FISCAL SPILLOVERS BETWEEN LOCAL GOVERNMENTS:
KEEPING UP WITH THE JONESES’ SCHOOL DISTRICT

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ABSTRACT

While there is a rapidly growing literature concerning fiscal spillovers between local governments, it is challenging to empirically distinguish spillovers from common underlying trends. Using financial and geographic data for every school district in the U.S. from 1972 to 2002, this paper tests for fiscal spillovers using a new type of instrumental variable strategy that tests whether districts located near state borders respond to the predicted fiscal behavior of neighboring districts located in a different state. The fiscal behavior of the out-of-state neighboring districts will often be driven by changes in their states’ finance formulas or other state-specific shocks that are plausibly unrelated to factors affecting the in-state, border district. Additional specifications examine the extent to which local spillovers across state borders differ from in-state local spillovers. The results confirm that districts' revenues are influenced by exogenous shocks in their neighbors' revenues, especially for districts that were already outspending their neighbors. On average, a one dollar increase in the mean revenues per pupil of nearby districts leads to about a 22 cent increase in a district’s own revenues per pupil, which is smaller than estimates derived from ordinary least squares models or traditional instrumental variables models. Spillovers occur regardless of whether districts use direct or representative democracy to determine local tax revenue levels, and there is suggestive evidence that positive spillovers are mitigated by household mobility across districts in metropolitan areas. Based on these results, policymakers should consider that school finance policies targeted towards particular districts can change the equilibrium spending levels and residential composition of other districts.
1. Introduction

In order to fully understand the political economy of local government spending and taxation decisions, one must understand whether one locality’s decision will influence other localities’ decisions. Fiscal spillovers could result from fiscal competition between localities and from households’ residential location decisions. Empirically, it is difficult to determine how much of the correlation in the tax and expenditure decisions of localities is due to spillovers as opposed to common underlying trends influencing these decisions, (e.g., unobserved demographic changes, common responses to changes in the local fiscal environment, etc.). If some portion of the changes in nearby localities’ fiscal changes is exogenous, then one can use an instrumental variables approach to identify the impact of spillovers. Yet most factors influencing nearby districts’ expenditures are also directly related to a district’s own expenditures, so it is difficult to find a valid instrumental variable.

This paper empirically tests for fiscal spillovers between U.S. public school districts using a new type of instrumental variable strategy. The strategy examines whether districts located near state borders respond to the predicted fiscal behavior of neighboring districts located in a different state. The fiscal behavior of the out-of-state neighboring districts will often be driven by changes in their states’ finance formulas or other state-specific shocks that are plausibly unrelated to factors affecting the in-state, border district. While this out-of-state-neighbor instrumental variable strategy is more likely to be exogenous than other types of instruments, other specifications are used to determine whether local spillovers across state borders appear to be different from in-state local spillovers. The empirical analyses include several robustness checks from which one can infer both the internal validity and external validity of the estimates derived from each approach.

The results suggest that a one dollar increase in the mean per pupil operating revenues of nearby districts causes a district to increase its own per pupil operating revenues by about 22 cents. This estimate is substantially lower than the corresponding estimate from an ordinary least squares model (37 cents) and this estimate is moderately lower than the corresponding estimate from a more traditional instrumental variable approach (28 cents). While this paper cannot rule out several theoretical explanations for the mean response of about 22 cents, exploring heterogeneous responses helps to reveal which mechanisms are most consistent with
observed behavior. The magnitude of fiscal spillovers is sensitive to whether a district is initially outspending its neighbors and is sensitive to whether districts are located in metropolitan areas. Spillovers occur regardless of whether districts use direct or representative democracy to determine local tax rates funding public school revenues.

The next section briefly summarizes the theoretical reasons why school districts’ operating revenues might be influenced by the operating revenues of nearby districts. Sections 3 and 4 describe the empirical methodologies and data used to test for spillovers. Section 5 presents the main results, Section 6 presents additional analyses which shed light on the mechanisms for fiscal spillovers, and Section 7 briefly concludes with a discussion of the implications of these findings.

2. Theoretical Background and Prior Empirical Research

There are several mechanisms by which school districts’ fiscal decisions may affect the fiscal decisions of nearby districts. There are three mechanisms which would cause a positive correlation between nearby districts’ revenue levels. First, there may be traditional tax competition, school districts restraining tax rates in order to compete for residents and/or businesses who might locate in one of the districts (see, for example, Wilson, 1999; Brueckner, 2000; Brueckner & Saavedra, 2001; Brueckner, 2003). Maintaining a relatively low tax rate may benefit some of a district’s residents if it leads to higher property values and a higher local tax base, or if it helps to attract businesses and commerce that improve employment and private revenues. Second, there may be service competition, school districts increasing revenues in order to attract students to the local public schools or to gain popularity among households with children. For example, increasing the number of advanced placement classes at the local high school to compete with a nearby high school might improve a district’s popularity among households with children in the region; school district officials might therefore decide to spend more on advanced placement classes, even if this leads to higher local property tax rates and does not lead to positive capitalization effects. A third mechanism could occur regardless of

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1 See Brueckner (2003) and Revelli (2005) for excellent summaries of the recent empirical literature concerning fiscal spillovers between governments and discussions of why it is very difficult to empirically distinguish various potential sources of fiscal spillovers.

2 Using a model in which local officials can extract rents by using public revenues for private benefits, Wilson and Gordon (2003) show that local officials’ concern about the size of local property tax bases can also decrease the share of local revenues devoted to socially wasteful spending.
whether student mobility is a concern—there may be informational spillovers, whereby a
district’s residents interpret the behavior of neighboring districts as an informative signal which
guides their voting behavior. Given the difficulty of measuring public school quality, residents
may feel that they need to spend money on local schools to “keep up with the Joneses” who live
in a nearby district.

In addition to these three mechanisms, there are two other mechanisms which could cause
either a positive or a negative relationship between nearby districts’ expenditures. There may be
*Tiebout* (1956) re-sorting after one district, for some exogenous reason, changes its expenditure-
tax bundle. This change might induce relocation decisions of people or businesses into nearby
districts, and this in turn could alter the aggregated social preferences in these nearby districts.
Tiebout re-sorting is distinct from tax competition, because it is unrelated to a district’s strategic
concern about its tax base. A district simply experiences a shift in aggregated social preferences
as people relocate there to form better matches between their own preferences and the district’s
expenditure-tax bundle.³ Using a computable general equilibrium model, Nechyba (2003) finds
that changes in the amount of state aid targeted to one district influence the spending levels of
nearby districts, as some households move across districts and some shift consumption between
the private and public schooling sectors.⁴ Finally, there may be *externalities*, whereby greater
levels of services provided by neighboring districts create an incentive to either expand or cut
back on a district’s own services. For example, perhaps the presence of field hockey teams in
neighboring school districts increases the benefit of adding a team. Alternatively, perhaps the
presence of a high revenue district nearby enables a district to maintain relatively low revenues
and still attract businesses that employ adults with school-aged children.

Several studies have empirically investigated the topic of fiscal spillovers in the United
States at the state or county level. To identify spillovers, many of these studies argue that certain

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³ Given imperfect Tiebout sorting, there is within-district variation in citizens’ preferences for local public
school expenditures. Changes in expenditures could induce the relocation of citizens with relatively high
or relatively low demand for local public school expenditures.

⁴ In Nechyba’s (2003) analysis, a block grant targeted towards the lowest income district leads to
increases in the equilibrium spending per pupil in both a medium-income district and a high-income
district, whereas an equivalent-sized matching grant targeted towards the lowest income district leads to
an increase in the equilibrium spending of the medium-income district and a decrease in the equilibrium
spending of the high-income district. These changes are much larger than those implied by the estimates
of average fiscal spillovers below, as grants lead to substantial capitalization effects in Nechbya’s
analysis.
variables used in their analyses are only correlated with a government’s expenditures through their effects on neighboring governments’ expenditures (Case, Hines, & Rosen, 1993; Figlio, Kolpin, & Reid, 1999; Baicker, 2004; Baicker, 2005). In their seminal study, Case, Hines, and Rosen (1993) identify the fiscal interdependence of state expenditures by assuming that demographic trends in a neighboring state are only correlated with a state’s own public expenditures via changes in the neighboring state’s expenditures. Their study reveals that a state’s own expenditures are not positively influenced by the spending of contiguous states, those that are geographic neighbors. Rather, their study reveals fiscal spillovers between similar states that are not necessarily geographically proximate. Figlio, Kolpin, and Reid (1999) and Baicker (2005) use policy variables to predict changes in states’ welfare expenditures and Medicaid costs respectively. Defining “neighbors” as states with high rates of cross-migration, Figlio et al. (1999) find that states respond to their neighbors’ welfare programs, especially when these programs become less generous. Baicker (2005) finds that a 10% increase in state expenditures causes neighboring states to increase expenditures by between 3.7% and 8.8%. In another study, Baicker (2004) cleverly uses data concerning capital punishment trials to show that counties are likely to increase both expenditures and revenues when a neighboring county experiences an unanticipated increase in taxes.

