2012), Isen (2014) and Baskaran (2014), using data drawn from contexts where spatial relationships are likely to be identified because of the existence of quasi-experimental variation, has confirmed the validity of the Gibbons and Overman critique. The implication is that using standard spatial estimation techniques to estimate the parameters of (1) is likely to produce biased estimates.

And, yet, the fact remains that spatial relationships probably exist and are potentially very important. Over time scholars have routinely argued that local fiscal policies are in part informed and shaped by nearest neighbors through some form of organizational mimicry. Chapman (1999) postulated that the pressures of inter-jurisdictional tax competition encourages local governments to balance the tax and service preferences of their residents, and the potential mobility of these residents, against the tax and service packages of neighboring jurisdictions[5]. In one of the earliest papers to establish spatial links in taxation, Ladd (1992) found among US counties that local tax burdens are related to the tax burdens in neighboring counties, beyond what is expected based upon shared demographic or economic conditions. Numerous other authors, such as Heyndels and Vuchelen (1999), have also found evidence of tax system mimicry in a variety of other contexts.

But what about fee usage by educational organizations? Killeen (2001) found qualitative evidence of a range of institutional and spatial relationships among California school districts that help explain the adoption, acceptance and routine alteration of school impact fee policy. During interviews, school administrators at the Alum Rock and Franklin-McKinley School Districts identified a range of mechanisms that steered the governance of impact fee policies, including facility planning requirements (class size reduction and year round schooling), school impact fee laws (Proposition 1A), various ballot initiatives and their related administrative codes (Proposition 13, Proposition 1A, Fee Level Codes), as well as inter-local agreements and local city ordinances. While this scholarship indicates that fee usage by local school districts is likely affected by non-structural pressures determined by demographic and fiscal contexts, the scholarship also suggests that districts define their local fiscal policies (e.g. fee usage) with an eye on how neighboring jurisdictions do so. In addition, local fiscal choices are influenced by a range of institutional pressures (e.g. regulations, policies and codes) from hierarchical and nearby organizations.

Data

The core data for this analysis are drawn from the National Center for Education Statistics' Common Core of Data (CCD). In fiscal year 1990, financial data collected by the Census Bureau and released as the F-33 survey became part of the CCD. However, in fiscal year 1990, no information on fees was collected. As a result, our data begin with fiscal year 1992 (school year 1991–1992). In fiscal years 1993 and 1994, the F-33 survey was only administered to a sample of school districts. In most states, the districts that were sampled were non-random. For example, in Colorado the sampled districts were larger, and they made less use of fees and alternative revenues. As a result, in all of our analyses, we have generated results using data extracts that both included and excluded fiscal years 1993 and 1994. While the results do not differ dramatically if we include data from fiscal years 1993 and 1994, below we have chosen to report the estimates that are generated when we exclude the data from those years.

While the detailed financial data available from the F-33 survey provided the data at the center of this analysis, we also drew from the CCD information on each district's student population. Data on student race, ethnicity, percent eligible for free lunch and the percent of students designated as special needs were available in all years up through the 2005–2006 school year; data on the population eligible for reduced-price lunches and the population of limited English proficient students only became available in the 1998–1999 school year. We used administrative information from the Colorado Department of Education to fill in gaps

in the data on special education and limited English proficiency that appeared after the 2004–2005 school year. As a result, when we do not use the fraction of students who are limited English proficient as a control, our sample includes fiscal years 1992 and 1995–2013; when we use fraction of students who are limited English proficient as a control, our sample includes fiscal years 1999–2013.

To create the measure of student fees used below, we combined four line items on the F-33 survey: transportation fees from pupils and parents, textbook sales and rentals, district activity receipts, and student fees, non-specified[6]. While we had hoped to create measures of district revenues from sales and from entrepreneurial activity, doing so was not possible since in Fiscal Year 2006 the Census Bureau added to the F-33 survey items on private contributions, rents and royalties, fines and forfeits, and sales of property. Prior to that year, these items were probably part of each district's miscellaneous revenues, though some districts may have included these items in their reports of other, non-fee alternative revenue sources. As a result, we can only be confident that our measures of fee revenue and of the total of local, non-tax revenue are consistently reported in all years.

Data on each district's racial/ethnic composition, age composition and per capita income were drawn from the Decennial Censuses of 1990, 2000 and, starting in school year 2006–2007, from the five-year extracts from the American Community Survey.

Information on the taxable value of property in each school district was drawn from the Annual Reports of the Division of Property Taxation of the state's Department of Local Affairs[7] and from the mill levy tables provided by the Colorado Department of Education. We used the annual reports to fill in data for years that could not be provided by the Department of Education. While, in those years in which the data overlapped, total assessed value amounts differed slightly between the two data sources, the differences did not appear to be of sufficient magnitude to dramatically affect any of our estimates[8].

Downes and Killeen (2014) found that one of the few consistent determinants of the use of fees and other alternative revenue sources was the presence of limits on the ability of localities to raise revenues or increase expenditures. However, the authors of that paper could only include relatively crude controls for the presence of local TELs, given the national nature of the analysis. In particular, they simply accounted for the presence in any year of a local TEL. Our expectation is that the impact of TELs is probably far more nuanced, particularly in a state like Colorado where voters can choose to override the limit. For that reason, we have used a spreadsheet provided by the Colorado Department of Education to determine the school districts in which voters have approved overrides. This same source of information also provides us with information on districts in which a proposed override has failed[9]. By the end of our period of analysis, many districts had passed at least one override. But, in any given year, override success was anything but certain. Thus, we are able to create dummy variables that indicate if a district has passed an override in that year or at any point in the past and if a district has attempted but failed to pass an override in that year or at any point in the past[10]. The reference category is then the set of districts that had not attempted an override by the year in question.

