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

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Abstract
This study identifies fiscal spillovers between local governments in the United States and examines whether these spillovers vary based on the form of democracy used to determine tax rates. There is an extensive theoretical literature concerning fiscal spillovers between local governments, but it is challenging to empirically distinguish spillovers from common underlying trends. Using panel data from U.S. school districts, I employ a new instrumental variable strategy—a cross-border spatial lag model. This model tests whether districts located near state borders respond to the predicted fiscal behavior of neighboring districts located in a different state. The results reveal that districts follow their neighbors' lead for school expenditures: a $100 increase in average nearby districts' operating expenditures causes at least a $20 increase in a district's own operating expenditures. As for categorical expenditures, spillovers are larger for instructional expenditures, are statistically insignificant for administrative expenditures, and are negative for capital expenditures. While much of the prior empirical literature on fiscal spillovers has focused on yardstick competition in representative democracy settings in Europe and the United States, spillovers in direct democracy settings are also substantial.

Keywords: school finance; tax competition; direct democracy; representative democracy; expenditure competition

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1. Introduction

Social scientists have debated the causes and consequences of mimicking behavior. Mimicking behavior may reflect people's malleable preferences and mimetic desire, whereby wants and aspirations are largely based on the inclination to imitate. Beginning in the early 1960's, the philosopher René Girard's influential writings observe the central role of mimetic desire in literature and religious texts. Economists also describe mimicking behavior among people with independently-determined preferences; this mimicking may result from complementarities, from agents' rational interpretation of signals (Banerjee, 1992), or from principal-agent problems (Scharfstein & Stein, 1990). Mimicking has been applied to a wide variety of consumption and investment decisions—including herd behavior in the investment decisions of managers (Scharfstein & Stein, 1990), hoarding behavior among consumers (Hong, de Paula, & Singh, 2016), the use of visible goods as status symbols within racial groups (Charles, Hurst, and Roussanov, 2009), and contagion across global financial markets (Calvo & Mendoza, 2000). Regardless of whether the desire to "keep up with the Joneses" and the tendency to "follow the herd" are more closely linked to innate survival instincts, influential social pressures, or the rational use of signals, mimicking can lead to important spillover effects of economic policies.

Within the field of public finance, previous empirical studies of mimicking behavior have largely focused on "yardstick competition" (e.g., Besley and Case, 1995), whereby elected officials' actions are constrained by nearby governments' actions in order to achieve sufficient political popularity in a context in which voters have limited information concerning government productivity. Movements in tax rates and expenditure levels in one government can thus lead to important spillovers for other governments' taxes and expenditures. The seminal work of Case, Hines and Rosen (1993) identified important mimicking behavior across state governments. Later studies have found evidence of positive spillovers specifically in terms of states' welfare spending

(Figlio, Kolpin, and Reid, 1999), states' Medicare spending (Baicker, 2005), and county revenues (Baicker, 2004). Studies have tested for yardstick competition within various countries 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 that fiscal spillovers are strongest when elected officials are most concerned about their political capital.[1]

This paper empirically examines fiscal spillovers in the local government setting and makes several new contributions to the broader literatures concerning fiscal spillovers, fiscal federalism, and education finance. While there is an extensive theoretical literature concerning fiscal spillovers between local governments, it is challenging to empirically distinguish spillovers from common underlying trends. This paper is one of the first to empirically examine local spillovers using plausibly exogenous variation and the first to do so across the entire United States. I employ a novel instrumental variables strategy, a cross-border spatial lag model, which examines whether school districts located near state borders respond to the predicted fiscal behavior of neighboring districts located in a different state.[2]

These out-of-state neighbor predictions are based on changes in local expenditures among otherwise similar districts in that state located far from the relevant state border. Because of frequent state-specific changes in education finance and tax policies, similar in-state districts'

[1] Please see Revelli (2005) for a more detailed review of these and related studies.
[2] To my knowledge, this is the first paper to use a cross-border instrumental variables strategy to identify spatial effects. Bayer, Ferreira, and McMillan's (2007) instrumental variables strategy is similar in spirit to this one; those authors instrument for the neighborhood-independent component of housing prices in the San Francisco area using similarly-constructed housing located at least 3 miles away. Brunner and Imazeki (2010) exploit state borders to examine the impact of statewide teacher tenure policies on teacher salaries across districts within the same metropolitan areas.

revenue changes are powerful predictors of the near-border districts' expenditures changes. Controlling for the effects of states' own important finance changes, the models identify fiscal spillovers based solely on very similar districts that have different out-of-state neighbors or no out-of-state neighbors at all. Given this specification, the estimates could be biased if out-of-state neighbors' observed baseline characteristics happen to be correlated with future shocks affecting districts' own spending. While theoretically possible, this concern is easily set aside—estimates of fiscal spillovers do not decrease if the model adds control variables for the state-by-year-specific effects of districts' neighbors' characteristics.

Falsification tests also support the validity of the cross-border spatial lag model. One falsification tests confirms a lack of estimated spillovers if the predicted out-of-state neighbors' spending changes are based on comparison districts in a different border state. Another falsification test confirms the absence of estimated spillovers if the sample is restricted to school districts lacking local discretion over their school expenditure levels, districts that are otherwise excluded from the analyses below.

This is also the first empirical study in any context to compare fiscal spillovers in direct and representative democracy settings. Using newly collected data on the form of local democracy used to determine local tax rates in all school districts in the United States, I examine whether fiscal spillovers are limited to cases where representative democracy fosters yardstick competition. A well-developed theoretical literature suggests that, distinct from yardstick competition, local tax competition may be an important mechanism for fiscal spillovers (e.g., Wilson, 1999, Brueckner, 2000, Brueckner & Saavedra, 2001, Brueckner, 2003, Wildasin, 2003, Wilson & Wildasin, 2003).

The empirical findings described below suggest that a $100 increase in the average per pupil operating expenditures of nearby districts causes a district to increase its own per pupil operating expenditures by about $20. This estimate may be viewed as a lower bound point estimate because spillovers might typically be greater between districts within the same state than between nearby districts located in different states. Additional analyses, examining spillovers between near-border districts within the same metropolitan areas, produce less conservative estimates of fiscal spillovers—suggesting that typical local spillovers may be closer to $25.   Both of these estimates are lower than the corresponding estimate from an ordinary least squares model ($28).