Previous studies of fiscal spillovers via informational spillovers have generally focused on “yardstick competition” (e.g., Besley and Case, 1995), a framework in which elected officials’ actions are constrained by nearby governments’ actions in order to achieve sufficient political popularity in a context in which citizens have limited information concerning government productivity. Empirical studies have tested for yardstick competition by examining whether fiscal interdependence decreases when elected representatives determining tax rates are no longer eligible for re-election due to term limits (e.g., Case, 1993; Besley & Case, 1995; Bordignon, Cernigliga, & Revelli, 2003), when elected officials enjoy overwhelming political support (e.g., Allers & Elhorst, 2005; Bordignon, Cernigliga, & Revelli, 2003; Solé Ollé, 2003), or when governments are subject to a performance rating system (Revelli, 2006). These studies find evidence that fiscal spillovers occur primarily in the context of elected officials following neighboring governments’ actions when these officials are most concerned about their political capital.5

5 Please see Revelli (2005) for a more detailed review of these and related studies.
While there has not previously been a national study of fiscal competition between U.S. school districts, there have been a few prior empirical studies investigating fiscal spillovers between municipalities or school districts in specific states. Brueckner and Saavedra (2001) investigate fiscal spillovers between 70 municipalities in the Boston area. They empirically test for spatial endogeneity in their models, and these tests fail to reject the null hypothesis that their independent variables are exogenously determined. Brueckner and Saavedra find evidence of positive spillovers between the municipalities, and they also find evidence that these spillovers disappeared after Proposition 2½ limited most Boston-area municipalities’ ability to increase local property taxes. While there is not strong empirical evidence of spatial endogeneity in these Boston-area data, one would expect spatially-correlated, unobserved variables to become increasingly important as one expands the sample to include a greater number of localities spread across a wider geographic area. Millimet and Rangasprad (2007) find evidence of fiscal spillovers between school districts in Illinois. Their study thoughtfully addresses the difficulties of separating spillovers from unobserved trends, though their empirical approaches might not fully address this problem. Babcock, Engberg, and Greenbaum (2005) find evidence of fiscal competition specifically related to public school teacher salaries in Pennsylvania districts. They find that a district’s salaries are highly influenced by previously established salaries in a comparison group of districts, defined by the contract negotiators. This provides an example of one of the many mechanisms through which a school district’s operating revenues may respond to changes in other districts’ operating revenues, though, in the case of Pennsylvania teacher contracts, the interdependence of revenues does not necessarily correspond with geographic proximity.

3. Methodology

Define \( R_{ijt} \) as the real operating revenue per pupil in district \( i \) located in state \( j \) during year \( t \). Suppose that changes in district \( i \)'s revenues per pupil are influenced by recent changes in the mean revenues among neighboring districts, as well as trends related to state finance policies and

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6 Millimet and Rangaprasad (2007) use a variety of specifications. One approach is an instrumental variables model similar in spirit to the one used in Case, Hines, and Rosen’s (1993) state-level analyses. In the local setting, however, these types of instrumental variables are likely to be endogenous due to residential mobility. Another approach is using lagged neighbor spending decisions, which is problematic if unobserved, common factors take different amounts of time to influence neighboring districts’ expenditures.
district \(i\)'s demographics. Define \(W_1\) and \(W_2\) as weighting matrices based on geographic proximity, where \(W_1\) is the relevant weighting matrix for determining responses to neighborly fiscal behavior and \(W_2\) is the relevant weighting matrix for autocorrelated error terms. In the baseline models, neighborliness is based on districts having centroid coordinates located within thirty miles of each other, and \(W_1\) is thus constructed so that multiplying \(W_1\) by a vector of values yields a vector with the mean values among districts located within thirty miles of each district.\(^7\) The relationship between changes in a district’s per pupil revenues and changes in neighboring districts’ revenues may be expressed as:

\[
R_{ijt} - R_{ijt-5} = \beta_1 W_1 (R_{ijt} - R_{ijt-5}) + \gamma_{jt} \beta_2 + \gamma_{jt} X_{ijt-5} \beta_3 + e_{ijt}
\]

\(e_{ijt} = \lambda_t W_2 e_{ijt} + \epsilon_{ijt}\)

with \(|\beta_1| < 1, |\lambda_t| < 1 \forall t, \) and \(\epsilon_{ijt} \sim N(0, \sigma^2),\)

where \(\gamma_{jt}\) represents a vector of state-year indicator variables and \(\gamma_{jt} X_{ijt-5}\) represents these indicator variables interacted with lagged control variables capturing district \(i\)’s characteristics.

While one could alternatively use a district fixed effects model to analyze fiscal spillovers, using this first-differenced dependent variable facilitates estimation of some of the more refined instrumental variable specifications described below.\(^8\) One could also examine the impact of lagged changes in neighbors’ revenues, but examining contemporaneous changes appears to sufficiently capture districts’ responses in the instrumental variables models below.\(^9\)

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\(^7\) Throughout this paper, distances between districts are based on distances between their centroid coordinates. Minimum distances between districts’ borders are not readily available in these data.

\(^8\) The main estimate of fiscal spillovers below is strikingly similar if one instead uses a district fixed effects model over the same sample period, (22.7 cent point estimate of fiscal spillovers for the district fixed effect model analogous to the first-differences model yielding a 22.2 cent estimate in row 1 of the second-to-last column of Table 2). Equation 1 can be derived from a framework in which the level of revenues in district \(i\) during year \(t\) is a function of the levels of revenues in other districts and of district \(i\)’s current demographics, i.e., \(R_{ijt} = f(W_1 R_{ijt}, X_{ijt})\). First-differencing these variables facilitates estimation of some of the instrumental variables described below, which exploit variation in changes in revenues coinciding with school finance reforms or other, major statewide changes. While the empirical models first-difference the revenues variables, changes in district \(i\)’s observed characteristics are endogenous; instead of first-differencing the demographic characteristics, the empirical models thus control for the state-by-year effects of detailed lagged demographic variables, which will be correlated with the exogenous component of districts’ demographic changes.

\(^9\) For example, in the main out-of-state instrumental variable below, if one adds lagged terms for the first two right-hand side terms in equation 3 below, then the estimated coefficient of the predicted neighbors’ change from the prior five year period is statistically insignificant and the estimated coefficient of the predicted contemporaneous neighbors’ change experiences a statistically insignificant decline from 22.2 cents to 19.1 cents.
The major challenge for estimating Equation 1 is that revenue changes, $R_{ijt} - R_{ijt-5}$, are endogenous. Unobserved shocks should have similar influences on a district’s own revenues and the revenues of neighboring districts. The empirical analyses in this paper address the endogeneity of neighboring districts’ expenditure changes by using plausibly exogenous predictors of their revenue changes as instrumental variables. The general form of the resulting model is:

$$R_{ijt} - R_{ijt-5} = \beta_1 W_1 \left( R_{ijt}^\wedge - R_{ijt-5}^\wedge \right) + \gamma_j \beta_2 + \gamma_j X_{ijt-5} \beta_3 + \epsilon_{ijt}$$

(2)

$$\epsilon_{ijt} = \lambda_t W_2 \epsilon_{ijt} + \epsilon_{ijt}$$

with $|\beta_1| < 1$, $|\lambda_t| < 1 \ \forall \ t$, and $\epsilon_{ijt} \sim N(0, \sigma^2)$. The control variables in the $X_{ijt-5}$ vector used in the models below include detailed, lagged demographic characteristics: state-year specific quartile spline terms for mean house value and for median income of residents, as well as variables measuring population density, the fraction of the district’s population composed of school-aged children (ages 5-17), and the fraction of the district’s population composed of people who are at least 65 years old. Given that the demographic effects are allowed to vary by state and by year, these variables will pick up the impact of state-specific factors and policies.

Due to the large sample size and ensuing lengthy computational time, most of the instrumental variable models below do not adjust the standard errors for spatial or serial autocorrelation, but instead assume that the $\lambda_t$’s equal zero. (See footnote 25 for a discussion of how the standard errors change for the main results when one adjusts for autocorrelation.)

The analyses below include estimates for three general types of instrumental variables strategies. The first strategy is a two-stage least squares model using the independent variables to predict revenue changes in neighboring districts and then examining the impact of those predicted changes. Given that this general type of approach has been used by prior studies, results from this first approach should help to reveal whether the novel instrumental variable methods used below produce substantially different estimates.

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10 This specification assumes that lagged house prices are exogenous because they do not reflect anticipated future changes in local taxes and expenditures; if one instead estimates similar models that do not control for lagged house values, then the estimates of fiscal spillovers below are moderately larger (e.g., .260 instead of .222 in row 1 of the second-to-last column of Table 2).
The second instrumental variables methodology assumes that the impact of any spatially-correlated unobserved variable dies out beyond a certain distance. Districts within the same state during the same year are compared based on five lagged characteristics: operating revenues per pupil, mean income, median house value, population density, and the fraction of the population composed of school-aged children (ages 5 to 17). I identify which four districts in the same state as district X and located at least some minimum distance from district X are most similar to district X based on these lagged characteristics. The average actual change in revenues among these four comparison districts is used as an instrumental variable for the change in district X’s revenues. For the main analyses, I use a minimum distance of 100 miles for the comparison districts. Using a shorter minimum distance would inflate estimates to the point where they are almost as large as ordinary least squares estimates, ostensibly because spatially-correlated, unobserved factors bias the results.