Eliason and Lutz (2018) argue that spending on schooling in Colorado as a whole, and in most school districts in the state, has not, in the long run, been constrained by TABOR. The failure of TABOR to create long-run constraints is due to the growing prevalence of overrides, the passage of Amendment 23 and the rapid growth in property values in select districts. Nevertheless, select districts did face short-term constraints. And these districts were more likely to seek an override.

Failing to account for the non-random nature of override votes could lead to biased estimates of the link between overrides and alternative revenues. We address this problem by instrumenting for overrides using a measure of the magnitude of each district's revenue shortfall or the gap between what its revenues would have been in a non-TABOR world and

its actual revenues. Eliason and Lutz (2018) suggest that the synthetic control methodology (Abadie *et al.*, 2010) can be used to generate just such a shortfall measure[11]. The Appendix describes the methodology we used to apply the synthetic control methodology and create a shortfall measure for each district.

Since actual revenues move exogenously post-TABOR and target revenues are calculated using each district's synthetic cohort, the shortfall is exogenous. To account for the potential nonlinearity of the likelihood of override passage in the extent of the true shortfall, we used the shortfall measure and its square as instruments for overrides and failures.

We used both Cartographic Boundary Shapefiles and the Census Gazetteer files from the US Census Bureau's TIGER mapping program to determine the spatial relationships between school districts. However, going back to some of the earliest work on the impact of "neighbors" on taxing and spending decisions of local governments (e.g. Case *et al.*, 1993), researchers have recognized that economic neighbors may not be geographic neighbors. In this context, school district budgeting decisions might be influenced by the decisions made by other school districts with which they typically share information or districts which are demographically similar. As a result, we also explored the possibility that "neighbors" consisted of all districts served by the same regional field services office[12] or that neighbors are districts with similar per capita incomes or per student taxable property wealth[13].

Summary information for key financial, student demographic and district demographic data for selected years is given in the Appendix[14]. In Colorado, the number of districts present in the 2012–2013 school year is essentially the same as the number present in 1991–1992, with the increase of two attributable to two districts being split as of the 2001–2002 school year. Because of limits imposed by the Stata routines we use to estimate spatial panel models, many of our estimates are based on the balanced panel of districts. One virtue of the Colorado context is that the need to use a balanced panel will matter little.

Table II reveals that the use of fees and other non-tax revenues has been relatively flat, even though both locally generated revenues and total revenues have grown. There is no indication that TABOR has led districts to make more use of non-tax revenues. While the table cannot offer a clear explanation for why there is little growth in use of these revenue sources, the growing presence of overrides suggests that the continued reliance on property taxation as one possible reason why districts have not diversified their revenue sources. In addition, the special treatment of K-12 expenditures at the state level, as signaled by Amendment 23 and the real growth in state aid, points to another factor that may have mitigated against growth in non-tax revenues.

Models

In the spirit of Killeen (2007), we begin our analysis by considering variants of (1) in which ρ, the coefficient on the spatial factor, is set to 0. Starting with this specification allows us to compare the results for Colorado to those from an analysis of national data (Downes and Killeen, 2014). In addition, we can use this specification to highlight the potential advantages of focusing on a single state and using data that facilitate drawing out the nuances of that state's context. Finally, instrumenting for override passage is possible in the non-spatial context, allowing us to determine if our results are sensitive to accounting for the endogeneity of override passage.

We then explore two variants of (1) in which ρ is not constrained to be 0. Many authors have modeled spatial relationships in local government choices by assuming that, once the spatial relationship in the policy variables is taken into account, there is no spatial correlation in the error terms ε_{it} (see e.g. Fowles, 2016; Brasington *et al.*, 2016). This modeling choice results in a spatial autoregressive (SAR) lag specification.

Critiques of the spatial relationships literature (e.g. Gibbons and Overman, 2012) have emphasized that, in many contexts, identification of those relationships requires that the spatial model be correctly specified. With that in mind, we have explored the sensitivity of our estimates to changing the definition of the set of neighbors and to estimating the models using different estimation methods (maximum likelihood, spatial panel fixed effects regression and spatial two-stage least squares)[15]. We have also considered models in which we allow the error terms to be spatially correlated (SAC). Below we present maximum likelihood estimates of SAR models[16].

Results

Estimates of non-spatial model

Table III gives results of models that do not account for the possibility of spatial correlation and treat override passage as exogenous. In Table IV, override passage is treated as endogenous[17]. For each revenue measure, we first present estimates that are analogous to the most general estimates presented in Downes and Killeen (2014), which were based on national data[18]. We then add assessed value per pupil to the models to allow for the possibility that the use of non-tax revenues is influenced by the ease with which districts can generate property tax revenues.