While this paper cannot rule out several theoretical explanations for positive spillovers, exploring heterogeneous responses helps to reveal which mechanisms are most consistent with observed behavior.[3] Responses are largest for districts that were initially outspending their neighbors. Spillovers in districts using direct democracy to determine local tax rates funding school expenditures are at least as large as in districts using representative democracy. Further analyses reveal positive in-kind spillovers for local tax revenues and reveal spillovers for certain types of categorical expenditures but not others. Spillovers are large for instructional expenditures, statistically insignificant for administrative expenditures, and negative for capital expenditures.

The next section briefly summarizes the theoretical reasons why school districts' expenditures might be influenced by the expenditures 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.

[3] See Brueckner (2003) and Revelli (2005) for excellent summaries of the 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. Background

Theoretical Motivation

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' expenditure 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. Second, there may be *service competition*, school districts increasing expenditures in order to attract students to the local public schools or to gain popularity among households with children. A third mechanism could occur regardless of 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. Besley and Case (1995) and the aforementioned studies of yardstick competition examine information spillovers specifically in representative democracy settings.

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. For example, Nechyba's (2003) computable general equilibrium model suggests 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.

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. Externalities could lead to positive or negative spillovers. There may be complementarities leading to positive spillovers, (e.g., the presence of debate teams in neighboring school districts increases the benefit of adding a team). Negative spillovers due to externalities may occur if neighboring districts' spending raises the costs of a districts' own spending, (e.g., one district's capital campaign may increase local construction costs or debt service costs). Negative spillovers due to externalities may also occur if neighboring districts' spending is a substitute for a district's own spending (e.g., the presence of a high-spending district nearby enables a district to maintain relatively low expenditures and still attract businesses that employ adults with school-aged children).

Prior Empirical Research

Several studies have empirically investigated the topic of fiscal spillovers in the United States at the state or county level. Studies by Case (1993) and by Besley and Case (1995) reveal that: (i) cross-state comparisons of recent state tax rate changes predict incumbent success in U.S. gubernatorial elections, and (ii) due to these yardstick comparisons, states' fiscal behavior is more highly correlated with neighboring states' fiscal behavior when governors are up for reelection. Case, Hines, and Rosen (1993) identify the fiscal interdependence of state expenditures by using neighboring states' demographic trends to predict changes in these neighbors' public expenditures. Their study reveals that a state's own expenditures are *not* strongly 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.

While there has not previously been a national study of fiscal competition between U.S. school districts, there have been some important empirical studies investigating fiscal spillovers between municipalities or school districts in specific states.[4] Isen (2014) does not find evidence of fiscal spillovers between school districts or counties in Ohio, based on whether expenditure referenda narrowly pass or fail to pass in neighboring governments. The Ohio school district referenda in Isen's study included bond measures for capital expenditures and requests to increase local property taxes for school operating expenditures. Brueckner and Saavedra (2001) do find evidence of positive spillovers between 70 municipalities in the Boston area, and they also find evidence that these spillovers disappeared after Proposition 2½ limited most of these municipalities' ability to increase local property taxes.[5] Millimet and Rangasprad (2007) find evidence of positive fiscal spillovers between school districts in Illinois.[6] Babcock, Engberg, and Greenbaum (2005)

[4] A few studies have also tested for spillovers in policy decisions between neighboring schools or school districts. Clark (2010) does not find any evidence of spillovers in neighboring British schools' decisions whether to become autonomous from local governing agencies. Rincke (2006) finds that Michigan school districts were more likely to participate in a voluntary inter-district choice program if neighboring districts had already decided to participate.

[5] Brueckner and Saavedra (2001) empirically test for spatial endogeneity in their models and fail to reject the null hypothesis that their independent variables are exogenously determined. Note that a similar test would be far less convincing for the national data set described below, because the theoretical likelihood of spatial endogeneity dramatically increases as one extends a data set to a wider geographic area. For instance, a lack of spatial endogeneity in the Boston-area data would require that an omitted variable affecting one town's spending is unrelated to omitted demographic changes for a neighboring town—or at least not more closely related to the neighboring town's demographic changes than changes in other towns in the Boston area. One would have to make dubious assumptions to take this approach to national data, such as assuming that omitted variables for Boston-area towns do not affect demographics in neighboring Boston towns more than they affect demographics in places like Springfield, Massachusetts or Chicago, Illinois.

[6] Millimet and Rangaprasad (2007) thoughtfully addresses the difficulties of separating spillovers from unobserved trends, though their empirical approaches might not fully address this problem. 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, neighboring districts' observed demographic variables might be correlated with important omitted variables for the district. In some specifications, Millimet and Rangaprasad use lagged neighbor spending decisions, but this is also problematic if unobserved, common factors take different amounts of time to influence neighboring districts' expenditures.

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.

3. Methodology

Define E_{ijt} as expenditures for district *i* in state *j* at time *t*. Define the parameter *c* as the number of years between observations, so that $\Delta E_{ijt} = E_{ijt} - E_{ijt-c}$. The naïve OLS model estimating fiscal spillovers is:

(1) $$\Delta E_{ijt} = \omega_1 \overline{\Delta E}_{ijt}^{neighbor} + \gamma_{jt}\omega_2 + X_{ijt-c}\omega_3 + u_{ijt} .$$

Similar to the challenge of identifying peer effects (Manski, 1993), estimates of ω_1 in Equation 1 would likely be biased due to omitted variables affecting both ΔE_{ijt} and $\overline{\Delta E}_{ijt}^{neighbor}$. Traditional spatial lag models seek to overcome this challenge by using neighbors' characteristics prior to time *t* to predict neighbors' expenditures at time *t*. But traditional spatial lag model estimates are also biased in this setting if neighbors' characteristics are correlated with important omitted variables that influence future expenditure changes in the district itself. For example, in a period of school finance equalization, a district's expenditure change may reflect not only the district's response to its neighbors' spending changes (the response of interest) but also the district's response to changes in the generosity of the state aid formula that happen to be correlated with lagged characteristics of its neighbors.