It is possible that districts respond to the behavior of other, similar districts located more than 100 miles away. While the estimates would still reflect fiscal spillovers rather than unobserved shocks, this could cause a serious interpretation problem because what would seem like a local response is really a response to similar but distant districts. Additional analyses, which specifically examine spillovers between observationally similar districts, discredit the notion that spillovers occur between school districts that are similar but geographically distant.

While this second instrumental variables methodology might be an improvement over the simpler two-stage least squares approach, this second approach could still be seriously flawed if spatially-correlated unobserved shocks do not die out beyond 100 miles. This problem could occur if a change in a state policy, such as a statewide school finance equalization program, leads to differential fiscal responses across districts and if districts’ responses tend to be related to unobserved variables that are correlated with their neighbors’ observed characteristics. This

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11 Observations for 1977, 1982, and 1987 are matched based on only four characteristics because house value information is not available from the 1970 Census.
12 To determine similarity, I compute the Z-score for each of these five variables among observations in the same state and year, and then compute an index of dissimilarity equal to the sum of the squared differences between district X’s Z-score and the Z-score of the comparison district.
13 Using a minimum distance of 50 miles for this paper’s main all-neighbor IV model, (row 1 of Table 2), would produce an estimate of fiscal spillovers of .344, compared to the corresponding ordinary least squares estimate of .374.
scenario could cause estimates of fiscal spillovers to be biased in either direction. In order to produce estimates of spillovers that are immune to these problems, the preferred specifications below use “out-of-state” instrumental variable models.

3.1 Baseline out-of-state neighbor IV specification

The out-of-state neighbor instrumental variables models identify fiscal spillovers based on instruments that are extremely likely to be exogenous, because their validity lies only on the assumption that unobserved shocks do not spread more than 100 miles across state borders. Spillovers are identified based only on the predicted values for neighboring districts in other states. Furthermore, these out-of-state neighbors’ predicted values are based on matches with districts that are not only located at least 100 miles away but are also at least 100 miles away from the relevant state border. The identification thus comes from trends in the neighboring state among districts that are similar to the border districts but are located far away from a district’s own state.

For the N school districts in the sample, define S as an N×N matrix with element $S_{ir}$ equal to one if districts $i$ and $r$ are in the same state and equal to zero otherwise. Define A as an N×N matrix of ones. To identify districts’ responses to their mean neighbors’ change solely from responses to out-of-state neighbors, one may use S to partition the mean change in neighbors’ revenues on the right hand side of equation 2, using instrumented values for out-of-state

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14 For example, suppose that Ohio enacts a school finance reform that provides more generous funding to districts with low levels of commercial property wealth. Suppose further that, controlling for a districts’ observable characteristics, districts tend to have higher levels of commercial property wealth if the districts’ neighbors have higher median resident incomes. Finally, suppose that, aside from Ohio’s commercial property wealth reform, there was a coincidental trend in Ohio whereby districts with relatively high median residential incomes chose to increase their school operating expenditures far more than other districts. In this case, one would underestimate fiscal spillovers between neighboring districts in Ohio, because a district with neighbors with high [low] median residential income would have a high [low] predicted change in mean neighboring district revenues per pupil but, controlling for its own median income, would have a relatively low [high] change in revenues per pupil due to its relatively large [small] commercial property wealth. Conversely, if Ohio’s reform made the finance policy less generous towards districts with low commercial property wealth, then one would overestimate fiscal spillovers using this type of instrumental variables model.

15 For the out-of-state IV models, 100 miles appears to be a sufficiently far cutoff for eliminating spatial endogeneity. Using a 150 mile cutoff produces an estimate of .172 (.043 standard error), which is moderately less than the corresponding out-of-state IV estimate of .222 in row 1 of Table 2 based on a cutoff of 100 miles.
neighbors’ changes and state-by-year indicators to absorb the impact of in-state neighbors’ changes:

\[
R_{ijt} - R_{ijt-5} = \beta_1 W_1 (A-S) (\hat{R}_{ijt} - \hat{R}_{ijt-5}) + (\beta_4 W_1 S + \beta_2 \gamma_{jt} + \gamma_{jt} X_{ijt-5} \beta_{3jt} + e_{ijt}
\]

(3)

\[
e_{ijt} = \lambda_i W_2 e_{ijt} + \epsilon_{ijt}
\]

with \(|\beta_1|<1, |\lambda_i|<1 \forall t, \text{ and } \epsilon_{ijt} \sim N(0, \sigma^2)\).

While the model continues to include all districts in the continental United States, the estimates of fiscal spillovers are identified from \(\beta_1\) on the first right-hand-side term, the response to predicted changes in out-of-state neighbors’ revenues. The partitioning of the neighbor weighting matrix into the first two right-hand side terms in equation 3 allows \(\beta_1\) to be identified not only by differences in the types of out-of-state neighboring districts, but also by variation in the proportion of a districts’ neighbors that are out-of-state. If the magnitude of spillovers across out-of-state districts differs from spillovers across in-state districts, then estimates of \(\beta_1\) from the first right-hand-side term in equation 3 will yield unbiased estimates of spillovers across out-of-state districts but not of spillovers between in-state districts. The next three subsections describe additional out-of-state IV specifications which provide robustness checks for the validity of this instrument and the applicability of the estimates to spillovers between in-state districts.

3.2 Out-of-state neighbors in states with major, unique finance reforms

There is prior empirical evidence that one state’s spending does not influence the spending of contiguous states (Case, Hines, & Rosen, 1993), but if districts in contiguous states have similar expenditure trends, then even the out-of-state neighbor IV might be endogenous. In the extreme case, contiguous states undergo identical finance trends and the estimates from equation 3 face the same potential pitfalls as estimates from equation 2.

To examine the importance of this issue, I estimate additional models which identify fiscal spillovers solely from cases in which one state has a significant finance reform while the neighboring state does not have a similar change. One state’s education finance reform has differential effects on its own border districts, and the lack of a similar policy within the neighboring state allows for a relatively clean case of identifying the impact on various border districts in the neighboring state. Ideally, one would instrument for districts’ revenue changes using only changes in state’s education finance policies, but it is extremely difficult to do so. It
is very difficult to accurately measure historical variation in each district’s local tax price and foundation level of state aid for a sufficient number of years, and, even if one were able to do so, the timing of districts’ responses to changes in these measures is highly variable. This makes using a district-specific school finance instrument problematic for identifying the timing of fiscal spillovers across districts. Instead, I allow the data to reveal the timing of states’ substantial education finance changes and I test whether instrumental variables models focusing on the occurrence of these substantial changes produce similar results as the baseline instrumental variables model. I divide each state’s districts into three groups based on income and examine whether the mean change in revenues was large for any of these groups. I define a large change as a group having a mean increase of more than $2,000 per pupil or a mean decrease of more than $1,000 per pupil, (in year 2000 dollars), changes equivalent to the 72nd or 22nd percentile in the distribution of individual district-level revenue changes. This definition of major finance changes matches up well with lists of court-ordered and state legislature-initiated school finance equalization programs compiled by Downes and Shah (2006) and Corcoran and Evans (2007): except for Montana and South Dakota, all states with court-ordered or state legislative reforms experienced this type of major finance change at least once during the ten years following these reforms. Next, I identify fiscal spillovers by using the instrumental variable only in cases in which a district has out-of-state neighbors in a state with large changes among one of the income groups and the district’s own state did not experience a “similar change” for its corresponding income group. I define a “similar change” as a change that is at least 50% as large as the other state’s change and in the same direction. Identifying spillovers from major, unique changes in states’ finances reduces the possibility of a biased estimate due to states engaging in similar

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16 This empirical approach may be more promising for a fiscal spillover study focusing on spillovers across a single state border over a long period of time, with very detailed district-level school finance variables which powerfully predict changes in districts’ equilibrium revenues over time.

fiscal policies, though it decreases the number of observations used to identify spillovers by more than 82%.

3.3 Out-of-state neighbors with high interstate residential mobility

There are theoretical reasons why one might suspect responses to out-of-state neighboring districts to differ from responses to in-state neighboring districts. Tax-payers, students, and school employees may tend to be less mobile across inter-state borders than across within-state borders. Fiscal spillovers between out-of-state neighbors may differ depending on whether residential mobility across state borders is limited due to state-level policies or other factors. To investigate this issue, I re-estimate the out-of-state neighbor IV models above restricting the source of identification to responses among border districts with rates of inter-state residential mobility above the median rate for border districts.

3.4 Out-of-state neighbor IV controlling for border-by-year fixed effects

The out-of-state neighbor IV estimates identify spillovers by comparing a border district’s revenue growth to the revenue growth of similar districts in the same state which did not have any out-of-state neighbors, had out-of-state neighbors with different characteristics, or had out-of-state neighbors located in a different state. Some of this identification is thus related to variation in revenue changes across geographic regions within the same state. The estimate of spillovers may be biased if, controlling for initial characteristics, within-state variation in revenue changes are related to unobserved characteristics. For example, suppose that districts in eastern Pennsylvania experienced high revenue growth compared to observationally similar districts in western Pennsylvania, at a time when New Jersey districts increased their revenues far more than Ohio districts increased their revenues. Differences in revenue growth between districts in eastern versus western Pennsylvania may be due to differences in their neighbors’ behavior, coincidental differences in omitted variables across the two regions of Pennsylvania, or some combination of both. If omitted variables across two regions of the same state are systematically correlated with the fiscal behavior of the neighboring states, then the previous out-of-state IV estimates of spillovers may be biased. To address this issue, I repeat all of the out-of-state neighbor IV analyses with the addition of controls for border-year fixed effects. For each five year interval, these border-year fixed effects absorb the average revenue change for districts
located in the same state with at least one neighboring district in a specific, other state. Continuing the example above, this model would control for the average difference during each time period between the changes in revenues in Pennsylvania districts bordering Ohio and the changes in revenues in Pennsylvania districts bordering New Jersey. The estimates of spillovers are thus identified from the behavior of districts along the same side of the same state border with out-of-state neighbors that possess different initial characteristics.