Explanatory variable	Fees	Fees	Alternative revenues	Alternative revenues	Total revenue	Total revenue
Enrollment/1,000	2.6676	3.4088*	−10.9879**	−11.1849**	−177.0016***	−140.5620**
	(1.8849)	(1.8089)	(4.3789)	(4.4228)	(65.8420)	(54.3926)
Fraction special education	431.2893	447.2963*	−339.1189	−343.3726	−8378.731	−7591.761
	(272.9124)	(265.2628)	(676.3541)	(674.9958)	(6748.794)	(5860.299)
Fraction eligible for free lunch	160.5640	156.5094	671.3935*	672.4709*	7124.824**	6925.483**
	(143.4364)	(139.3838)	(386.0416)	(386.4464)	(3041.014)	(2717.813)
Fraction Asian-American	−795.0265	−805.1446	−1922.519	−1919.830	10042.53	9545.079
	(751.7829)	(752.8561)	(1760.403)	(1763.306)	(22954.19)	(21949.71)
Fraction Native American	−20.5770	69.4426	1050.451	1026.529	15360.44	19786.19
	(293.6716)	(308.6870)	(1096.819)	(1064.241)	(16148.66)	(15835.97)
Fraction African-American	118.5994	113.5878	−197.5091	−196.1774	−5304.647	−5551.037
	(316.5439)	(319.0155)	(985.0527)	(983.6373)	(6257.743)	(5853.296)
Fraction Hispanic	−81.1752	−77.7169	−956.9950***	−957.9140***	−4508.930*	−4338.909*
	(117.1782)	(114.1584)	(366.5502)	(367.1090)	(2519.117)	(2470.590)
State categorical aid	0.0653	0.0665	0.1187	0.1183	3.8544	3.9158
	(0.0537)	(0.0539)	(0.1581)	(0.1581)	(2.7489)	(2.5210)
Federal aid	−0.0242*	−0.0233*	−0.0564	−0.0567	0.9820**	1.0276**
	(0.0131)	(0.0133)	(0.0469)	(0.0469)	(0.4947)	(0.4912)
Override passed	31.9050	13.8388	244.4856***	131.2124***	884.2543**	591.6422
	(21.1586)	(11.0500)	(78.5602)	(42.2931)	(383.5279)	(363.7768)
Override failed	16.2374	5.5995	106.4576*	57.5827*	143.2541	−138.7961
	(17.8180)	(9.3618)	(64.0581)	(34.6097)	(584.7253)	(270.6104)
Assessed value per pupil/1,000	–	0.0904	–	−0.0240	–	4.4469**
		(0.0638)		(0.1017)		(2.1871)
Observations	3,544	3,544	3,544	3,544	3,544	3,544
Districts	180	180	180	180	180	180
Within R^2	0.0450	0.0505	0.0289	0.0289	0.1283	0.1472
F-statistic	3.64	3.61	3.77	3.66	14.70	16.00

Notes: Dependent variable: per pupil revenue measure (in parentheses are standard errors calculated by clustering by school district). Estimation method: linear regression. [a]All regressions include district-specific fixed effects and year effects *,**,***Significant at the 10, 5 and 1 percent levels, respectively

Table III.
Sources of variation in alternative sources of school district revenues[a]

Explanatory variable	Fees	Fees	Alternative revenues	Alternative revenues	Total revenue	Total revenue
Enrollment/1,000	−3.4857	−3.4339	−32.7222	−43.5170*	−666.1852*	−816.1476*
	(12.2538)	(12.8228)	(23.6495)	(26.2148)	(351.3006)	(443.3826)
Fraction special education	532.4997	532.2176	−160.6606	−101.8268	2311.008	3128.339
	(318.7472)	(320.1203)	(1002.047)	(1082.415)	(16905.26)	(20162.21)
Fraction eligible for free lunch	104.6073	105.1157	380.7706	274.7642	−3077.999	−4550.662
	(173.0628)	(175.5039)	(511.8758)	(571.2173)	(9192.549)	(11059.79)
Fraction Asian-American	−160.9776	−155.6303	−1516.064	−2631.049	−92151.54	−107641.1
	(1817.232)	(1817.094)	(4191.873)	(4573.406)	(98596.35)	(114443.8)
Fraction Native American	1259.526	1257.600	5354.425**	5755.968**	79106.14	84684.47
	(1013.716)	(1045.674)	(2668.279)	(2935.181)	(65174.01)	(76638.71)
Fraction African-American	376.7801	378.6176	−206.7954	−589.9396	−40002.30	−45325.02
	(940.9804)	(940.0095)	(2084.417)	(2140.700)	(58975.02)	(69375.30)
Fraction Hispanic	−324.8922	−324.8316	−1593.905*	−1606.533*	−2336.891	−2512.317
	(280.0936)	(280.7792)	(838.7061)	(890.133)	(16852.74)	(19526.52)
State categorical aid	0.1322	0.1323	0.2607	0.2460	3.0153	2.8110
	(0.0895)	(0.0892)	(0.2610)	(0.2832)	(3.9131)	(4.5604)
Federal aid	0.0230	0.0231	0.0488	0.0374	−0.0300	−0.1279
	(0.0387)	(0.0381)	(0.0996)	(0.1081)	(1.9852)	(2.2531)
Override passed	643.7038**	641.4773*	2210.858***	2675.133***	31094.89*	37544.69*
	(325.1222)	(374.6208)	(797.8214)	(1005.710)	(13956.01)	(19825.88)
Override failed	−206.3274	−210.2457	467.3132	1284.346	66912.75***	78263.15***
	(312.4913)	(304.7430)	(703.9138)	(797.9795)	(15422.68)	(20780.90)
Assessed Value per pupil/1,000		0.0024		−0.5031*		−6.9890
		(0.0783)		(0.2812)		(6.5145)
Observations	3,400	3,400	3,400	3,400	3,400	3,400
Districts	170	170	170	170	170	170
Centered R^2	−1.5164	−1.5164	−0.3734	−0.5042	−9.7836	−13.4421
Kleibergen–Papp F	3.968	4.254	3.968	4.254	3.968	4.254
F-statistic	2.72	3.69	2.64	2.57	2.00	1.55

Notes: Dependent variable: per pupil revenue measure (in parentheses are standard errors calculated by clustering by school district). Estimation method: instrumental variables regression (estimated in Stata using xtivreg2). [a]All regressions include district-specific fixed effects and year effects. *,**,***Significant at the 10, 5 and 1 percent levels, respectively

Table IV.
Sources of variation in alternative sources of school district revenues[a]

When we look at the estimated effects that have parallels in the national analysis, there are a few minor differences between the implications of Table III and results for the nation as a whole. However, those differences are not apparent when we instrument for override passage (Table IV). Thus, many of the key lessons from the previously published national results are echoed by the Colorado results.

Starting with the results in Table III, it is worth noting how the results for Colorado do not fully duplicate the results for the nation as a whole. While in our earlier analysis we found that, all else equal, fees and all non-tax local revenues were lower in larger districts, in Colorado fees are higher in districts with higher enrollment. And the negative relationship between federal aid and use of fees and other non-tax revenues evident in Colorado was not apparent in the national results.