This paper's empirical models apply a novel instrumental variables approach to separate fiscal spillovers from common local shocks—exploiting *state-specific* changes in education financing. There is wide within-state variation in school expenditure changes, because states frequently revise their education finance formulas and occasionally establish limits on local taxation.[7] It is difficult to predict the timing of school districts' responses to their states' reforms. Studies of the impact of school finance reforms have thus examined districts' expenditure changes over fairly long periods of time following events. Murray, Evans, and Schwab (1998) discuss the importance of allowing a sufficient time lag for districts to respond to policy changes. Jackson, Johnson, and Perisco (2015) use event-study models to examine the effects of court-ordered finance reforms on students' future earnings; because their treatment measure is based on students' years of exposure to K-12 schooling after these reforms, their models allow up to 12 years for these reforms to reach their full impact. Card and Payne (2002) examine changes over nearly twenty years, from the 1970's to the 1990's. Because knowing the *precise timing* of districts' behavior is critical for testing whether districts respond to *recent* changes in their neighbors' behavior, the state finance policy parameters that have been so useful in these other studies provide insufficient explanatory power to serve as instrumental variables for examining fiscal spillovers over periods of five years or less. State policy variables may also be invalid instruments for examining within-state local fiscal spillovers, because one would have to assume that districts' direct responses to the policy changes are independent of how these districts initially compared with their neighbors' characteristics.

This paper uses an instrumental variable strategy, a cross-border spatial lag model, which overcomes these challenges. The cross-border spatial lag model: (1) predicts districts' revenue changes using similar, in-state districts that are located both far from the districts themselves and far from the relevant state border, (2) predicts average revenue changes among all of a district's neighbors as a function of these predicted changes only for out-of-state neighbors, and (3) examines

[7] Corcoran and Evans (2007) offer a comprehensive review of this literature and historical account of states' court-ordered education finance reforms. Other studies have analyzed cross-state variation in local tax and expenditure limitation policies, including Figlio (1997), Mullins and Wallin (2004), and Downes (2007). Downes and Figlio (1999) also describe states' policies and offer an insightful review of the literature concerning how these policies affect educational outcomes.

how predicted average revenue changes for a district's neighbors affects the district's own revenues, controlling for heterogeneous trends within the district's own state.

The instrumental variable—predicted changes in out-of-state neighbors' revenues based on similar but distant districts in that other state—is powerful, plausibly exogenous, and likely to produce conservative estimates of spillovers. The estimates should be conservative, because districts may be even more responsive to in-state neighbors than to out-of-state neighbors. The validity of the instrumental variable relies on the assumption that the spatial correlation of district-level spending shocks dies out over large distances across state borders. The default minimum distance used below is 100 miles from the state border. While this distance is based on an arbitrary "round number," it seems to be sufficiently large—using a smaller distance of 75 miles does not produce larger estimates of spillovers, suggesting that common shocks have dissipated by 75 miles across state borders.

The instrumental variable is powerful for two reasons. First, similar districts in the same state tend to experience very similar trends in their expenditures due to the importance of state policies. Second, these trends vary greatly across neighboring states. Neighboring states experience different changes in mean district expenditures and in the ratio of expenditures in poor districts versus wealthy districts, even during periods when neither state experienced any official school finance reform or tax reform. To illustrate these points, Figure 1 displays a choropleth map describing district-level changes in operating revenues per pupil between 1987 and 1992. Much of districts' revenue changes are due to within-state policy changes, and it is easy to identify states in Figure 1 even though this map does not include any lines for state borders. A similar pattern occurs if one examines a choropleth map for other years or for percent changes in revenues rather than dollar changes or for expenditures instead of revenues. This graphical evidence is merely suggestive; formal tests of the power of the instrumental variable are described below.

The cross-border spatial lag model isolates changes in districts' expenditures due to exogenous changes in expenditures for nearby districts in another state. The first step is to instrument for the out-of-state neighbors' expenditure changes. I create an instrumental variable for the out-of-state neighbors' expenditure changes based on actual expenditure changes for similar but geographically-distant districts in their states. Similarity is based on five lagged characteristics of school districts: operating expenditures per pupil, mean income, median house value,[8] population density, and the fraction of the population composed of school-aged children (ages 5 to 17). 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.

The cross-border lag model uses predicted changes among those neighbors to instrument for average changes among *all* neighbors. Suppose that district *i* in state *j* is located near the border of another state, state *k*, and define E_{rkt} as expenditures in district *r* located in state *k*. Let q index the four most similar districts to district *r*, among all districts located in state *k*, located at least 100 miles from district *r*, and located at least 100 miles from the border between states *j* and *k*. I predict the change in expenditures in district *r* using the average changes in those four distant comparison districts:

$$\widehat{\Delta E}_{rkt} = \frac{1}{4}\sum_{q=1}^{4} \Delta E_{qkt} \qquad (2)$$

I then calculate the instrumental variable for changes in district *i*'s neighbors' average spending: the average value of $\widehat{\Delta E}_{rkt}$ for district *i's* out-of-state neighboring districts, denoted as

[8] Observations for 1977, 1982, and 1987 are matched based on only four characteristics because house value information is not available from the 1970 Census. This specification assumes that lagged house prices are exogenous and do not reflect anticipated future changes in local taxes and expenditures; if I instead estimate similar models that do not include lagged house values, then the estimates of fiscal spillovers below are smaller: .14 instead of .20 for the baseline model and 0.20 instead of 0.25 for the model restricting the sample to metropolitan areas.

$\overline{\widehat{\Delta E}}_{\iota\jmath t}^{out-of-state\ neighbor}$. I try several different definitions of "neighboring" before settling on a preferred definition of districts with centroid coordinates (center points) located within 30 miles of each other. If district *i* does not have any out-of-state neighboring districts within 30 miles, then $\overline{\widehat{\Delta E}}_{\iota\jmath t}^{out-of-state\ neighbor}$ is set to zero.

The remaining step in the cross-border lag model is to model the effect of out-of-state neighbors on a district's overall average neighbor expenditure changes. The model uses state-by-year fixed effects both alone and also interacted with the fraction of a district's neighbors that are in-state neighbors. That interaction term serves to control for the portion of average neighboring district expenditure changes that are driven by the in-state neighbors. Define π_{it} as the share of district *i*'s neighbors that are located outside of district *i*'s state during year *t*. I estimate the following two equation system using two stage least squares:

(3a) $$\overline{\Delta E}_{ijt}^{neighbor} = \pi_{it}\varphi_1\overline{\widetilde{\Delta E}}_{\iota\jmath t}^{out-of-state\ neighbor} + (1-\pi_{it})\gamma_{jt}\varphi_{2jt} + \gamma_{jt}\varphi_{3jt} + X_{ijt-c}\,\varphi_{4jt} + \varepsilon_{ijt}$$

(3b) $$\Delta E_{ijt} = \beta_1\overline{\Delta E}_{ijt}^{neighbor} + (1-\pi_{it})\gamma_{jt}\beta_{1jt} + \gamma_{jt}\beta_{2jt} + X_{ijt-c}\,\beta_{3jt}\,\gamma_{jt} + e_{ijt}.$$

Estimates of β_1 in Equation 3b reveal the effect of changes in average neighboring district expenditures on districts' own expenditures. The $X_{ij(t-c)}$ vector includes lagged demographic characteristics: population density, percent of the population ages 5-17, percent of the population above the age of 64, and quadratic terms for median income. The model also includes controls for state-by-year fixed effects (γ_{jt}). Because the effects of districts' lagged demographic characteristics are allowed to vary by state and by year, these variables will control for the impacts of state-specific policy changes over time. The identifying variation comes only from variation in districts' out-of-state neighbors.

