FISCAL SPILLOVERS BETWEEN LOCAL GOVERNMENTS:

KEEPING UP WITH THE JONESES' SCHOOL DISTRICT

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Abstract

This is the first national study of fiscal spillovers between local governments and the first to examine whether fiscal 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 a national panel of school-district level data, 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. Additional specifications focus on local spillovers in metropolitan areas and spillovers across relatively permeable state borders to reveal how the initial, conservative estimates of local spillovers may compare with in-state local spillovers. The results reveal that districts follow their neighbors' lead for changes in school expenditures. Districts also respond to changes in their neighbors' categorical expenditures: they respond quickly to changes in average class sizes and respond forcefully to changes in expenditures on teachers' salaries.

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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.[1] 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), 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. More recent work has found evidence of positive spillovers specifically in terms of states' welfare spending (Figlio, Kolpin, and Reid, 1999), states' Medicare spending (Baicker, 2005), or county revenues (Baicker, 2004). 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

[1] 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.

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.[2]

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 for localities across the nation. I employ a novel instrumental variables strategy which examines whether school districts located near state borders respond to the predicted fiscal behavior of neighboring districts located in a different state.[3]

These out-of-state neighbor predictions are based on changes in local expenditures among otherwise similar districts in that state located at least one hundred miles from the relevant state border. Because of frequent state-specific changes in education finance and tax policies, similar in-state districts' revenue changes are powerful predictors of the near-border districts' expenditures changes. The models control for the effects of states' own important finance changes while allowing fiscal spillovers to be identified solely based on differences for very similar districts that happen to have different out-of-state neighbors or no out-of-state neighbors at all. Given this specification, the estimates could only 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—models continue to reveal evidence of fiscal spillovers even when adding detailed controls for characteristics of districts' neighbors and/or adding controls for border-by-year fixed effects. Additional analyses explore how these conservative estimates of fiscal spillovers might compare with fiscal spillovers across districts within the same states. These analyses examine spillovers between near-border districts in metropolitan areas with high rates of inter-state residential mobility.

[2] Please see Revelli (2005) for a more detailed review of these and related studies.

[3] 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 (forthcoming) exploit state borders to examine the impact of statewide teacher tenure policies on teacher salaries across districts within the same metropolitan areas.

This is also the first empirical study 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 local tax competition may be another important mechanism for fiscal spillovers.[4]

The empirical findings described below suggest that a one dollar increase in the mean per pupil operating expenditures of nearby districts causes a district to increase its own per pupil operating expenditures by about 25 cents. This estimate is substantially lower than the corresponding estimate from an ordinary least squares model (38 cents) and this estimate is slightly 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 25 cents, exploring heterogeneous responses helps to reveal which mechanisms are most consistent with observed behavior.[5] Responses are largest for districts that were initially outspending their neighbors and districts located in metropolitan areas. 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 analysis reveals that positive spillovers occur specifically for local tax revenues and certain categorical expenditures. Districts respond quickly to their neighbors' changes in class sizes and respond forcefully to their neighbors' changes in expenditures on teachers' salaries. Unlike these operating expenditures, outlays for capital expenditures do not produce statistically significant short-term spillovers across neighboring districts.

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.

[4] See, for example, Wilson, 1999, Brueckner, 2000, Brueckner & Saavedra, 2001, Brueckner, 2003, Wildasin, 2003, Wilson & Wildasin, 2003.

[5] 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. 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' 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 other 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.[6] 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.[7]

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.

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[6] 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.

[7] Externalities could lead to positive or negative spillovers. There may be complementarities leading to positive spillovers, (e.g., the presence of field hockey teams in neighboring school districts increases the benefit of adding a team). Alternatively, neighboring districts' spending may 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).

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 school districts, there have been a few prior empirical studies investigating fiscal spillovers between municipalities or school districts in specific states.[8] Brueckner and Saavedra (2001) investigate fiscal spillovers between 70 municipalities in the Boston area. They empirically test for spatial endogeneity in their models and fail to reject the null hypothesis that their independent variables are exogenously determined.[9] Brueckner and Saavedra find evidence of positive spillovers between the Boston-area municipalities, and they also find evidence that these spillovers disappeared after Proposition 2½ limited most of these municipalities' ability to increase local property taxes. 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.[10] Babcock,

[8] 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.

[9] 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. In the national data set, one would have to make the dubious assumption that omitted variables for the Boston-area town do not affect demographics in neighboring Boston towns more than they affect demographics in places like Springfield, Massachusetts and Chicago, Illinois.