4. Data

The analyses use geographic data for every school district in the United States based on the Census TIGER files, which provide centroid coordinates for the districts and allow the researcher to identify which districts share a border. I combine these data with district-level financial panel data available in five year intervals from 1972 to 2002. The financial data for (spring of) 1992, 1997, and 2002 come from the School District Finance Survey (F-33 files), while earlier years come from the Census of Government Files.

The dependent variable in the main analyses below equals five-year changes in school district operating revenues per pupil.\textsuperscript{18} Data concerning operating revenues, the revenues that districts spend on everything but new construction projects and the maintenance of existing buildings, are available for a greater number of years than data concerning operating expenditures, and it is difficult to precisely estimate spillovers for specific types of school expenditures.\textsuperscript{19} The vast majority of schools’ operating expenditures come from same-year

\textsuperscript{18} Observations with suspicious levels of revenues per pupil are removed from the data prior to analysis. Less than 1% of all observations in the raw data are dropped due to questionable revenue per pupil values. In particular, I drop observations with real operating revenues per pupil below $400 or above $22,000, (measured in year 2002 $). I also drop observations that would suggest a more than $10,000 change in revenue per pupil in one five-year period followed by a level of revenue per pupil five years later that is within $4000 of the original level from ten years prior. It is highly unlikely that any district would actually undergo such a large, temporary change in revenue per pupil. The estimates remain very similar, (within .04 for the instrumental variable estimates displayed in row 1 of Table 2), if one fails to drop any of these outliers.

\textsuperscript{19} Detailed expenditure data are only consistently available starting in 1990. Even if estimates out-of-state neighbor IV models using three year intervals (instead of five year intervals) from 1990 to 2002 and examines total expenditures per pupil or expenditures per pupil broken down by category, the confidence intervals around the estimates of fiscal spillovers are very large. Using a three year interval framework for the out-of-state neighbor IV model and controlling for state-border-by-year fixed effects, point estimates provide loose evidence that fiscal spillovers are more closely related to spillovers in instructional staff salaries (.229 slope with a .090 standard error) than spillovers in non-instructional expenditures (.069 slope for districts’ support service expenditures with a .093 standard error).
operating revenues, so expenditures per pupil and revenues per pupil have a correlation of about 0.9 during years in which both are available. Data concerning total operating revenues are also available for more years than data concerning locally-funded operating revenues, and the use of total operating revenues facilitates the cross-state-border analyses given that the marginal tax price for local public school revenues varies substantially across states. The results below thus examine “expenditure competition” between school districts, with operating revenues serving as a proxy for operating expenditures. While more than half of total public school operating revenues are state-funded for the majority of districts in the sample, local citizens or their elected representatives ultimately determine the last dollar spent on local public schools in these districts.20

I combine these financial data with U.S. Census demographic data aggregated to the school district level for 1970, 1980, and 1990. Some analyses below also include self-collected data concerning variation in the local political processes for determining school district expenditure levels. Political institutions could influence the magnitude or speed of districts’ responses to their neighbors’ actions. I obtained information concerning local democratic institutions from 1970 to 2002, using surveys of state finance experts, reviews of school finance documents (e.g., U.S. Department of Education, 2001), data from Saiz’s (2005) New England municipality interviews with local school officials, and referenda frequency information for 1970-1972 from Hamilton and Cohen (1974).21 In the 48 continental states, about 37% of all

20 Total operating revenues include federally funded revenues, though the majority of observations in the sample are cases in which districts received less than five percent of their total revenues from the federal government. Changes in the allocation of federal funds over time could lead to a spatial correlation in school district revenues, but the instrumental variables estimates of fiscal competition should not be biased by these types of changes, especially given that the models control for the state-by-year-specific effects of lagged independent variables.

21 We first surveyed the contributors to "Public School Finance Programs of the U.S. and Canada: 1998-99" (U.S. Department of Education, 2001) from each state regarding the form of local democracy in that state. If necessary, we also contacted state education officials who were members of the American Education Finance Association. While most of the survey responses alluded only to current practices, the information reported in Hamilton and Cohen (1974) allowed us to detect state-level changes in these policies over time. These changes typically coincided with state education finance equalization reforms. Most states with inter-district variation in the form of local democracy are New England states where the school districts coincide with towns and each district’s form of democracy matches the municipal form of democracy coded by Saiz (2005). One exception is New Hampshire, which required us to survey each district individually. Anecdotal evidence for districts in places with intra-state variation suggests that these districts tend to retain the same form of democracy over time, but it is possible that district-level longitudinal changes are a source of measurement error in these data.
districts currently determine expenditure levels exclusively through local citizens voting directly, 55% determine their expenditure levels through locally elected representatives, and citizens in the remaining 8% of districts do not have much discretion over local public school operating expenditure levels. Analyzing the magnitude and speed of fiscal responses in various local democratic systems may thus provide insights into the political mechanisms by which fiscal spillovers occur. Except where noted, the analyses below exclude districts lacking local discretion over local public school operating expenditure levels, because these districts would not have the capacity to engage in fiscal competition.22

A small share of localities experienced school district re-organizations during the sample period, such as mergers between districts or unifications of elementary-level districts with secondary-level districts. While panel studies of education finance usually ignore these mergers or drop all observations for districts which ever re-organized, it may be important to verify that the ensuing sample selection does not have a large effect on the empirical results. The analyses below incorporate historical data concerning any type of school district re-organization. They include a full set of observations for districts that merged by combining data from the participating districts for observations predating the merger, and they also control for whether a district re-organized and whether any of a district’s neighbors re-organized.23

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22 The data used for the main analyses thus exclude observations for districts in California from 1977 on, Michigan from 1997 on, Nevada from 1977 on, New Mexico from 1977 on, Oregon for 1997, and Wyoming for 2002. California is classified as a no-local-control state, because in 1976 the Serrano decision took away virtually all local control of operating expenditure levels. However, California districts have had the option of using a parcel tax to fund some local public school operating expenditures. During the sample period, this parcel tax required approval from two-thirds of district voters and its use was mostly limited to relatively wealthy districts in the northern part of the state. I exclude all California districts from the main analyses because they had relatively little local discretion, but I also account for California’s parcel tax option in additional analyses that focus on districts lacking any local discretion.

23 The models include four independent variables related to re-organizations: an indicator for whether the district re-organized during that time period, an indicator for whether any of the district’s neighbor’s re-organized during that time period, an indicator for whether the number of neighbors increased from the prior period, and an indicator for whether the number of neighbors decreased from the prior period. Additional analyses, not shown here, test the sensitivity of this specification by instead using a balanced panel containing districts that never underwent any re-organization. Using this restricted sample tends to increase the estimates of fiscal spillovers in the out-of-state instrumental variable models and decrease the estimates of fiscal spillovers in the all-neighbor instrumental variables models, but the direction of these changes vary depending on the models’ other parameters.
5. Results

5.1 Descriptive Statistics

Table 1 displays how district operating revenues per pupil have changed between 1972 and 2002. Each five year interval was associated with a rise in real mean district revenue per pupil, except for the period between 1972 and 1977 when large growth in student populations outpaced revenue growth. Mean revenues per pupil increased rapidly from the late 1970’s through the mid 1980’s, as population growth slowed and many states enacted school finance reforms. Changes in a district’s own revenues per pupil are highly correlated with changes in the mean neighboring districts’ revenues per pupil. This correlation was particularly high for in-state neighbors during the 1980’s, when many states enacted school finance equalization policies. The correlation is much higher for in-state neighboring districts than for out-of-state neighboring districts, which is consistent both with common underlying trends for in-state neighbors and with districts being more responsive to changes among their in-state neighbors.

The Appendix displays the means and standard deviations of descriptive variables used to formulate some of the independent variables in the regressions below. Districts with at least one out-of-state neighbor were remarkably similar to other districts in terms of mean observed characteristics.

5.2 Power of the Instrumental Variables

Both the out-of-state neighbor IV and all-neighbor IV are powerful predictors of actual mean revenue changes. The high power of these instruments reflects states frequently changing their education finance formulas, with similar consequences for similar districts within the same state. The predicted mean neighbor revenue change alone explains almost 50% of the within-state-by-year variation in actual mean neighbors’ revenue changes, (i.e., in a model that controls for state-by-year fixed effects but nothing else). When actual mean neighbor revenue changes are regressed on the all-neighbor instrument and all of the control variables used in the second stage regression, the coefficient on the all-neighbor instrument is highly statistically significant ($t$-statistic>200 and $p<.000001$). Similarly, limiting the sample to districts with at least one out-of-state neighbor, the coefficient on the predicted mean out-of-state neighbor revenue change is highly statistically significant ($t$-statistic>100 and $p<.000001$) when actual mean revenue
changes among out-of-state neighboring districts are regressed on their predicted values and the second stage control variables.