Although the expenditures variables in Equations 3a and 3b are expressed as first-differences, this model can be derived from a theoretical framework in which the *level* of expenditures in district *i* during year *t* is a function of the *levels* of expenditures in other districts and of district *i*'s demographics. The first-differencing of expenditures in Equations 3a and 3b facilitates tests for heterogeneous effects, such as whether spillovers are greater when districts were previously outspending their neighbors. Changes in district *i*'s demographic characteristics are endogenous, so instead of first-differencing the demographic characteristics, Equations 3a and 3b control for the state-by-year effects of detailed lagged demographic variables (which will be correlated with the exogenous component of districts' demographic changes).

4. Data

The analyses use geographic data for every school district in the United States based on the Census TIGER files, which provide districts' centroid coordinates and allow the researcher to identify which districts share a border. I estimate two sets of models below. The first set has the advantage of covering fiscal spillovers over a relatively long time period when many states enacted meaningful finance reforms: five year intervals from 1972 to 2012. The financial data for (spring of) 1992, 1997, 2002, 2007, and 2012 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 first set of analyses below is the five-year change in school district operating expenditures per pupil, though I use revenues to proxy for expenditures in these particular models because expenditure data are not available throughout this time period.[9] The vast majority of schools' operating expenditures

[9] Operating revenues include all revenues except those earmarked for new construction projects or the maintenance of existing buildings. Observations with suspicious levels of revenues per pupil are removed from the data prior to analysis. I drop a total of 818 observations (0.7 percent of the raw data from 1977 to 2012) due to real operating expenditures per pupil below $400 or above $50,000, (measured in year 2000 $), or due to values suggesting a more than $18,000 change in expenditure per pupil in one five-year period. It is highly unlikely that a district would actually

come from same-year operating revenues, so expenditures per pupil and revenues per pupil have a correlation of about 0.9 during years in which both are available. For expositional convenience, I continue to refer to this dependent variable as the change in expenditures.

The second set of models covers shorter time intervals and a shorter time range, based on the U.S. Department of Education's F-33 data and Common Core of Data from 1992 to 2013. The advantages of these data are that they include revenues funded by local taxes and also include categorical expenditures (e.g., instructional salaries, administrative expenditures). While the first set of models are ideal for precisely estimating overall levels of fiscal spillovers, the second set of models reveal whether certain types of expenditures lead to greater (or more immediate) spillovers and confirm spillovers operating through changes in locally-funded tax revenue. Even where state-funded revenues compose the majority of total public school operating revenues, local citizens or their elected representatives ultimately determine the last dollar spent on local public schools in all districts included in the analyses below.[10]

I combine these financial data with U.S. Census demographic data aggregated to the school district level for 1970, 1980, 1990, and 2000. 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 2007, 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

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undergo such a large change. Similarly, 3-year change analyses (from 1992 to 2013) exclude observations with 3-year changes of more than $7,000 or $18,000 per pupil for categorical expenditures or local tax revenues, respectively.

[10] Total operating revenues (expenditures) 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.

with local school officials, and referenda frequency information for 1970-1972 from Hamilton and Cohen (1974).[11] In the 48 continental states, about 37% of all 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. 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.[12]

A small share of localities experienced school district reorganizations 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 reorganized, 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 reorganization. They include a full

[11] 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 Rhode Island, which required us to survey each district individually. Unilateral changes in individual districts' form of democracy, though rare, might be a source of measurement error in these data.

[12] 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 limited local discretion 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.

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 reorganized.[13]

5. Results

5.1 Descriptive Statistics

Table 1 displays how district operating expenditures per pupil have changed between 1972 and 2012. Real district expenditures per pupil increased for the average district in most periods, except for 1972-1977 when large growth in student populations outpaced revenue growth and 2007-2012 when the country experienced a recession. Mean expenditures per pupil increased rapidly from the late 1970's through the late 1980's, as population growth slowed and many states enacted school finance reforms. Changes in a district's own expenditures per pupil are highly correlated with changes in the mean neighboring districts' expenditures per pupil. This correlation was particularly high during the 1970's and 1980's. The correlation is 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 correlations in expenditures of neighboring districts have decreased considerably over time, as state finance formulas have had an increasing role in determining school district funding.

The Appendix displays the means and standard deviations of descriptive variables used to formulate the independent variables in the 5-year interval regressions below. Districts with at least

[13] The models include a control variable for whether the district reorganized during that time period. Additional estimates, omitted for brevity, test the sensitivity of pre-merging values by instead using a balanced panel containing districts that never underwent any reorganization. Using this restricted sample increases the magnitude of the main 5-year change estimate from .199 to .262.

one out-of-state neighbor tend to be wealthier and to have a greater population density than other districts.

5.2 Power of the Instrumental Variables

The instrumental variable is an extremely powerful predictor of actual mean expenditure changes because states frequently change their education finance formulas, with similar consequences for similar districts within the same state. Table 2 displays the first-stage estimates for the 5-year interval regression models, along with the partial F-statistics for the exclusion restriction. Depending on the specification, the first-stage coefficient on the instrumental variable ranges from 0.749 to 1.005. In all specifications, the instrumental variable is a powerful predictor of actual changes in average neighbors spending, with a partial F-statistic as high as 1,990 for the preferred specification where neighbors are defined based on being within a thirty mile radius of districts' centers.

For the additional analysis using three-year changes in categorical expenditures, the instrumental variables continue to be powerful. The predicted out-of-state neighbor changes by category are strongly related to actual average changes among all neighbors—with partial *F-statistics* of 1214 for locally-funded revenues, 1086 for instructional expenditures, 794 for instructional salaries, 908 for teacher-pupil ratios, 301 for school administration costs, 1931 for student support services salaries, and 145 for capital outlays.