Nevertheless, the dominant theme of Table III is that the primary lessons from the national results are echoed by the Colorado results. First, there is no consistent evidence that districts use local non-tax revenues to compensate for reductions in aid[19]. Second, while, once temporally stable variation is taken into account, additional variation in revenues from fees and other non-tax sources is weakly related to student demographics, certain demographic factors do seem to be related to use of alternative revenue sources.

For example, a 1 percentage point increase in the fraction of students designated as special education results in an almost $4.50 increase in per pupil fees. And a 1 percentage point increase in the fraction of students who are Hispanic results in about a $9.60 decrease in alternative revenues.

Most of the results in Tables III and IV tell the same story. In the estimates that underlie Tables III and IV, the estimated coefficients on the year dummy variables for the recession years (2000–2001, 2001–2002, 2007–2008 and 2008–2009) indicate that in Colorado, as in the country as a whole, there is no evidence that districts use fees and other alternative revenues to smooth the impact of economic downturns. In fact, coefficients on the year dummy variables that are not presented suggest that use of fees and all alternative revenues has declined after the Great Recession. The relationship between state aid and alternative revenue use is negligible in Table IV, as it was in Table III. And, while again the impact of student demographics is limited, student racial/ethnic composition does matter. For example, a 1 percentage point increase in the fraction of students who are native American results in about a $58 increase in per pupil non-tax revenues. And a 1 percentage point increase in the fraction of students who are Hispanic results in over a $16 decrease in alternative revenues.

Finally, as has been seen in national results, districts do appear to respond to limits on local revenue-raising ability by making more use of fees and other non-tax revenues. But the Colorado context allows us to provide a more nuanced picture of the nature of the response to limits. The positive relationship between alternative revenue and passage of an override indicates that districts do not view non-tax revenues and override revenues as substitutes. While at first blush this is surprising, there are at least two possible explanations for these results. First, the positive relationship might be driven by the local politics of overrides. The case for an override is stronger if the override is a last resort, with all other revenue sources exhausted. The positive coefficients on both override passed and override failed suggest that districts pursuing overrides work to exhaust nontraditional revenues even as they ask the voters to increase the reliance on the property tax. Effectively, overrides and non-tax revenues appear to be complementary methods for closing gaps created by limits like TABOR.

Alternatively, the positive relationship might simply result from district efforts to maintain relatively constant shares of the different revenue sources. Results from Massachusetts (Downes, 2016) suggest this possibility. Massachusetts, like Colorado, has in place an iconic TEL, Proposition 2½. Massachusetts, like Colorado, permits overrides. However, in Massachusetts, unlike Colorado, overrides and alternative revenues appear to be negatively related. One key difference between Massachusetts and Colorado is that TABOR limits all revenues, while Proposition 2½ only limits property tax revenues. As a result, overrides in Massachusetts are more likely to shift the revenue mix toward property taxes, while TABOR overrides make possible increases in revenues from all sources and allow shares of revenues from all sources to remain stable. Determining whether one of these explanations, or another explanation, is correct is beyond the scope of this paper.

Focusing on a single state makes it feasible not just to provide a more nuanced picture of the link between local tax limits and alternative revenue use but also to account for any role that local property wealth might play. The estimates in Table IV indicate that, if there is any relationship at all, use of fees and other nontraditional revenues are lower in wealthier districts [20]. Alternative revenues do not appear to be accentuating any wealth-based non-neutralities that exist in Colorado, though the heavier use of nontraditional revenues in poorer districts could have troubling implications for the overall progressivity of the revenue system.

Estimates of spatial models

Table V gives estimates of SAR models of revenues[21] when the override dummies are replaced by their fitted values calculated using the first-stage estimates in Table AII[22],[23].