5.3 Main Results

Table 2 reveals estimates of fiscal spillovers given various definitions of neighbors and various methodologies. Each estimate represents the impact on a district’s own revenues per pupil if the mean neighboring district revenues per pupil increases by one dollar, measured in year 2000 dollars. As discussed previously, these regressions also control for state-year fixed effects, as well as state-year-specific effects of districts’ lagged demographic variables. From left to right, each column of Table 2 displays estimates for models which are increasingly likely to only reflect responses to exogenous changes in neighbors’ behavior. The first column displays estimates from ordinary least squares regressions. The second column displays estimates from a traditional two-stage IV model. The third column displays estimates from the IV model that uses predicted neighbors’ changes based on the changes of geographically distant districts, and the fourth column displays estimates from the IV model that only uses predicted changes for out-of-state neighbors. Finally, the fifth column displays estimates from the IV model that only uses predicted changes for out-of-state neighbors in states with major, unique school finance changes. The samples sizes vary slightly between the rows of Table 2 as the definition of neighboring districts changes, because districts must have at least one valid neighbor to be included in the regression. The sample sizes are the same across each row, because districts need not have an out-of-state neighbor to be included in the out-of-state neighbor IV regressions.

The point estimates in row 1 of Table 2 suggest that a one dollar increase in the mean operating revenues per pupil of districts within a thirty mile radius leads to between an 22.2 cent and 28.4 cent increase in a district’s own operating revenues per pupil, depending on which instrumental variables specification is used. These estimates remain statistically significant if

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24 A small number of districts do not have any valid neighbors when neighborliness is defined based on radii, because these districts’ geographic areas are so large that the distance between their centroid coordinates and the nearest neighbor’s centroid coordinates is greater than thirty miles. A few additional districts do not have at least one neighbor that is within this distance and also meets the criteria for having a similar median household income.
one adjusts the standard errors for spatial and serial autocorrelation. As expected, the OLS estimates (first column) are greater than the corresponding instrumental variable estimates. The out-of-state neighbor IV estimate of fiscal spillovers equals 22.2 cents, and this estimate remains fairly similar (23.1 cents) when limiting the out-of-state IV identification to cases in which the outside state had a major, unique finance change. The out-of-state IV estimates is less than the traditional IV estimate of 27.5 cents, a difference that is statistically significant at the .10 level. The difference between these two models’ estimates are even greater and more statistically significant if one uses other definitions of neighbors (see rows 2 and 3). In contrast, the all-neighbor IV estimates are very similar and not statistically different from the traditional IV model estimates. These results suggest there may be a moderate upward bias in the traditional IV models and this bias is not eliminated by using only distant, in-state districts to predict neighboring districts’ behavior.

Row 2 of Table 2 display estimates of spillovers among districts that are not only located within a thirty mile radius but also have similar median household incomes. A neighboring district has a similar median income if its median income is within 20% of the district’s own median income. Estimates of spillovers do not increase when the set of neighbors is restricted based on similarity in household incomes, as the instrumental variables estimates in row 2 range from about 17 to 28.5 cents. These findings are important for interpreting the baseline results in row 1. As noted in Section 3, it is theoretically possible that fiscal spillovers occur between districts with similar demographic characteristics even if they are not geographically proximate. If this were the case, however, then limiting the set of local neighbors to districts with similar demographic characteristics would likely have increased the estimates of fiscal spillovers, because these similar, local districts’ predicted behavior would have more closely captured the behavior of distant districts with similar demographic characteristics. While demographic

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25 The standard errors reported in the tables do not account for potential spatial autocorrelation and serial autocorrelation. Adjusting for autocorrelation increases the standard errors for the 30 mile radius models (row 1 of Table 2) from .01 to .04 for the all-neighbor IV model and from .04 to .05 for the out-of-state neighbors IV model. These adjusted standard errors were found using the GMM estimation proposed by Conley (1999), finding weighted averages of spatial autocovariance terms with weights set to zero if districts are located more than 50 miles away from each other or their observations are more than one time period (5 years) apart. I am grateful to Tim Conley for providing the Matlab program which I adapted for these estimations. Due to the lengthy computational time required, (several months), spatially adjusted standard errors were not computed for the other models.

26 Just over half of all out-of-state districts within a thirty mile radius meet this income criterion.
similarities are important for spillovers between states (Case, Hines, & Rosen, 1993), geographic proximity appears to be a much more important determinant of spillovers between school districts.

Estimates of spillovers do not increase if one alters the definition of neighbors from a thirty mile radius to another geographic criterion. Distances of forty miles yield similar estimates of spillovers as distances of thirty miles, and spillover estimates decrease when the set of neighbors is based on either a twenty or fifty mile radius. Row 3 of Table 2 reveals that the estimates do not increase when neighborliness is defined based solely on contiguity, whether districts’ borders touch one another. For the out-of-state IV models, estimates of fiscal spillovers are very small and statistically insignificant when neighborliness is based on contiguity. The greater estimates of spillovers for districts within thirty miles than for contiguous districts and for districts within twenty miles are due to greater fiscal spillovers between proximate, non-contiguous districts that are relatively small in geographic size.27 The remaining analyses presented in this paper thus define neighboring districts as those within a thirty mile radius.

Table 3 displays estimates of spillovers based on the additional out-of-state neighbor IV specifications described in the previous section. The first two columns of Table 3 are analogous to the estimates in columns 3 and 4 of row 1 of Table 2. For the most inclusive group of out-of-state neighbors, spillovers decrease from about 22 cents to 12 cents when border-year fixed effects are included. Controlling for border-year fixed effects does not substantially change the estimates based on out-of-state neighbors in states with major, unique finance changes, which is sensible given that there are very few cases in which a state borders two different states that simultaneously undergo substantial finance changes.

To investigate the importance of the permeability across a state border, I re-estimate the out-of-state IV analysis identifying fiscal spillovers solely from the 50 percent of out-of-state neighboring school districts with the highest rates of inter-state residential mobility.28 Column 3

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27 The set of contiguous districts includes cases in which the districts’ centroid coordinates are more than thirty miles apart and the set of districts within a thirty mile radius includes numerous small districts that are not contiguous. A twenty mile radius is not very far and therefore disqualifies many close neighbors, especially considering that these are centroid to centroid distances rather than border to border distances.

28 Estimates suggest that more than 6.4% of the residents living in these districts in the year 2000 had lived in a different state five years earlier. These estimates are available from the 2000 Census Public Use Micro-Sample and unavailable for earlier years, and these mobility rates do not identify the particular states where residents formerly resided. The high mobility group excludes a handful of districts in which more than 50% of residents lived in a different city during the prior 5 years, because inter-state mobility
of Table 3 displays the estimates for this model, with or without border-year fixed effects. Districts with relatively high rates of inter-state mobility have a greater fiscal response to their out-of-state neighbors, especially if the model does not control for border-year fixed effects. The estimate controlling for border-year fixed effects is arguably more informative, because the potential regional bias described earlier might be particularly strong when residents in multiple regions of a state are highly mobile across different state lines. To the extent that inter-state neighbors across relatively permeable state borders are similar to typical neighboring districts, this estimate suggests the typical level of fiscal spillovers may be about 24 cents per a $1 change in the mean neighbors’ revenues.

5.4 Falsification tests
5.4.1 Opposite border states’ trends as instruments

The only way in which the out-of-state neighbor instrumental variable estimates above would be biased is if there are broad regional trends in revenues per pupil which spread beyond one hundred miles across state borders and, controlling for districts’ observed characteristics, are correlated with the observed characteristics of districts’ out-of-state neighbors. For the models with border-year fixed effects, the omitted effects of regional trends would have to be correlated with differences in observed characteristics along the same state border. To explore whether the estimates above are actually biased by these types of regional trends, one can conduct the following falsification test: estimate a similar out-of-state neighbor IV model which uses instrumented values based on changes in revenues for similar districts in a different neighboring state. For example, one can predict changes in revenue for districts in eastern Pennsylvania using actual changes in revenue for similar districts in New York that are far from the New Jersey border, and then examine whether the revenue changes of New Jersey districts are related to these counterfactual predicted changes in nearby Pennsylvania districts. If there are important regional trends related to districts’ own, unobserved characteristics as well as observed characteristics of districts’ neighbors, then one would continue to see a significant estimate for fiscal spillovers in this counterfactual model.

in these districts was generally due to local colleges or military bases rather than typical residential relocation decisions. While high interstate mobility between 1995 and 2000 could possibly be endogenous with respect to fiscal changes during the 1990’s, the results remain similar if one focuses on fiscal spillovers between these districts prior to the 1990’s.
In fact, as expected, an “opposite state” counterfactual out-of-state neighbor IV model produces statistically insignificant estimates of fiscal spillovers. In this counterfactual model, the first stage predictions of revenue changes are based on changes in similar districts in the state whose border is first crossed in the opposite direction by a straight line connecting the center points of the border states of interest. For example, out-of-state neighbors of Illinois districts which are located in Indiana would have their values predicted by similar districts in Missouri. In cases where the lines hit something other than another state, (e.g., an ocean, Mexico, Canada), the state closest to this point of contact is treated as the opposite state, (e.g., New York in the New Jersey district and out-of-state Pennsylvania district example above). In models that define neighbors based on a thirty mile radius and control for border-by-year fixed effects, the opposite state, out-of-state neighbor IV model produces an estimate of fiscal spillovers equal to .019 with a standard error of .048. Omitting controls for border-by-year fixed effects, this opposite state counterfactual model produces negative estimates of fiscal spillovers. This suggests that the earlier out-of-state IV estimates are not biased upward due to broad regional trends in school revenues.