5.3 Main Results for 5-year Intervals

Table 3 reveals estimates of fiscal spillovers given various definitions of neighbors and various methodologies. Each estimate represents the impact on a district's own expenditures per pupil if the mean neighboring district expenditures per pupil increases by one dollar, measured in year 2000 dollars. These regressions control for state-year fixed effects, as well as state-year-specific effects of districts' lagged demographic variables: population density, percent of the population ages 5-17, percent of the population above the age of 64, and quadratic terms for median income. For the sake of comparison, the first column displays estimates from ordinary least squares regressions. The second column displays estimates from this paper's preferred model—the cross-border spatial lag model that limits the identifying variation to predicted changes for out-of-state neighbors. The sample sizes are the same across each column, because districts need not have an out-of-state neighbor to be included in the cross-border spatial lag models. The samples sizes vary slightly between the rows as the definition of neighboring districts changes, because districts must have at least one valid neighbor to be included in the regression.[14]

The point estimate in column 2 of row 1 of Table 3 suggest that a $100 increase in the mean operating expenditures per pupil of districts within a thirty mile radius leads to a roughly $20 increase in a district's own operating expenditures per pupil. As expected, this estimate is positive but smaller than both the OLS estimate. Row 2 of Table 3 display estimates of spillovers among districts that are not only located within a thirty mile radius but also had similar median household incomes. I define a neighboring district as having a similar median income if its previous median income was within 30 percent of the district's own previous median income; this restriction decreases the average number of neighbors by 32 percent. Estimates of spillovers do not increase when the set of neighbors is restricted based on similarity in household income. While demographic similarities are important for spillovers between states (Case, Hines, & Rosen, 1993), geographic proximity is important for spillovers between school districts.[15]

[14] 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.

[15] It would be inappropriate to try additional, non-geographic measures of neighborliness in the local setting, where there is far less reason to suspect that governments care about the behavior of geographically distant governments.

Estimates of spillovers decrease if one alters the definition of neighbors from a thirty mile radius to another geographic criterion, such as a radius of twenty miles or forty miles. 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. Row 3 of Table 3 reveals that the cross-border spatial lag estimates are very small when neighborliness is defined based solely on contiguity, whether districts' borders touch one another. The set of contiguous districts includes a small number of cases in which districts' centroid coordinates are more than thirty miles apart, and, conversely, the set of districts within a thirty mile radius includes numerous small districts that are not contiguous. The thirty-mile radius criterion produces the largest fiscal spillover estimates because there are large, positive fiscal spillovers between proximate, non-contiguous districts that are relatively small in geographic size.

5.4 Additional Specifications Examining Internal and External Validity

The cross-border spatial lag estimates confirm other studies' findings of positive fiscal spillovers, yet it is also important to consider the external validity of these estimates. In theory, responses to out-of-state neighboring districts may 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. Previous work by Coomes and Hoyt (2008) finds that large differences in

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Non-geographic weighting matrices should only be used when there are strong theoretical reasons to suspect that characteristics determined neighborliness; otherwise, models with non-geographical measures of neighborliness may produce spurious evidence of spillovers due to misspecification in the functional form of the independent variables. A non-spatial weighting matrix has the effect of creating additional terms for the explanatory variables, so the coefficients on the interactions between this weighting matrix and the explanatory variables may be non-zero simply because the model omitted important polynomial terms or interaction terms for observations' own characteristics. The spatial econometrics literature too often ignores this potential problem with non-geographic weighting matrices.

states' tax rates affect residential location decisions in multistate metropolitan areas. Compared with less populated areas, residents in metropolitan areas may be particularly attuned to and sensitive to the behavior of their out-of-state neighbors. To investigate this issue, I estimate cross-border spatial lag models while restricting the sample to districts located in Metropolitan Statistical Areas (MSA's).

Column 3 of Table 3 displays estimates for this MSA-only sample. As expected, the estimated fiscal spillovers are larger for districts in metropolitan areas. For a $100 increase in average neighbor spending, the estimated fiscal spillovers for districts in metropolitan areas are $24.7 for the baseline definition of neighbors and $27.5 when neighbors are further restricted to districts with similar demographics. One may view the $19.9 and $24.7 estimates in Row 1 as lower-bound and upper-bound point estimates for typical fiscal spillovers between all types of nearby school districts.

Next, it may be important to verify that the cross-border spatial lag estimates are not biased due to systematic relationships between the observed characteristics of neighboring districts and unobserved characteristics of districts themselves. Unlike traditional spatial lag models, cross-border spatial lag models may include control variables for time-period-specific effects of any type of lagged neighbor characteristic, even the same characteristics used for the first stage behavioral predictions. I estimate additional models that control for state-by-year-specific effects of the lagged average neighbor characteristics—median residential income, median residential income squared, population density, and the fraction of the population between ages 5 and 17. These neighboring-district variables are interacted with state-by-year indicators, where states are the districts' own states. I control for lagged average characteristics from all neighboring districts for one robustness check, and I control for lagged average characteristics for out-of-state neighbors only in a second robustness check. Regardless, the estimates from these models are similar to the .199 estimate from

Table 3; the robustness checks produce estimates of .191 (.071 standard error) and .197 (.062 standard error), respectively. This confirms that the estimates are not biased upward due to coincidental correlation between unobserved shocks and neighboring districts' observed characteristics.

5.5 Falsification Tests

5.5.1 Districts with limited local discretion

Some states have removed school districts' discretion over their level of operating expenditures. 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 expenditures per pupil among neighbors are not due to districts' own fiscal responses. For this falsification test, I re-estimate Equations 3a and 3b using observations from districts lacking control—observations omitted from the analyses above, as described in footnote 12. As expected, the estimates suggest that districts unable to locally determine their budgets do not respond to changes in neighboring districts' expenditures. The estimate coefficient is 0.01, with a standard error of 0.16.

5.5.2 Counterfactual predicted neighbor changes

The cross-border spatial lag model assumes that the spatial correlation of spending shocks almost completely dies out at distances of more than 100 miles across state borders. There is a strong theoretical basis for this assumption, given that state and local governments dominate educational decision-making in the U.S. and that regional housing markets are typically less than 100 miles wide. The robustness of the results to the inclusion of controls for state-by-year-specific effects of lagged mean neighboring characteristics suggests that the baseline estimates are unlikely to be biased from omitted variables. Also, as mentioned earlier, one does not see larger estimates of spillovers using a smaller minimum distance of 75 miles, suggesting that 100 miles should be far enough across state borders to all but eliminate common shocks. Nevertheless, it may be informative to confirm that the estimates are not biased due to any broad regional trends for specific types of districts.