Explanatory variable	Neighbors: districts within 50 miles			Neighbors: districts in same decile of 1991 assessed value per pupil		
	Fees	Alternative revenues	Total revenue	Fees	Alternative revenues	Total revenue
Enrollment/1,000	−3.2027	−45.3482***	−844.1297***	−2.8636	−52.5464***	−816.1198***
	(5.5194)	(11.5852)	(119.1416)	(4.9366)	(13.6960)	(114.8035)
	[−3.3552]	[−45.7128]	[−847.7694]	[−3.0159]	[−37.1782]	[−819.9842]
Fraction special education	507.1623*	−38.7234	3060.639	480.2350*	347.7116	3244.451
	(294.4650)	(762.0726)	(3916.820)	(291.5141)	(896.2291)	(3962.047)
	[517.5208]	[−12.4482]	[3187.908]	[491.3153]	[306.3801]	[3375.637]
Fraction eligible for free lunch	110.7525	245.3369	−5305.488	109.3104	338.6808	−4540.616
	(160.5667)	(393.1170)	(3591.483)	(157.0867)	(496.4907)	(3496.204)
	[108.6032]	[241.5075]	[−5367.632]	[107.4885]	[181.5422]	[−4601.339]
Fraction Asian-American	−71.9964	−2731.521	−105462.6***	−186.8626	−3,248.016	−107,466.2***
	(1404.086)	(2911.228)	(32201.19)	(1392.752)	(3,845.772)	(32,072.59)
	[−139.1778]	[−2882.746]	[−106790.1]	[−255.0527]	[−2,721.350]	[−108,852.5]
Fraction Native American	1,215.474	6,080.808***	89,145.60***	1,180.339	5,565.697***	84,327.13***
	(793.2203)	(1,558.152)	(20,755.23)	(758.5563)	(1,748.427)	(20,340.69)
	[1,251.979]	[6,153.033]	[90,243.30]	[1,217.992]	[3,856.04]	[85,447.30]
Fraction African-American	362.4585	−674.7617	−46,247.88***	376.2061	−1,081.569	−45,741.94***
	(458.3654)	(1,202.271)	(8,199.784)	(458.9057)	(1,483.656)	(8,152.303)
	[347.6354]	[−703.7257]	[−46,587.74]	[362.5508]	[−762.4711]	[−46,106.84]
Fraction Hispanic	−313.2659*	−1,614.063***	−2,038.241	−308.8575*	−1,591.412**	−2,522.143
	(85.5012)	(497.0202)	(2,949.065)	(181.3818)	(606.0434)	(2,957.501)
	[−309.3354]	[−1,598.504]	[−1,920.705]	[−305.6504]	[−1,071.592]	[−2,404.850]
State categorical aid	0.1281**	0.2582	2.9898***	0.1279**	0.4330*	2.8060***
	(0.0601)	(0.1701)	(0.9549)	(0.0605)	(0.2610)	(0.9498)
	[0.1270]	[0.2587]	[2.9962]	[0.1271]	[0.3187]	[2.8113]
Federal aid	0.0252	0.0321	−0.0693	0.0222	−0.0306	−0.1700
	(0.0289)	(0.0779)	(0.5933)	(0.0278)	(0.0951)	(0.6020)
	[0.0245]	[0.0269]	[−0.2043]	[0.0215]	[−0.0281]	[−0.2062]
Fitted value of override passed[b]	620.3889*	2,822.831***	39,833.52***	594.6419*	2,971.501***	37,623.77***
	(367.3290)	(687.9829)	(6,373.919)	(353.6348)	(802.8038)	(6,034.396)
	[629.4164]	[2,833.378]	[40,006.64]	[604.3593]	[2,112.263]	[37,801.62]
Fitted value of override failed[b]	−219.3471	1,360.134	78,824.35***	−207.0348	2,118.719	78,465.07***
	(497.9216)	(1,298.223)	(13,979.01)	(492.0231)	(1,745.230)	(13,831.34)
	[−193.4692]	[1,436.221]	[79,538.63]	[−181.8342]	[1,648.837]	[79,219.30]
Assessed value per pupil/1,000	0.0076	−0.5242***	−7.3516***	0.0078	−0.6478***	−7.1152***
	(0.1027)	(0.1902)	(2.2384)	(0.1007)	(0.2206)	(2.1665)
	[0.0049]	[−0.5308]	[−7.4137]	[0.0053]	[−0.4716]	[−7.1799]
ρ (spatial correlation)	−0.0531	0.0672*	−0.0012	−0.0133***	0.1250***	0.0043
	(0.0584)	(0.0348)	(0.0444)	(0.0038)	(0.0208)	(0.0038)
Observations	3,360	3,360	3,360	3,360	3,360	3,360
Districts	168	168	168	168	168	168
Within R^2	0.0156	0.0001	0.1199	0.0173	0.0011	0.1183
Log of pseudo-likelihood	−21,803.981	−27,356.173	−32,572.777	−21,797.889	−27,357.306	−32,571.573

Notes: Dependent variable: per pupil revenue measure (in parentheses are standard errors calculated by clustering by school district; in brackets are average direct effects). Estimation method: maximum likelihood. Model: spatial autoregressive (SAR) lag specification. [a]All regressions include district-specific fixed effects and year effects; [b]instruments are the difference between each district's revenue and the revenue of a synthetic control district, as well as the square of that difference; the procedure used to create synthetic control districts is described in the Appendix. *,**,***Significant at 10, 5 and 1 percent levels, respectively

Table V.
Sources of variation in alternative sources of school district revenues[a]

We present estimates when "neighbors" are defined as districts within 50 miles and when neighbors are defined as districts in the same decile of 1991 per pupil taxable property wealth[24]. Since, given the nature of spatial models like (1), the coefficient estimates fail to reveal the full impact of a change in an explanatory variable, we also include average direct

effects for each variable. Average direct effects are most directly comparable to the estimates in Tables III and IV.

What these estimates reveal is that, in the Colorado context, there is little if any spatial correlation between fees, all non-tax revenues and total revenues when we define neighbors as geographically proximate districts. When we estimate models that define as neighbors districts with similar per capita incomes or similar per pupil taxable property wealth, the estimates support the conclusion that each district's revenues are correlated with those of its neighbors. This is evident from the final three columns of Table V. But the direct effects differ little from the estimates in Tables III and IV. Spatial links, if they exist, seem to be quantitatively small.

To determine if our findings are driven by bias or true spatial links, we followed Gibbons and Overman's (2012) suggestion and estimated models that included the explanatory variables and spatially weighted averages of the neighbors' explanatory variables. Gibbons and Overman argue that the coefficients on the spatially weighted averages will be jointly significant if there is any spatial relationship[25]. When the dependent variable is fees or total revenues, we cannot reject the null that the spatially weighted averages do not matter, no matter how we define neighbors. And we consistently find spatial relationships when the dependent variable is all non-tax revenues. But, even for alternative revenues as a whole, the implied effects of the explanatory variables are little affected by accounting for spatial correlation, even in those cases where the spatial correlation is significant. In summary, exploring spatial relationships does not seem to be an avenue for learning more about why school districts make such little use of non-tax revenues.

Concluding remarks

Downes and Killeen (2014) found that fees and other locally generated non-tax revenues continue to be relatively little used, even in the aftermath of the severe economic dislocations of the Great Recession. That national examination of trends in alternative revenue use did not lend itself to isolating the types of institutional change that produce significant heterogeneity in tax prices and, as a result, in the incentive to use non-tax revenues. As a result, here we turned to Colorado, a state where fees and other alternative revenues have long been utilized by most of the state's districts and where the role of fiscal institutions can be explored in relatively nuanced ways.

The Colorado context allowed us to provide a more complete picture of sources of variation in the use of non-tax revenues. Earlier work indicated that non-tax revenues were higher in districts subject to TELs. Looking at Colorado allowed us to refine our understanding of the nature of that relationship. In particular, here we find that non-tax revenues appear to be complements to, not substitutes for, property tax revenues that become available to districts when voters override local tax limits.