5.4.2 Districts with limited local discretion

Some U.S. school districts lack local discretion over operating revenues, because the state decides their level of revenues for them. These districts provide a nice falsification test for the instrumental variables models—spillovers should be absent for districts lacking local discretion, because any co-movements in revenues per pupil among neighbors are not due to districts’ own fiscal responses. For this falsification test, I re-estimate the models in row 1 of Table 2 using observations from districts lacking control. This sample includes observations from Nevada, New Mexico, and some Californian districts.29 California removed much local control from its districts after the Serrano decision in 1976, but California’s districts have also had a parcel tax option for funding local public school operating expenditures. Due to the availability of this parcel tax option, observations from half of all California districts are excluded from the “no local control” sample, specifically districts whose median household income in 1980 was above the median Californian district. These Californian districts had a moderate probability of

29 Observations are excluded if the form of local democracy changed within the past ten years or the next five years, because such a change could lead to residential movement that would influence the estimates.
attempting to adopt a parcel tax to fund local school operating expenditures, while this probability was extremely low for all other Californian districts.\(^\text{30}\)

As expected, the out-of-state neighbor IV estimates suggest that districts unable to locally determine their budgets do not respond positively to changes in neighboring districts’ revenues. The estimates of spillovers for these districts are -.076 cents (standard error of .169 cents) for the out-of-state neighbor IV model and -.111 cents (standard error of .204 cents) for the out-of-state neighbor IV model controlling for border-year fixed effects. While the point estimates for these districts are negative, there are large standard errors due to the small sample size for this group (N=1,431) and the estimates are statistically insignificant. For the all-neighbor IV model, the estimate of fiscal spillovers is positive for districts lacking local control, but is not statistically significant at conventional levels. The estimated coefficient equals .099 cents with a .065 cent standard error and a p-value of .131. Unfortunately, given that the standard errors for these fiscal spillover estimates are so large for this small sample of districts lacking local control, one cannot very confidently rule out similar fiscal responses as those found among the other types of districts. While somewhat inconclusive due to limited power, this evidence is consistent with smaller, near-zero responses among districts lacking local control.

5.4.3 Spillovers via enrollment changes

Given that the dependent variable in these models equals revenues per pupil, one might worry that the apparent fiscal spillovers are actually due to changes in the denominator, student enrollments. To verify that the out-of-state neighbor IV model identifies spillover effects between revenues rather than enrollments, I estimated a similar model but with the dependent variable equal to the percent change in student enrollment in the district over the same five year period. Reassuringly, predicted changes in neighbors’ revenues per pupil do not predict districts’ own enrollment changes well at all, as the p-value of the instrumental variable’s estimated coefficient equals .963 in this alternative model.

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\(^{30}\) According to EdSource (2007), between 1983 and 2006, “210 school districts out of nearly 1,000” attempted to pass this type of parcel tax, and “about 90% of the elections were held in districts that were below the state average of 49% low-income students.” Given that a non-trivial portion of the wealthier Californian districts exercised local discretion, including them in the “no-local-control” group would increase the estimates of fiscal spillovers among “no-local-control” districts from -.080 to -.075 for the out-of-state neighbor IV model, from -.091 to -.052 for the out-of-state neighbor IV model with border-year fixed effects, and from .084 to .197 for the all-neighbor IV model.
6. Exploration of Heterogeneous Effects

This section describes heterogeneity in fiscal spillovers along several dimensions: whether the district is located in a Metropolitan Statistical Area, whether the district was initially wealthier than its neighbors, whether the district was initially outspending its neighbors, the form of local democracy used to determine local school revenues, and whether the districts’ state had adopted a limitation on local tax revenues. Table 4 displays the results of separate regressions which add an indicator for districts’ initial status compared to their neighbors, as well as an interaction term between this indicator and the predicted change in neighboring districts’ revenues. Collectively, panels (i) and (ii) of Table 4 reveal that relative median household income is very important for spillovers across state lines but not very important for spillovers between in-state, neighboring districts. For the out-of-state neighbor IV models, spillovers are strongest when a district’s initial median household income exceeds the mean of its neighbors but does not exceed the median household income of every neighbor. In fact, fiscal spillovers are very small and statistically insignificant in these models if a district’s median household income already exceeds that of all of its neighbors. This result might be due to state policy differences in terms of the progressiveness of taxation and school expenditures across state lines; relatively wealthy households residing near state borders likely choose to reside in the state with fewer redistributive policies, and cross-state differences in progressiveness overshadow marginal changes in the tax-expenditure bundles of nearby, out-of-state districts. Only the districts that are the wealthiest in their region appear to be immune to the behavior of their out-of-state neighbors.

Additional analyses suggest that local fiscal spillovers are asymmetric and that the prior estimates may be very slightly biased downwards due to reversion to the mean. Panel (iii) of Table 4 reveals that districts starting with relatively low revenues increase their revenues by more than $1,000 per pupil, on average, compared to other districts. This difference in intercepts suggests that there is substantial mean reversion in terms of district revenues within local regions. While districts with relatively high revenues per pupil have a smaller intercept due to this mean reversion, the slope associated with responses to changes in neighbors’ revenues is much larger for these relatively high spending districts. For the model using the out-of-state neighbor instrumental variable controlling for border-year fixed effects, districts initially
possessing greater per pupil revenues than their neighbors raise an additional 36.1 cents per pupil for a one dollar increase in the average per pupil revenues of their neighbors; in contrast, districts initially possessing lower per pupil revenues than their neighbors respond to their neighbor’s revenues changes with a slope of only 10.2 cents per pupil. For the all-neighbor IV model, these estimated slopes equal 39.4 cents and 28.1 cents respectively. Local spillovers are less often a matter of “keeping up with the Joneses” than a matter of “staying ahead of the Joneses.”

Metropolitan area status provides another interesting source of heterogeneity in the size of spillovers. I classify districts as located inside an MSA (Metropolitan Statistical Area) based on their classification in the 2000 Census. One might expect densely populated metropolitan areas to engage in greater degrees of fiscal competition, because tax rates and public school popularity may be more readily capitalized into house prices in these areas and because residents may be relatively well informed of the behavior of surrounding districts. Panel (iv) of Table 4 confirms that responses to the revenue changes of out-of-state neighbors are in fact strongest among districts located in Metropolitan Statistical Areas (MSA’s). Fiscal spillovers across state borders are more than 45 percent greater in MSA’s than in non-MSA’s. On the other hand, fiscal spillovers in the all-neighbor IV model are strongest outside of MSA’s, with a 29.8 cent response for non-MSA districts versus a 23.7 cent response for districts in MSA’s.

While these all-neighbor IV model results may seem surprising, additional empirical analyses suggests that they are related to residential mobility across districts in the same state and same MSA. Changes in the tax-expenditure bundle offered by one district can cause some of its residents to move to other, nearby districts, and can marginally influence the location decisions of people moving to the area. The additional analyses empirically test for the impact of changes in neighboring districts’ revenues on the residential composition of a district; they are similar to the main analyses, except the dependent variable is changed to the median household income in the school district. Given that there will be multiple, simultaneous changes

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31 Demographic variables are only available at ten year intervals corresponding with Census years. Therefore, instead of using five year intervals as in the previous analyses, these models regress median household income on the predicted changes in neighboring districts’ operating revenues per pupil over the prior ten years. These models use the same set of control variables as in the previous analyses, so the control variables include lagged median household income and other lagged demographic characteristics. A limitation of these analyses is that the fiscal and demographic data are not available in precisely the same year, so the Census year median household income variable (i.e., 1980, 1990, and 2000) is actually regressed on predicted ten year changes in neighboring district revenues that slightly overlap these Census years (i.e., 1972 to 1982, 1982 to 1992, and 1992 to 2002). These analyses thus fail to capture the impact
occurring in districts’ expenditures and demographics, these analyses simply reveal reduced-
form relationships associated with districts moving from one equilibrium to another. The results 
suggest that an increase in the revenues of in-state, neighboring districts is associated with a 
decrease in a district’s own median household income if and only if the district is located in a 
metropolitan area. In particular, the all-neighbor IV model suggests that a $1000 increase in 
mean revenues of neighboring districts leads to a $124 decrease in a metropolitan districts’ own 
median household income but only an $11 decrease in a non-metropolitan districts’ own median 
household income, a difference of $113 that is statistically significant at the .03 level. Rising 
school revenues in nearby metropolitan area districts appear to lure away relatively wealthy 
households, thus lowering the median income of a district’s own residents in spite of the 
district’s positive fiscal response to its neighbors. Given that local public education spending is a 
normal good, this change in residential composition would attenuate the district’s positive fiscal 
response. Changes in household income may be symptomatic of more general changes whereby, 
particularly in metropolitan areas, an increase in neighboring districts’ revenues leads to an 
influx of residents with a relatively low demand for public school services.\(^{32}\)

Fiscal spillovers occur regardless of whether local school tax revenue decisions are 
determined through direct or representative democracy. Table 5 displays regression results for 
models dividing the sample based on the form of local democracy used to determine public 
school district operating revenues. The sample is divided based on the three categories described 
in Section 4: (1) districts without any local discretion, (2) districts using representative 
democracy, and (3) districts exclusively using direct democracy. As discussed earlier, the 
estimates of fiscal spillovers are statistically insignificant and relatively small for districts 
without any local discretion. For the out-of-state neighbor IV models, the estimates of fiscal 
spillovers are larger for direct democracy districts than for representative democracy districts, 
while the converse holds for the all neighbor IV models. These differences may be at least

\(^{32}\) While the differential impact on median household income can partially explain why metropolitan 
districts have smaller net fiscal spillovers, this income differential alone is not large enough to explain the 
entire .109 difference in the slopes of fiscal spillovers between metropolitan and non-metropolitan 
districts—the elasticity of local school operating revenues with respect to median household income 
would have to be as high as 4 in order to explain this entire difference.
partially due to the greater importance of yardstick competition between in-state districts than between out-of-state districts.