To check this, I found counterfactual predicted changes in neighboring districts' spending using the "opposite state." These counterfactual predictions use 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.[16] This counterfactual model does not produce a statistically significant estimate of spillovers: the estimated coefficient is 0.049 with a standard error of 0.106.

5.5.3 Spillovers via enrollment changes

Given that the dependent variable in these models equals expenditures per pupil, apparent fiscal spillovers might actually be due to changes in the denominator: student enrollments. To verify that the cross-border spatial lag model identifies spillover effects between expenditures 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 five year period. Reassuringly, predicted changes in average neighbors' expenditures per pupil have a statistically insignificant effect on districts' changes in student enrollments (p=.562).

[16] 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.

6. Additional Results

6.1 Heterogeneous Effects

This section describes heterogeneity in fiscal spillovers along five dimensions: (i) whether the district was initially wealthier than its neighbors, (ii) whether the district was initially outspending its neighbors, (iii) the form of local democracy used to determine local school revenues, (iv) whether the districts' state recently enacted a major school finance reform or tax limitation, and (v) whether the district's state had a local tax/expenditure limit in effect. Table 4 displays results from regression models similar to Equation 3 but adding interaction terms between expenditures and indicator variables. The models also include the indicator variable alone as an independent variable if it is not subsumed by the state-by-year fixed effects. The models are otherwise identical to the one used to determine the 0.199 estimate in Table 3.

The results in panel (i) of Table 4 suggest stronger responses among districts that were initially poorer than their average neighbor. The results in panel (ii) reveal that local fiscal spillovers are asymmetric, with much larger responses from districts that were already outspending their neighbors. Districts initially spending more than their average neighbor raise an additional \$31.7 per pupil for a \$100 dollar increase in the average per pupil expenditures of their neighbors; districts initially spending less than their average neighbor only respond to this with an additional \$18.3 per pupil. This large difference in slopes is statistically significant at the .01 level. Local spillovers do not only reflect "keeping up with the Joneses"; they often reflect "staying ahead of the Joneses."

Fiscal spillovers occur regardless of whether local school tax revenue decisions are determined through direct or representative democracy. The estimate is larger for direct democracy districts (\$24.8 versus \$14.9), but this difference in slopes is not statistically significant at conventional levels. Strong spillovers in direct democracy districts reveals that yardstick competition between elected officials is not a necessary condition for local fiscal spillovers; this is noteworthy because yardstick competition has been the central focus of the prior empirical literature on fiscal spillovers.

The fourth panel in Table 4 displays estimates for models that add interaction terms with indicators for whether the state recently enacted a major education finance equalization reform. I use the starting year for court-ordered or legislation reforms, based on Downes & Shah (2006) and Corcoran & Evans (2007),[17] to identify cases when these reforms began during an observation's 5-year interval or during the immediate prior 5-year interval. 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. Districts respond to neighboring districts even if their state has not recently passed an official finance reform. The estimates in panel (iv) of Table 4 suggests that spillovers are actually smaller among districts in states that recently experienced school finance reforms, but this difference in slopes is not statistically significant.

States' local tax limitation rules are another potentially important mediating factor for local fiscal spillovers. Brueckner and Saavedra (2001) find that tax competition between Boston-area municipalities disappeared after the arrival of Massachusetts' Proposition 2½ limited local tax rates. The final panel of Table 4 displays a similar finding for national data using the cross-border spatial lag model.[18] Local fiscal spillovers disappear when states have adopted local tax limitation policies.

[17] Given the timing of the recorded reforms, I restrict the sample for this analysis to changes in expenditures occurring before 2008.

[18] 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.

6.2 Spillovers by Revenue Source and by Type of Expenditure

Data from recent years can provide insights into whether spillovers occur for specific types of categorical expenditures. I estimate models using predicted neighbor categorical spending changes for 3-year periods, (i.e., setting k equal to 3 in Equation 3). To examine the timing of responses, these models also include a lagged term for predicted neighbor changes during the prior 3 year period. The dependent variables in these models cover changes once every 3 years, starting with the 1992 to 1995 period and ending with the 2010 to 2013 period. Due to power limitations, I use 3-year changes rather than annual changes.

First, these data are helpful for exploring whether the estimated fiscal spillovers operate through changes in the local tax rates of districts and their neighbors. I estimate a version of Equation 3 with 3-year changes in *locally-funded revenues per pupil* as the dependent variable and instrument for average neighbor locally-funded revenues per pupil. As shown in the first column of Table 5, the estimated coefficient on the contemporaneous predicted changes in neighbors' local revenues is statistically insignificant, while the estimated coefficient on predicted changes from the prior period equals .22 and is statistically significant at the .10 level. School districts increase their local tax effort for school funding when neighboring districts have increased their own local effort, but this response typically takes more than three years. This delay may reflect both a time lag in observing the neighbors’ actions and a lag in taking action that alters the budget, especially since many districts only consider changing their local tax rates bi-annually or every 3-5 years.

The remaining columns of Table 5 display estimates of fiscal spillovers based on estimation of Equation 3 using three year changes in categorical expenditures. In each model, both the dependent variable and the terms for predicted changes are based on the same type of expenditure change, so these models capture districts’ responses to neighboring districts’ spending changes in particular areas. Table 5 reveals three notable findings. First, large spillovers appear to occur fairly quickly for instructional expenditures, with an estimated effect of $33 per pupil in response to a $100 change in neighbors' instructional expenditures per pupil during the most recent three year period. This response does not appear to be driven by in-kind responses to neighboring districts' changes in teachers-pupil ratios or in instructional salaries; the overall level of instructional expenditures appears to be far more important.

Second, spillover effects are statistically insignificant for non-instructional operating expenditures such as school-level administrative spending and district-level spending on student support service salaries. One might have expected stronger spillovers for school-level administrative costs, particularly if districts respond to changes in neighboring districts' principals' salaries or the number of assistant principals hired by neighboring districts. The cross-border spatial lag model might understate spillovers between in-state districts if principals are not very mobile in terms of switching employment across state boundaries. The lack of a statistically significant estimate for student support services salaries is less surprising, because districts may be less aware of neighboring districts' employment of nurses, school counselors, and other specialists.