[10] 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, neighboring districts' observed demographic variables might be correlated with important omitted variables for the district. The estimates in Table 2 below suggest that this type of instrumental variables model may produce estimates

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.

3. Methodology

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.[11] 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: Card and Payne (2002) examine changes from the 1970's to the 1990's, Hoxby (2001) uses a district-fixed effects model with ten year intervals, and Murray, Evans, and Schwab (1998) use models that either allow reforms to permanently affect all future periods or to have effects that increase over time. The latter two studies each discuss the importance of allowing a sufficient time lag for districts to respond to policy changes. 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 short-term or medium-term fiscal spillovers. State policy variables also might not be valid instruments for examining within-state local fiscal spillovers, because one would have to assume districts' direct responses to the policy changes do not depend on how these districts initially compare with their neighbors' characteristics.

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that are biased toward OLS estimates—though the size of this bias is not always large. 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.

[11] Several studies have examined the impact of state education finance reforms, including Murray, Evans, Schwab (1998), Hoxby (2001), Card and Payne (2002), and Downes and Shah (2006). 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.

This paper uses a cross-state-border instrumental variable strategy that overcomes these challenges. The two-step estimation procedure: (1) predicts districts' revenue changes using similar, in-state districts that are located far from the district itself and far from the relevant state border, and then (2) determines how predicted changes for nearby, out-of-state districts are related to districts' own revenue changes, controlling for heterogeneous trends within the districts' own states. This type of 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, because states share borders with districts and so district-level variation may appear to be sensitive to state location even when this is not the case. The formal tests of power described below are even more revealing than this suggestive visual evidence.

To instrument for changes in districts' expenditures, I first compare districts within the same state during the same year based on five lagged characteristics: operating expenditures per pupil, mean income, median house value,[12] 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 are most similar to district X based on these lagged characteristics, subject to these four districts being at least some minimum distance from district X and some minimum distance from the state border close to district X.[13] I use the average actual change in expenditures among these four comparison districts as an instrumental variable for the change in district X's expenditures.

[12] 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 one instead estimates similar models that do not include lagged house values, then the estimates of fiscal spillovers below are slightly larger. .

[13] 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.

For the analyses below, I use a minimum distance of 100 miles for the comparison districts. While this distance choice is ad hoc, further analyses support the idea that spatially-correlated expenditure shocks do not spread as far as 100 miles across state borders. On the other hand, using a much shorter minimum distance would inflate the estimates to the point where they are nearly as large as the ordinary least squares estimates, ostensibly because spatially-correlated shocks bias the results.

Using these predicted revenue changes, I can isolate changes in districts' expenditures due to exogenous changes in expenditures for nearby districts in another state. Define E_{ijt} as a measure of expenditures in district *i* located in state *j* during year *t*. Suppose that changes in district *i*'s expenditures are influenced by recent changes in the mean expenditures among neighboring districts, as well as trends related to state finance policies and 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. Traditional instrumental variables models identifying spatial spillovers—i.e., spatial lag models—have the following general form:

(1)
$$E_{ijt} - E_{ijt\text{-}k} = \beta_1 W_1 \widehat{(E_t - E_{t-k})} + \gamma_{jt}\beta_2 + \gamma_{jt} X_{ijt\text{-}k}\beta_{3jt} + e_{ijt}$$
$$e_{ijt} = \lambda_t W_2\, e_{ijt} + \varepsilon_{ijt}$$
$$\text{with } |\beta_1|<1,\ |\lambda_t|<1\ \forall t, \text{ and } \varepsilon_{ijt}\sim N(0,\sigma^2),$$

where *k* is some constant, γ_{jt} represents a vector of state-by-year indicator variables and $\gamma_{jt}X_{ijt\text{-}k}$ represents these indicator variables interacted with lagged control variables capturing district *i*'s characteristics. I modify this model to create a "cross-border spatial lag model," identifying districts' responses to their mean neighbors' change solely from responses to out-of-state neighbors. 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. Using S to partition the mean change in neighbors' expenditures on the right hand side of equation (1) yields:

(2)
$$E_{ijt} - E_{ij(t\text{-}k)} = \beta_1 W_1 (A\text{-}S)\, \widehat{(E_t - E_{t-k})} + (\beta_{4j} W_1 S + \beta_2)\gamma_{jt} + \gamma_{jt} X_{ij(t\text{-}k)}\beta_{3jt} + e_{ijt}$$
$$e_{ijt} = \lambda_t W_2\, e_{ijt} + \varepsilon_{ijt}$$
$$\text{with } |\beta_1|<1,\ |\lambda_t|<1\ \forall t, \text{ and } \varepsilon_{ijt}\sim N(0,\sigma^2).$$