Some districts with local control might actually be constrained in terms of their local expenditure decisions, because many states limit school district revenues or expenditures. Brueckner and Saavedra (2001), for example, find that tax competition between Boston-area municipalities disappeared after the arrival of Massachusetts’ Proposition 2½. To explore this issue in the school district context, I also tested for heterogeneous levels of fiscal spillovers based on the timing of states’ adoption of tax or expenditure limits. On average, states’ adoptions of these policies did not decrease the magnitude of spillovers between school districts.33 This result may stem from the fact that tax and expenditure limits are typically binding for only a subset of districts in a particular state, and districts might be strongly influenced by their neighbors when deciding whether to raise the maximum allowable revenues.

7. Conclusion

School districts’ revenues respond to changes in nearby districts’ revenues. The results in Tables 2 and 3 suggest that fiscal spillovers between districts are, on average, in the neighborhood of 22 cents per a $1 change in mean neighbors’ revenues, where the relevant set of neighbors is districts located within a thirty mile radius. The most conservative instrumental variables estimate is the 12 cent response from the out-of-state neighbor IV model that controls for border-year fixed effects, and this may understate overall fiscal spillovers because some districts are less responsive to out-of-state neighbors’ behavior than other neighbors’ behavior. Even in models controlling for border-year fixed effects, the out-of-state IV estimates are very close to 22 cents when identification is based only on states with unique school finance reforms or only on districts with high rates of inter-state residential mobility. The magnitude of the main

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33 I defined states as restricting districts’ ability to increase expenditures if the state limited district expenditures, limited district revenues, or limited both local property taxes and property value assessments. This classification is similar to one adopted by Figlio (1997) and by Downes (2007). Like Downes, I use information presented by Mullins and Wallin (2004) to identify the timing of states’ adoptions and removals of these policies. While the adoption of these policies is certainly non-random, this specification assumes that their adoption is not related to unobserved variables which influence the magnitude of fiscal competition. Controlling for state-specific slopes of fiscal spillovers and controlling for year-specific slopes of fiscal spillovers, the sign of the estimates are actually consistent with state limits increasing spillovers, though these estimates are not statistically significant if one excludes observations that immediately followed the adoption of the limitation policies.
estimates of local fiscal spillovers thus suggest economically important effects, but slightly smaller effects than one would estimate using methodologies that are biased due to spatially correlated omitted variables, spatially correlated measurement error, and/or common responses to changes in fiscal federalism (i.e., common vertical spillovers rather than horizontal spillovers).

While studies of fiscal spillovers between state governments in the U.S. or between municipal governments in other countries have found yardstick competition to be the primary source of these spillovers, this paper’s results suggest that yardstick competition due to political agency problems may not be essential for spillovers between U.S. school districts. Spillovers between in-state neighboring districts are greater when districts use representative democracy, but substantial positive spillovers also occur between direct democracy districts. It is worth noting that informational spillovers between school districts might be important even in direct democracy settings; citizens may interpret nearby districts’ behavior as a valuable signal concerning the potential benefits of changing local tax rates, such as positive effects on local house prices. In addition, one should not rule out yardstick competition due to political agency problems in direct democracy districts, given the well documented importance of local officials as agenda-setters in various direct democracy settings (Romer and Rosenthal, 1979, 1982; Romer, Rosenthal, and Munley, 1992; Pecquet et al., 1996; Dunne et al. 1997; Holcombe and Kenny, 2007).

Another important finding is that the positive spillovers between local governments are often mitigated by Tiebout sorting. To date, there have been relatively few empirical studies directly testing household mobility predictions consistent with the Tiebout model.34 This paper finds that positive fiscal spillovers between nearby districts in the same state are actually smaller in metropolitan areas, and there is suggestive evidence that this difference is due to households’ residential location decisions across districts in the same metropolitan area. When considering policies that would substantially alter the distribution of revenues across districts, policy makers should thus consider that these policies will influence the distribution of residents across districts.

General equilibrium effects of neighboring districts’ spending changes are an important topic for further study. Future empirical research might also explore whether fiscal spillovers are

34 See Banzhaf and Walsh (2008), who find evidence consistent with Tiebout sorting by observing changes in population densities in response to exogenous improvements in local environmental quality.
related to non-residential property wealth, which has been an understudied topic due to the scarcity of district-level data accurately measuring the non-residential portion of the property tax base. Another topic meriting further study is fiscal spillovers between private schools and public schools.

As for this paper’s estimates, the most striking evidence of heterogeneity in fiscal spillovers is the much greater responses among school districts that were already outspending their neighbors. This finding has important implications for the optimal design of states’ school finance systems. Policies that focus on increasing revenues in relatively low-spending districts could indirectly lead to substantial increases in the revenues of other, nearby districts. Policymakers hoping to narrow expenditure gaps across districts must recognize that narrowing these gaps is akin to hitting a moving target. When policies aim at boosting the expenditures of low spending districts, the higher spending neighboring districts respond by further increasing their own expenditures.
References


Table 1: Changes in School District Operating Revenues per Pupil, 1972-2002

<table>
<thead>
<tr>
<th>Time Period</th>
<th>Mean (Year 2000 $)</th>
<th>Standard Deviation</th>
<th>Correlation with Change in Operating Revenues per Pupil Among…</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>All neighbors</td>
</tr>
<tr>
<td>1972-77</td>
<td>-2,229</td>
<td>2,090</td>
<td>.523</td>
</tr>
<tr>
<td>1977-82</td>
<td>2,652</td>
<td>1,886</td>
<td>.597</td>
</tr>
<tr>
<td>1982-87</td>
<td>1,063</td>
<td>3,525</td>
<td>.626</td>
</tr>
<tr>
<td>1987-92</td>
<td>1,782</td>
<td>4,368</td>
<td>.724</td>
</tr>
<tr>
<td>1992-97</td>
<td>473</td>
<td>1,655</td>
<td>.368</td>
</tr>
<tr>
<td>1997-2002</td>
<td>1,447</td>
<td>1,770</td>
<td>.279</td>
</tr>
</tbody>
</table>

Note to Table 1: As with the paper’s main analyses, neighboring school districts are those with centroid coordinates that are within thirty miles of each other.
### Table 2: Estimates of Fiscal Spillovers between U.S. School Districts

<table>
<thead>
<tr>
<th>Definition of Neighbors</th>
<th>OLS</th>
<th>Instrumental Variables Model Identified Using…</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Traditional 2-stage Least Squares</td>
<td>Predicted neighbors’ revenue changes based on similar but distant districts</td>
</tr>
<tr>
<td>(1) Districts within a 30 Mile Radius ($N=65,568$)</td>
<td>.374 (0.008)</td>
<td>.275 (0.013)</td>
</tr>
<tr>
<td>(2) Districts within a 30 Mile Radius and with Similar Median Household Income ($N=64,383$)</td>
<td>.301 (0.008)</td>
<td>.272 (0.014)</td>
</tr>
<tr>
<td>(3) Contiguous School Districts ($N=65,951$)</td>
<td>.348 (0.007)</td>
<td>.264 (0.012)</td>
</tr>
</tbody>
</table>

Notes to Table 2: Each cell represents a separate regression and reveals the estimated change in a district’s operating revenue per pupil from a one dollar increase in the average operating expenditures per pupil among neighboring districts during five year intervals from 1972 to 2002. The sample sizes vary slightly across the rows, because observations are only included if they have non-missing predicted changes in revenues for at least one neighboring district. (A small number of districts lack any neighbors within a certain radius that had non-missing data for the relevant variables from the prior Census, particularly for the 1970 Census for which data are unavailable for some of the smallest school districts.) The sample sizes are constant across the same row, because districts are included in the various samples regardless of whether they have any out-of-state neighbors. All values are in year 2000 dollars. Each regression controls for state-year fixed effects, as well as for the state-year specific effects of lagged demographic variables. The regressions also control for recent district re-organizations. The sample excludes districts which lack much local discretion over operating revenues per pupil.
Table 3: Additional Estimates of Fiscal Spillovers between Districts Located within Thirty Miles of each other Across State Borders