Finally, spillovers for capital expenditures are actually negative, though estimated less precisely because capital projects are relatively infrequent. Capital projects for some school districts may delay the approval of capital projects in nearby districts, due to competition for construction crews or due to staggered distribution of capital revenues from higher levels of government. This finding of negative spillovers for capital expenditures may help to explain Cellini et al.'s (2010) finding that some districts' infrastructure investments induce large increases in local house values; these large, immediate housing premiums are possible because districts do not quickly copy their neighbors' infrastructure investments.

7. Conclusion

School districts' expenditures respond to changes in nearby districts' expenditures. Conservative estimates of fiscal spillovers between districts are in the neighborhood of $20 per $100 change in average neighbors' expenditures, where the relevant set of neighbors is districts located within a thirty mile radius. These conservative estimates may understate overall fiscal spillovers because some districts are less responsive to out-of-state neighbors' behavior than other neighbors' behavior. The cross-border spatial lag estimates exceed $24 for spillovers within the same metropolitan area.

Additional results suggest that yardstick competition due to political agency problems—the focus of much of the prior empirical literature on fiscal spillovers—may not be necessary for spillovers between local governments. Spillovers between in-state neighboring districts are actually greater when districts use direct democracy rather than representative democracy, though this difference is not statistically significant. Informational spillovers between school districts would be important in direct democracy settings if citizens interpret nearby districts' behavior as a valuable signal concerning the potential benefits of changing local tax rates. Signals may be stronger as larger majorities of voters in neighboring districts support higher expenditure levels, which could explain why Isen's (2014) regression discontinuity models produce much smaller estimates than those found here. There may also be yardstick competition in direct democracy districts due to political agency problems, 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, 2008).

Additional analyses confirm that fiscal spillovers are partly due to responses to districts' *locally-funded* tax revenues per pupil. This local budget response takes a few years to occur, possibly due to the infrequency of consideration of substantial tax rate changes. Districts respond more immediately to changes in some categorical expenditures—particularly instructional expenditures. Districts do not experience positive spillovers for other types of categorical expenditures. Changes in instructional expenditures may be particularly salient for the residents of local communities.

Many additional interesting questions related to local fiscal spillovers deserve further study but are beyond the scope of this paper. One important question is whether fiscal spillovers are related to non-residential property wealth—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. Future research might also investigate potential general equilibrium effects of neighboring local governments' spending changes.

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

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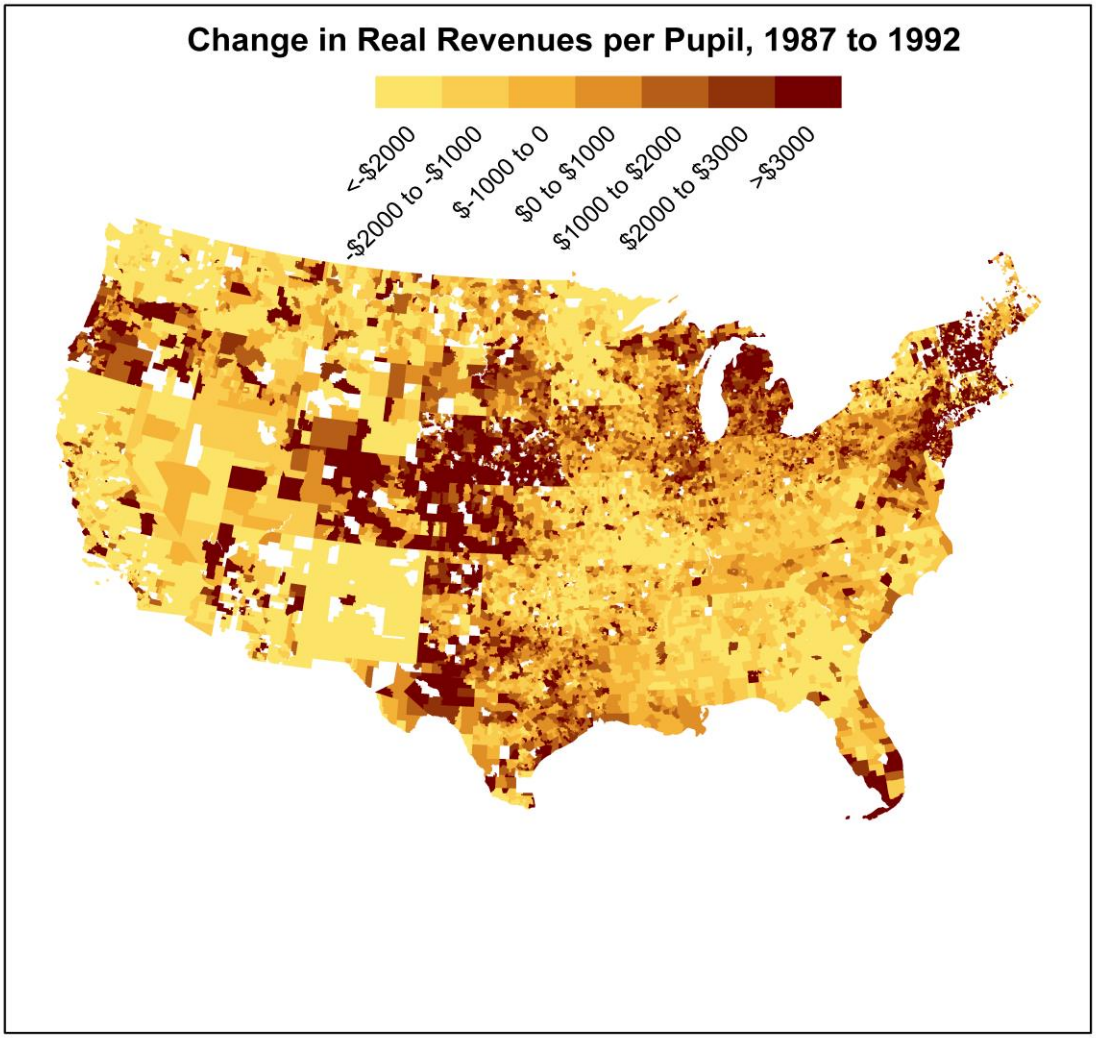
Change in Real Revenues per Pupil, 1987 to 1992
<-$2000
-$2000 to -$1000
$-1000 to 0
$0 to $1000
$1000 to $2000
$2000 to $3000
>$3000

Table 1: Changes in School District Operating Expenditures per Pupil, 1972-2012

Time Period	Mean (Year 2000 $)	Standard Deviation	Correlation of Change in Operating Expenditures per Pupil with those of…		
			All neighbors	In-state neighbors	Out-of-state neighbors
1972-77	-1,889	1,870	.493	.490	.362
1977-82	2,808	2,291	.490	.483	.380
1982-87	485	2153	.417	.425	.162
1987-92	1,635	2,114	.464	.464	.258
1992-97	605	2,213	.199	.203	.063
1997-2002	1,601	2,769	.184	.184	.038
2002-2007	367	2,907	.238	.232	.090
2007-2012	-87	2,915	.268	.263	.021

Notes to Table 1: As in the paper's main analyses, neighboring school districts are those with centroid coordinates that are within thirty miles of each other. For these 5-year intervals, I use changes in operating revenues to proxy for changes in expenditures.