In addition, while the results for Colorado confirm that variation in use of non-tax revenues is related to variation in district demographics, here we were also able to examine links between district property wealth and use of non-tax revenues, something that is not feasible in national-level studies. Popular discussion of fees and other non-tax revenues suggests that use of those revenues is higher in districts with greater local property wealth. We find that is not true in Colorado, where local property wealth does not appear to be positively related to the extent to which alternative revenues are used, once temporally stable variation is taken into account. If anything, use of these alternative revenues declines as property wealth increases, which is consistent with the property tax being the preferred local revenue source.

But the results above also leave lingering questions. In Colorado, as in the rest of the nation, alternative revenues are not utilized more during economic downturns, nor are such revenues higher in districts receiving less intergovernmental aid. While observers of local

budgeting and fiscal behavior have argued that districts look at their neighbors when deciding on the levels of fees and other non-tax revenues, we find little evidence in support of this argument.

We have thus essentially left open the question why not fees and other nontraditional revenues in times of fiscal distress? Contractions of state aid, new regulation of intergovernmental grants or responses to state or local property tax caps have all been suggested as aspects of how revenue constraints might trigger a response toward fee or user charge mechanisms. The findings in this manuscript suggest that on the whole institutional pressure that does not make property taxes prohibitively costly does not motivate fee or other non-tax revenue usage in school districts.

What we know with certainty is that, even in the face of fiscal constraints, school districts remain dependent on traditional revenue sources. In addition, the primary lesson from this study and its predecessor may be that traditional empirical techniques alone are inadequate to answer the "why" question. Additional state-level case studies, particularly ones that account for the use of non-tax revenues by overlying governments, could assist in answering the question, but are likely to be inadequate. School district decision makers might feel that charging activities fees has the potential to be inequitable and to reduce behavior that generates social benefits or that charging for transportation would increase congestion and reduce ambient air quality near the schools. These concerns would lead school districts to eschew fees, and no traditional empirical technique would tease out the role that these philosophical objections have in constraining the use of non-tax revenues. In other words, the presence of potential philosophical opposition to fees and non-tax revenue in educational finance implies that traditional empirical analyses need to be supplemented with carefully crafted qualitative analyses that include surveys of the actors on the ground, as in Brent and Lunden (2009), that probe attitudes toward fees and other alternative revenue sources.

Notes

1. Articles also discuss the growth of tuition revenues (see Sullivan, 2016) and private contributions (see Rich, 2014) in response to revenue shortfalls.
2. In Table I and elsewhere in the paper, all dollar figures are transformed to 2017 dollars using the CPI-U.
3. After 2010, the mandated growth rate is inflation.
4. We would not expect the growth of nontraditional revenues necessarily to occur during the recessions, both since revenue declines tend to lag the downturns by two to three years (Lutz *et al.*, 2011) and since, in the Great Recession, federal aid replaced much of the lost state aid through fiscal year 2011. Nonetheless, even accounting for these lags in response, we have found no evidence in national data of counter-cyclical movement in nontraditional revenues.
5. Schneider (1989) asserts that limitations on inter-jurisdictional competition result in local governments effectively operating as spatial monopolists. The essence of his argument is that the amount of actual competition that may take place is limited because local governments carve up the market for publicly provided goods. Zax (1989) is one of several papers that document the positive link between inter-jurisdictional competition and the density of local governments.
6. All of the analyses presented below were also executed using as our dependent variable transportation fees and revenues from school lunch. We did this to see if increased use of fees was more evident in those services that are more akin to private goods. None of our substantive conclusions changed when we focused on these specific sources of revenue.
7. Thanks to Carol Schlauder for providing us with scanned versions of the relevant portions of the reports from 1991 to 2003.

8. For example, in the 2003–2004 school year the correlation between the two data sources in total assessed value is 0.9991.

9. Thanks to Eric Brunner for making us aware of this information and for providing us with additional background information on the override elections.

10. If the voters in a district in which an override has failed subsequently pass an override, we set the dummy variable for override failure to 0. Also, in cases where we could determine that an override had sunset (expired) we set the override dummy back to 0 after the sunset date. Of the 170 approved overrides, 2 expired in our period of analysis (one in 2003, one in 2008). One also expired in 2013. That might understate the number of expiring overrides because, in the earliest years, the information we had said nothing about duration. However, given the small number of expiring overrides in the years for which we had good information on expiration, we do not expect our results are sensitive to characterizing expiring overrides as not expiring.

11. Abadie *et al.* (2010) note that root mean squared prediction error in the pre-intervention period provides an indication of how well the trajectory of the outcome variable for the synthetic control approximates the trajectory of that variable in the affected district. While closeness of that trajectory is most important for inference, which is not how we are using the synthetic control, that closeness does give some indication of how good our measure of the shortfall, and the quality of our instruments, is likely to be. The outcome variable in our case is the log of real per pupil current expenditures. For that outcome variable, the median of the root mean squared prediction errors is 0.234, with 25 percent of the values below 0.158 and 75 percent of the values below 0.331. Since, in the pre-intervention period, the median of the log of real per pupil current expenditures was 8.830, with first and third quartiles of 8.630 and 9.016, the root mean squared prediction errors are generally small relative to the quantity being predicted.

12. Colorado districts are also served by Boards of Cooperative Educational Services (BOCES). However, since many BOCES include few districts, we could not have as a neighbor group all districts served by the same BOCES.

13. To limit potential endogeneity issues, we defined income neighbors as districts in the same decile of per capita income in 1990. Taxable wealth neighbors were districts in the same decile of property wealth per pupil in 1991.

14. We provide summary statistics for selected years, rather than summary statistics across all of the years, to provide the reader a feel for the cross-time changes in dependent and explanatory variables. To conserve space, we chose years that divided the period of analysis relatively evenly.

15. We used the Stata routine xsmle (Belotti *et al.*, 2016) to generate the maximum likelihood estimates, the routine spregfext to generate spatial fixed effects regression results, and the routine gs2slsarxt to generate the spatial two-stage least squares estimates.