<table>
<thead>
<tr>
<th>Instrumental Variables Model Identified Using…</th>
<th>All out-of-state neighbors</th>
<th>Out-of-state neighbors in states with major, unique finance changes</th>
<th>Out-of-state neighbors with high inter-state residential mobility</th>
</tr>
</thead>
<tbody>
<tr>
<td>Baseline</td>
<td>.222 (.038)</td>
<td>.231 (.064)</td>
<td>.435 (.074)</td>
</tr>
<tr>
<td>Controlling for border-year fixed effects</td>
<td>.120 (.055)</td>
<td>.210 (.099)</td>
<td>.238 (.098)</td>
</tr>
</tbody>
</table>

Notes to Table 3: See Notes to Table 2. Neighboring districts are defined as having high inter-state residential mobility if they were above the median rate of inter-state moves for border districts based on the 2000 Census, (see footnote 28).
Table 4: Heterogeneous Estimates of Fiscal Spillovers Based on Initial Characteristics Compared to Neighbors or Based on Location in a Metropolitan Area

<table>
<thead>
<tr>
<th>IV Model Identified Using…</th>
<th>All out-of-state neighbors</th>
<th>All out-of-state neighbors controlling for state-year-border fixed effects</th>
<th>All Neighbors, (Predicted neighbors’ revenue changes based on similar but distant districts)</th>
</tr>
</thead>
</table>

(i) $D_i = 1$ if District Initially Had Lower Median Household Income than Average of Neighbors’ Median Household Income

<table>
<thead>
<tr>
<th>Predicted Change in Mean Neighbors’ Revenues</th>
<th>.273</th>
<th>.158</th>
<th>.266</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(.044)</td>
<td>(.059)</td>
<td>(.012)</td>
</tr>
<tr>
<td>&quot; &quot; &quot; * $D_i$</td>
<td>-.085</td>
<td>-.069</td>
<td>.005</td>
</tr>
<tr>
<td></td>
<td>(.038)</td>
<td>(.039)</td>
<td>(.019)</td>
</tr>
<tr>
<td>$D_i$</td>
<td>.011</td>
<td>.012</td>
<td>.013</td>
</tr>
<tr>
<td></td>
<td>(.019)</td>
<td>(.019)</td>
<td>(.009)</td>
</tr>
</tbody>
</table>

(ii) $D_i = 1$ if District Initially Has a Greater Median Household Income Than Every Neighbors’ Median Household Income

<table>
<thead>
<tr>
<th>Predicted Change in Mean Neighbors’ Revenues</th>
<th>.247</th>
<th>.142</th>
<th>.279</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(.039)</td>
<td>(.057)</td>
<td>(.011)</td>
</tr>
<tr>
<td>&quot; &quot; &quot; * $D_i$</td>
<td>-.211</td>
<td>-.136</td>
<td>-.050</td>
</tr>
<tr>
<td></td>
<td>(.076)</td>
<td>(.081)</td>
<td>(.017)</td>
</tr>
<tr>
<td>$D_i$</td>
<td>-.002</td>
<td>-.011</td>
<td>.016</td>
</tr>
<tr>
<td></td>
<td>(.035)</td>
<td>(.036)</td>
<td>(.037)</td>
</tr>
</tbody>
</table>

(iii) $D_i = 1$ if District Initially Had Lower Revenues per Pupil than Avg. Neighbors’ Revenues per Pupil

<table>
<thead>
<tr>
<th>Predicted Change in Mean Neighbors’ Revenues</th>
<th>.507</th>
<th>.361</th>
<th>.394</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(.039)</td>
<td>(.054)</td>
<td>(.011)</td>
</tr>
<tr>
<td>&quot; &quot; &quot; * $D_i$</td>
<td>-.273</td>
<td>-.259</td>
<td>-.113</td>
</tr>
<tr>
<td></td>
<td>(.034)</td>
<td>(.035)</td>
<td>(.007)</td>
</tr>
<tr>
<td>$D_i$</td>
<td>1.10</td>
<td>1.12</td>
<td>1.20</td>
</tr>
<tr>
<td></td>
<td>(.014)</td>
<td>(.014)</td>
<td>(.015)</td>
</tr>
</tbody>
</table>

(iv) $D_i = 1$ if District is Located in a Metropolitan Statistical Area (based on MSA boundaries in 2001)

<table>
<thead>
<tr>
<th>Predicted Change in Mean Neighbors’ Revenues</th>
<th>.171</th>
<th>.093</th>
<th>.298</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(.042)</td>
<td>(.058)</td>
<td>(.012)</td>
</tr>
<tr>
<td>&quot; &quot; &quot; * $D_i$</td>
<td>.143</td>
<td>.092</td>
<td>-.061</td>
</tr>
<tr>
<td></td>
<td>(.049)</td>
<td>(.057)</td>
<td>(.011)</td>
</tr>
<tr>
<td>$D_i$</td>
<td>-.031</td>
<td>-.020</td>
<td>.032</td>
</tr>
<tr>
<td></td>
<td>(.022)</td>
<td>(.022)</td>
<td>(.023)</td>
</tr>
</tbody>
</table>

Notes to Table 4: Neighboring districts are defined as those located within a 30 mile radius. Each column of each panel provides three estimated coefficients from a single regression.
Table 5: Heterogeneous Estimates of Fiscal Spillovers Based on Local Democracy

<table>
<thead>
<tr>
<th>Form of Local Democracy for Determining Public School Operating Expenditures</th>
<th>Instrumental Variables Model Identified Using…</th>
<th>All Neighbors, (Predicted neighbors’ revenue changes based on similar but distant districts)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>All out-of-state neighbors</td>
<td>All out-of-state neighbors controlling for state-year-border fixed effects</td>
</tr>
<tr>
<td>(1) No Local Control</td>
<td>-.076</td>
<td>-.111</td>
</tr>
<tr>
<td>(N=1,431)</td>
<td>(.169)</td>
<td>(.204)</td>
</tr>
<tr>
<td>(2) Representative Democracy</td>
<td>.153</td>
<td>.098</td>
</tr>
<tr>
<td>(N=35,501)</td>
<td>(.056)</td>
<td>(.083)</td>
</tr>
<tr>
<td>(3) Direct Democracy Only</td>
<td>.265</td>
<td>.127</td>
</tr>
<tr>
<td>(N=29,162)</td>
<td>(.052)</td>
<td>(.075)</td>
</tr>
</tbody>
</table>

Notes to Table 5: Point estimates are from regressions analogous to the models in the third and fourth columns in row 1 of Table 2 and the first column in the second row of Table 4, except that the sample is divided based on districts’ form of local democracy for determining operating expenditures. The sample also excludes observations for districts that recently changed their form of local democracy; observations are dropped if the form of local democracy changed within the past ten years or next five years.
### Appendix: Summary Statistics for School District Characteristics,

*Means with Standard Deviations in Italics*

<table>
<thead>
<tr>
<th></th>
<th>Full Sample</th>
<th>Districts in sample with at least one out-of-state neighbor within 30 miles&lt;sup&gt;a&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of Districts</td>
<td>65,568</td>
<td>25,777</td>
</tr>
<tr>
<td>Operating revenues per pupil</td>
<td>6,854</td>
<td>6,841</td>
</tr>
<tr>
<td>(at the beginning of each 5 year period)</td>
<td>3,638</td>
<td>3,641</td>
</tr>
<tr>
<td>5-year change in operating revenues per pupil</td>
<td>908</td>
<td>910</td>
</tr>
<tr>
<td></td>
<td>3,151</td>
<td>3,173</td>
</tr>
<tr>
<td>Variables based on the 1980 Census&lt;sup&gt;b&lt;/sup&gt;</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Median Household Income (year 2000 $)</td>
<td>37,402</td>
<td>37,446</td>
</tr>
<tr>
<td></td>
<td>12,540</td>
<td>12,751</td>
</tr>
<tr>
<td>Mean House Value (year 2000 $)</td>
<td>101,663</td>
<td>101,937</td>
</tr>
<tr>
<td></td>
<td>47,649</td>
<td>48,820</td>
</tr>
<tr>
<td>Proportion of the Population Ages 5-17</td>
<td>0.22</td>
<td>0.22</td>
</tr>
<tr>
<td></td>
<td>0.03</td>
<td>0.03</td>
</tr>
<tr>
<td>Proportion of the Population Ages 65 &amp; over</td>
<td>0.12</td>
<td>0.12</td>
</tr>
<tr>
<td></td>
<td>0.04</td>
<td>0.04</td>
</tr>
<tr>
<td>Population Density (people per km²)</td>
<td>438</td>
<td>435</td>
</tr>
<tr>
<td></td>
<td>1075</td>
<td>1070</td>
</tr>
</tbody>
</table>

<sup>a</sup> In this paper’s main analyses, neighboring districts are defined as districts with centroid coordinates located within thirty miles of each other.

<sup>b</sup> Census variables are limited to the 1980 year in this table in order to facilitate comparisons of characteristics of districts with or without out-of-state neighbors. The actual regression analyses control for the state-by-year effects of variables based on the immediate prior Census data (1970, 1980, or 1990).