Table 2 First-stage Estimates from Cross-border Spatial Models

Sample:	Full	MSA-only
<u>Definition of Neighbors</u>		
(1) Districts within a 30 Mile Radius	0.893	1.005
	(0.020)	(0.030)
partial F-statistic	*1990.0*	*1144.8*
(2) Districts within a 30 Mile Radius and with Similar Median Household Income	0.753	0.749
	(0.020)	(0.030)
partial F-statistic	*1355.8*	*625.0*
(3) Contiguous School Districts	0.859	0.973
	(0.031)	(0.059)
partial F-statistic	*792.2*	*269.8*

Notes to Table 2: Each cell displays an estimated coefficient of φ_1 in Equation 3a, followed by its standard error in parentheses, followed by the partial F-statistic in italics for the exclusion restriction between Equations 3a and 3b.

Table 3: Estimates of Fiscal Spillovers between U.S. School Districts

	OLS	Cross-border spatial lag model	Cross-border spatial lag model
Sample:	Full	Full	MSA-only
<u>Definition of Neighbors</u>			
(1) Districts within a 30 Mile Radius	.282 (.016)	.199 (.062)	.247 (.103)
(2) Districts within a 30 Mile Radius and with Similar Median Household Income	.236 (.014)	.164 (.065)	.275 (.117)
(3) Contiguous School Districts	.354 (.013)	.100 (.067)	.053 (.114)

Notes to Table 3: Each cell represents a separate regression and reveals the estimated change in a district's operating expenditures per pupil from a one dollar increase in the average operating expenditures per pupil among neighboring districts during five year intervals from 1972 to 2012. 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 and for recent district reorganizations. The full regression sample contains 88,918 district-level observations and the MSA-only (metropolitan area) sample includes 41,473 district-level observations. Each sample excludes cases where districts lacked local discretion over operating expenditures per pupil.

Table 4: Heterogeneous Fiscal Spillovers

	Estimated Spillovers	*p*-value of difference in estimates
***(i)** Initial Median Household Income Compared to Neighboring Districts' Initial Median Incomes*		
> Average Income Among Neighbors	.142	
	(.065)	*.003*
≤ Average Income Among Neighbors	.235	
	(.062)	
(ii) *Initial Expenditures per Pupil Compare to Neighboring Districts' Initial Expenditures*		
> Avg. Neighbors' Expenditures per Pupil	.317	
	(.077)	*.007*
≤ Avg. Neighbors' Expenditures per Pupil	.183	
	(.070)	
***(iii)** Form of Local Democracy Determining Local School Revenue Levels*		
Direct Only	.248	
	(.085)	*.325*
Representative	.149	
	(.075)	
***(iv)** Official State-level School Finance Reforms (pre-2008)*		
Recent Legislative or Court-ordered Reform	.146	
(current 5-year period or prior 5-year period)	(.118)	
		.451
No Recent Reform	.252	
	(.070)	
***(v)** Statewide Local Tax/Expenditure Limits*		
State Has Local Tax/Expenditure Limit in Place	.024	
(current 5-year period or prior 5-year period)	(.090)	
		.072
State Does Not Have Limit	.238	
	(.073)	

Notes to Table 4: Neighboring districts are defined as those located within a 30 mile radius. Panels (i) through (v) each lists two estimated coefficients from a single regression in which the first stage and second stage expenditure variables are interacted with indicator variables for the two categories listed. These models control for the independent variables described in the notes to Table 3.

Table 5: Estimates of Categorical Spillovers: 3-Year Changes for School Districts from 1992 to 2013

	Three Year Change in the District's Per Pupil…						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Local Revenue	Instructional Expenditures	Instructional Salaries	Teachers	School-level Administrative Costs	Student Support Service Salaries	Capital Construction/ Improvement Outlays
Avg. Neighboring Districts' Categorical Change During Same 3 Years	-.12 (.17)	.33** (.13)	.11 (.10)	-.08 (.10)	-.06 (.26)	.14 (.17)	-.41** (.21)
Avg. Neighboring Districts' Categorical Change During Prior 3 Years	.22* (.12)	.03 (.09)	.13 (.12)	.02 (.07)	-.05 (.17)	.006 (.04)	-.23 (.22)
N	66,844	66,836	66,846	60,690	63,885	61,820	66,936

Notes to Table 5: Each column displays results from the cross-border spatial lag model specified in Equation 3 using three year time intervals (i.e., with k=3). Each regression controls for state-year fixed effects, as well as for the state-year specific effects of lagged demographic variables and for recent district reorganizations. The sample excludes districts which lack local discretion over operating expenditures per pupil. The sample size varies across columns because teacher-pupil ratios are unavailable for some districts and because suspiciously large changes are removed from the sample.

Appendix: Summary Statistics for School District Characteristics,
Means with Standard Deviations in Italics

	Full Sample	Districts with Predicted Expenditure Changes for at least one Out-of-State Neighbor[a]
Number of District-Level Observations	88,918	28,560
Operating expenditures per pupil (year 2000 $) (at the beginning of each 5 year period)	7,447	7,833
	4,022	*4,355*
5-year change in operating expenditures per pupil	688	744
	2,665	*279*
<u>Values from Immediate Prior Census</u>		
Median Household Income (year 2000 $)	38,029	42,119
	14,032	*17,532*
Proportion of the Population Ages 5-17	0.22	0.22
	0.05	*0.05*
Proportion of the Population Ages 65 & over	0.13	0.13
	0.05	*0.05*
Population Density (people per km^2)	230	413
	723	*109*

[a] In this paper's main analyses, neighboring districts are defined as districts with centroid coordinates located within thirty miles of each other.