Estimates of β_1 provide unbiased estimates of the effect of local fiscal spillovers across state borders. The variables in the $X_{ij(t-k)}$ vector include detailed, lagged demographic characteristics. Because the effects of districts' lagged demographic characteristics are allowed to vary by state and by year, these variables will control for the impact of policy changes in districts' own states.[14]

Alternative specifications of the cross-border spatial lag model represented in equation (2) are feasible. Instead of using first-differences, some researchers estimate spatial lag models using fixed effects. The baseline estimates of equation (2) are very similar to estimates from an analogous district-level fixed effects model. The first-differences models in this paper facilitate estimation of additional specifications that differentiate fiscal spillovers based on recent state reforms or pre-existing relationships between districts and their neighbors.

Some specifications below include extra control variables—i.e., state border-by-year fixed effects and/or state-by-year specific effects of mean neighboring district characteristics. Others test for delayed responses by adding lagged predicted changes in neighbors' expenditures as independent variables. Others test for different types of heterogeneous effects, such as greater spillovers among districts in metropolitan areas or districts with high inter-state residential mobility. Finally, I conduct several falsification tests to verify the validity of the cross-border spatial lag approach.

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 2007. The financial data for (spring of) 1992, 1997, 2002, and 2007 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

[14] Equations (1) and (2) can be derived from a 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 current demographics, i.e., $E_{ijt}=f(W_1E_t, X_{ijt})$. While the empirical models first-difference the expenditures 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.

use revenues to proxy for expenditures in these particular models because expenditure data are not available throughout this time period.[15] The vast majority of schools' operating expenditures 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 2007. The advantages of these data are that they include districts' operating expenditures, their revenues funded by local taxes, and their 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 that spillovers largely operate through changes in local 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.[16]

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 with local school officials, and referenda frequency information for 1970-1972 from Hamilton and

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[15] 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. 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; it is highly unlikely that any district would actually undergo such a large change. The estimates remain fairly similar, (within .05 for the instrumental variable estimates displayed in row 1 of Table 2), if one fails to drop any of these outliers. Similarly, the second set of analyses exclude observations with suspiciously large 3-year changes of more than $5,000 or $7,000 per pupil for categorical or overall expenditures, respectively.

[16] 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.

Cohen (1974).[17] 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.[18]

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 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 and whether any of a district's neighbors reorganized.[19]

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[17] 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.

[18] 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, but I also account for California's parcel tax option in additional analyses that focus on districts lacking local discretion.

[19] The 5-year change models include three control variables related to reorganizations: an indictor for whether the district reorganized 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. The 3-year change models include an indicator for whether the district reorganized during that c time period. Additional estimates, omitted for brevity, test the sensitivity of pre-merging values in the 5-year change specification by instead using a balanced panel containing districts that never underwent any reorganization. Using this restricted sample slightly increases the estimates of fiscal spillovers, (e.g., from .249 to .271 for the model in the first row of column 1 of Table 2).

5. Results

5.1 Descriptive Statistics

Table 1 displays how district operating expenditures per pupil (proxied by revenues) have changed between 1972 and 2007. Each five year interval was associated with a rise in real mean district expenditures per pupil, except for the period between 1972 and 1977 when large growth in student populations outpaced revenue growth. Mean expenditures 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 expenditures per pupil are highly correlated with changes in the mean neighboring districts' expenditures 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 the independent variables in the 5-year interval regressions below. Districts with at least 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 changing their education finance formulas, with similar consequences for similar districts within the same state. The predicted mean neighbor expenditure change alone explains almost 50% of the raw within-state-by-year variation in actual mean neighbors' 5-year expenditure changes, (i.e., in a model that controls for state-by-year fixed effects but nothing else). These predictions are powerful even when including the same control variables as in equation 2. Using a sample of districts with at least one out-of-state neighbor, I regressed mean 5-year expenditure changes among out-of-state neighboring districts on their predicted values and these control variables. The exclusion restriction for the mean predicted changes variable has a *partial F-statistic* above 10,000 and a *p*-value less than .000001.

To motivate the analysis of spillovers across categorical expenditures in Section 6.2 below, note that the instrumental variables are also powerful for three-year changes in categorical expenditures across districts in metropolitan areas. The predicted mean out-of-state neighbor changes for these districts are strongly related to actual mean out-of-state neighbor changes—i.e., changes in teacher-pupil ratios (*partial F-statistic*=303), instructional salary costs per pupil (*partial F-statistic*= 568), school administration costs per pupil (*partial F-statistic*=69), student support services salary cost per pupil (*partial F-statistic*=110), and capital outlays (*partial F-statistic*=87).