16. Fowles (2016) argues persuasively that spatial maximum likelihood is preferable to spatial two-stage least squares. In this case, estimates generated using other estimation methods, which are available upon request, are qualitatively similar to the maximum likelihood estimates. And the implications of the SAC models, which are also available upon request, are almost identical to those of the SAR models.

17. Table AII gives the first-stage results. Since the standard statistics used to determine if the model is identified and if the instruments are weak have only been shown to be valid if the errors are independent and identically distributed, conclusions based on these statistics are suggestive. The Kleibergen–Papp rk Wald F for the most inclusive specifications is 4.254, which would suggest a bias relative to OLS of less than 20 percent if the errors were i.i.d. The Anderson–Rubin *F*-statistic of 8.18 suggests that the coefficients on the override and override failure variables are jointly significant at the 1 percent level even if the instruments are weak.

18. The estimates in Downes and Killeen (2014) were from log-linear models, while the estimates here are from linear models. We use the linear specification here to avoid omitting districts that generate no revenue from fees. However, for the Colorado data, the pattern of estimates and the substantive conclusions from log-linear models and from models estimated using a Poisson

specification, which Nichols (2010) has suggested as a method to generate better inferences in situations when zero values of the dependent variable are common, are the same as the pattern of estimates in Tables III and IV.

19. In the results presented here, we use state categorical aid per pupil as our measure of state aid, since that measure is less likely than total state revenue to be endogenous. However, the implications of the estimates are unchanged if we replace state categorical aid with total aid from the state.
20. Downes (2016) found much the same relationship for (Massachusetts) school districts.
21. Estimates of SAC models, which yield the same implications as those of the SAR models, are available upon request.
22. Since the fitted values are pre-estimated quantities, bootstrapping would be the natural procedure for finding asymptotically valid standard errors. However, Belotti *et al.* (2016) argue that standard bootstrapping methods are inappropriate in spatial contexts. As a result, the estimates in Table V should be interpreted as suggestive.
23. Estimates of the SAR models with the override variables treated as exogenous are available upon request. The implications of these estimates are nearly identical to the implications of the estimates in Table V.
24. We have also estimated models in which neighbors are districts served by the same regional field office and districts in the same decile of per capita income in 1990.
25. Case *et al.* (1993) make a similar argument.
26. We used Jens Hainmueller's Synth package for Stata to construct each district's synthetic control.
27. We tried other subsets of states, including those states that had no local tax and expenditure limit prior to TABOR, but found the states chosen by Eliason and Lutz generated the best pre-TABOR matches.

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Appendix

To construct our instruments for override passage and failure, we started with the literature on the determinants of override votes. Early work by Bradbury (1991) and more recent work by Wallin and Zabel (2011) suggest that local fiscal conditions are a strong determinant of override success. For example, Bradbury finds that overrides are less likely to be successful in Massachusetts towns subject to Proposition 2½ when those towns have more excess capacity, as measured by the gap between the towns actual levy and the levy limit imposed by Proposition 2½. Similarly, Wallin and Zabel find that overrides of Proposition 2½ are less likely when the gap between current revenues and the cost of providing a target level of services is larger. Thus, we expected that overrides would be more likely to be successful if the district's shortfall, or its gap between desired revenues and revenues as limited by TABOR was larger.

To construct a shortfall measure for each district, we followed the logic of Eliason and Lutz (2018), who used the synthetic control methodology to construct a measure of the post-TABOR gap in tax revenues for Colorado. Using the synthetic control approach, we can use pre-TABOR data to construct a set of control group of non-Colorado districts for each district in Colorado. The synthetic control methodology, which is described in Abadie *et al.* (2010), uses data on the log of pre-TABOR total revenues and potential determinants of those revenues to select the control districts and determine the weight each control district should receive. The log of desired post-TABOR revenues for the Colorado district are then the weighted average of the logs of the actual revenues of the control districts.

Searching over all districts in the country to find potential controls would have been prohibitively time-consuming[26]. To make the search feasible, we began by narrowing consideration to districts in a subset of states. We settled on the states chosen by Eliason and Lutz in their analysis of TABOR to provide candidates for control districts[27]. That meant the control districts were chosen from Arizona, Hawaii, Kansas, Maryland, Nevada, New Hampshire, New York, Utah and Washington.

For all districts, post-TABOR expenditure data were drawn from the Common Core of Data. We turned to the 1972, 1977, 1982 and 1987 Censuses of Government and the 1970, 1980 and 1990 Decennial Censuses for the pre-TABOR information needed to select control districts. Not all districts in the USA were observed consistently back to 1972; we have financial data on 10,143 districts in 1972, with demographic data on 9,632 of those districts from the 1970 Census. Nevertheless, we had about 1,600 districts from which to select the control group for each Colorado district. And we had data going back to 1972 on 168 of the districts in Colorado.

The shortfall for each district was the weighted average of the log of total revenues in the control districts minus the log of the actual revenue of each Colorado district post-TABOR. The combination of TABOR and the Gallagher Amendment has meant that, for districts that have failed to pass an override, revenues are at or very near the revenue limit created by TABOR. While we could not get data on each district's revenue limit in each year, several sources (Great Education Colorado, 2016a; Brown, 2000; Resnick *et al.*, 2015) note that district revenues are at the TABOR limits. Thus, revenue moves exogenously in districts that have failed to pass overrides.

Post-TABOR, revenue in each synthetic district is a weighted average of revenue in the districts that compose the synthetic control. Since the districts used to create each synthetic district are drawn from states other than Colorado, any movement in their spending will be exogenous to Colorado districts. As a result, the shortfall, which is the difference between the log of revenues in the synthetic district and the log of the constrained revenues in the Colorado district, is the difference between two exogenous quantities. Thus, the shortfall is exogenous. Our instruments were constructed from these yearly shortfall measures. The resulting first-stage estimates follow in Table AII.

Table AI. Summary statistics – explanatory variables

Variable	1991–1992 Observations	1991–1992 Mean	1991–1992 SD	1996–1997 Observations	1996–1997 Mean	1996–1997 SD	2001–2002 Observations	2001–2002 Mean	2001–2002 SD
Enrollment	176	3,368.222	9,047.734	176	3,821.784	10,052.234	178	4,164.714	10,899.446
Fraction special education	176	0.084	0.035	176	0.094	0.035	178	0.101	0.030
Fraction eligible for free lunch	176	0.251	0.150	176	0.258	0.153	178	0.245	0.145
Fraction reduced-price lunch eligible	–	–	–	–	–	–	178	0.101	0.057
Fraction limited English proficient	–	–	–	–	–	–	178	0.050	0.071
Fraction African-American	176	0.011	0.033	176	0.013	0.037	178	0.016	0.036
Fraction Asian-American	176	0.008	0.012	176	0.009	0.013	178	0.011	0.014
Fraction Native American	176	0.009	0.028	176	0.010	0.029	178	0.013	0.034
Fraction Hispanic	176	0.158	0.186	176	0.172	0.186	178	0.197	0.195
Total state aid per pupil	176	4,104.008	2,166.495	176	4,921.323	2,246.308	178	5,657.721	2,538.119
Federal aid per pupil	176	532.834	443.035	176	414.426	323.275	178	558.329	528.114
Override passed	176	0	0	176	0.080	0.271	178	0.253	0.436
Override failed	176	0	0	176	0.074	0.262	178	0.067	0.251
Assessed value per pupil	176	128,084.260	149,717.500	176	120,042.580	152,962.790	178	142,423.560	176,293.100

Variable	2006–2007 Observations	2006–2007 Mean	2006–2007 SD	2012–2013 Observations	2012–2013 Mean	2012–2013 SD
Enrollment	178	4,439.388	11,375.502	178	4,781.253	12,542.760
Fraction special education	178	0.101	0.030	178	0.101	0.030
Fraction eligible for free lunch	178	0.302	0.158	178	0.366	0.160
Fraction reduced-price lunch eligible	178	0.103	0.050	178	0.108	0.050
Fraction limited English proficient	178	0.080	0.109	178	0.068	0.086
Fraction African-American	178	0.011	0.025	178	0.013	0.028
Fraction Asian-American	178	0.010	0.012	178	0.009	0.012
Fraction Native American	178	0.009	0.019	178	0.008	0.021
Fraction Hispanic	178	0.169	0.157	178	0.181	0.161
Total state aid per pupil	178	6,168.535	2,444.738	178	5,183.433	3,338.568
Federal aid per pupil	178	707.845	546.601	178	815.830	732.928
Override passed	178	0.399	0.491	178	0.455	0.499
Override failed	178	0.084	0.279	178	0.208	0.407
Assessed value per pupil	178	187,434.610	250,424.770	178	261,513.510	433,038.670

Variables	Override passed	Override failed
Enrollment/1,000	0.0110 (0.0139)	0.00344 (0.0100)
Fraction special education	−0.1526 (0.3595)	0.0091 (0.2411)
Fraction eligible for free lunch	0.0320 (0.1685)	0.1226 (0.1336)
Fraction Asian-American	−0.4400 (1.8505)	1.7174 (1.8935)
Fraction Native American	−1.7826** (0.8181)	0.0039 (0.9489)
Fraction African-American	−0.2084 (0.9463)	0.6094 (1.1649)
Fraction Hispanic	0.4237 (0.2843)	−0.2063 (0.2305)
Federal aid	−0.0000728*** (0.0000207)	0.0000415* (0.0000214)
State categorical aid	−0.0000877 (0.0000886)	0.0000193 (0.000059)
Assessed value per pupil/1,000	1.44e−04 (1.11e-04)	5.48e−05 (8.34e−05)
Linear term of orthogonal polynomial from shortfall measure	0.0565*** (0.0216)	−0.0078 (0.0148)
Quadratic term of orthogonal polynomial from shortfall measure	−0.0148 (0.0105)	0.0181*** (0.0066)
School year effects		
1994–1995	0.0663*** (0.0208)	0.0556*** (0.0197)
1995–1996	0.0611*** (0.0209)	0.0775*** (0.0224)
1996–1997	0.0744*** (0.0233)	0.0789*** (0.0226)
1997–1998	0.1103*** (0.0289)	0.0588*** (0.0214)
1998–1999	0.1385*** (0.0314)	0.0744*** (0.0237)
1999–2000	0.1965*** (0.0354)	0.0776*** (0.0262)
2000–2001	0.2539*** (0.0384)	0.0710*** (0.0297)
2001–2002	0.2709*** (0.0402)	0.0612** (0.0275)
2002–2003	0.2950*** (0.0423)	0.0680** (0.0294)
2003–2004	0.3390*** (0.0460)	0.0583* (0.0298)
2004–2005	0.3622*** (0.0477)	0.0649** (0.0329)
2005–2006	0.3980*** (0.0498)	0.0661** (0.0328)
2006–2007	0.4315*** (0.0482)	0.0562* (0.0290)
2007–2008	0.4295*** (0.0479)	0.0696** (0.0314)
2008–2009	0.4446*** (0.0483)	0.1146*** (0.0353)
2009–2010	0.4427*** (0.0500)	0.1114*** (0.0374)
2010–2011	0.4913*** (0.0536)	0.1265*** (0.0376)
2011–2012	0.4297*** (0.0518)	0.1910*** (0.0394)
2012–2013	0.4522*** (0.0521)	0.1693*** (0.0380)
Constant	−0.0698 (0.0884)	−0.0569 (0.0651)
Observations	3,400	3,400
Districts	170	170
R^2	0.235	0.069
F-statistic for excluded instruments	3.52	3.87
Kleibergen–Paap Wald rk *F*-statistic	4.254	

Notes: Both specifications include district-specific effects. In parentheses are standard errors robust to heteroskedasticity and calculated by clustering by school district. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

Table AII.
First-stage estimates for override passed and override failed dummy variables

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