5.3 Main Results for 5-year Intervals

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 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. From left to right, each column of Table 2 displays estimates for models which are increasingly likely to reflect only responses to exogenous changes in neighbors' behavior. For the sake of comparison, the first column displays estimates from ordinary least squares regressions and the second column displays estimates from a traditional spatial lag model, estimated using a two-step instrumental variables approach. The third 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 (based on Equation 2 above). 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.[20] The proportion of neighboring districts used to identify spillover effects may vary across the rows in the third column of Table 2, but note that the weighting of the first two right-hand-side terms in Equation 2 will mechanically scale the first term based on that proportion. In other words, the estimates displayed in this paper's tables should be interpreted as *responses to average changes among all neighboring districts*, even though these estimates are primarily identified from a subset of the neighboring districts.

[20] 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.

The point estimate in column 3 of row 1 of Table 2 suggest that a one dollar increase in the mean operating expenditures per pupil of districts within a thirty mile radius leads to a 24.9 cent increase in a district's own operating expenditures per pupil. As expected, this estimate is positive but smaller than both the OLS estimate (37.9 cents) and the traditional spatial lag model estimate (28.1 cents). The estimate for the cross-border spatial lag model remains statistically significant if one adjusts the standard errors for spatial and serial autocorrelation.[21]

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. I define a neighboring district as having a similar median income if its median income is within 20% of the district's own median income, and approximately half of all neighbors meet this criterion. Estimates of spillovers do not increase when the set of neighbors is restricted based on similarity in household income—the cross-border spatial lag model's estimate decreases to 19.1 cents.

These results suggest that significant fiscal spillovers are unlikely to occur between districts with similar demographic characteristics if they are not geographically proximate. Otherwise, limiting the set of local neighbors to districts with similar demographic characteristics would have increased the estimates of fiscal spillovers, because these similar districts' predicted behavior more closely captures the behavior of distant districts with similar demographic characteristics. While demographic 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. 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. 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

[21] The standard errors reported in the tables do not account for potential spatial autocorrelation and serial autocorrelation. Adjusting for autocorrelation only increases the standard error of the main estimate, (column 3 of row 1 of Table 2) from .04 to .05. I computed this adjusted standard error 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.

may produce spurious evidence of spillovers due to misspecification in the functional form of the independent variables.[22]

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 point 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, (based on estimates from additional models not shown here for brevity). 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. 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.[23] This paper's remaining analyses thus define neighboring districts as those within a thirty mile radius.

Additional analysis reveals that five year periods are sufficiently long to capture these fiscal spillovers. If I add lagged variables for the first two right-hand side terms in equation 2, then the estimated coefficient of the predicted neighbors' change from the prior 5-year period is statistically insignificant and the baseline estimate of spillovers barely changes. Section 6.2 below explores the timing of fiscal spillovers in more detail.

5.4 Additional Specifications Examining Internal and External Validity

Table 3 displays results from various alternative specifications of the cross-border spatial lag model. To facilitate comparison, the estimate in the first row of the first column of Table 3 is identical to the estimate in the first row of the third column of Table 2.

First, 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

[22] 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.
[23] 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.

of lagged neighbor characteristic, even the same characteristics used for the first stage behavioral predictions. The second row of Table 3 displays estimates from models that control for state-by-year-specific effects of lagged mean neighbor characteristics—mean residential income, median house values, the fraction of the population above the age of 64, and the fraction of the population between ages 5 and 17. These neighboring-district variables are based on in-state neighbors, as well as out-of-state neighbors where relevant, and are interacted with state-by-year indicators based on districts' own locations. The estimates in the second row of Table 3 are never significantly smaller than the estimates in the first row of Table 3, confirming that the estimates are not biased upward due to coincidental correlation between unobserved shocks and neighboring districts' observed characteristics.

Next, it may be important to verify that the estimates of fiscal spillovers do not disappear if there is a change in the identifying variation. Equation 2 identifies spillovers by comparing a border district's behavior with similar in-state districts that 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. Column 2 of Table 3 displays estimates from models that eliminate this type of identifying variation by controlling for state border-by-year fixed effects. These models should produce even more conservative estimates of fiscal spillovers, because the fixed effects remove some of the actual fiscal responses[24] and might also eliminate upward biases caused by common regional shocks.[25] Estimates of spillovers in these border-by-year fixed effect models are identified solely from the behavior of districts along the same side of the same state border with out-of-state neighbors that possess different initial characteristics.[26] Reassuringly, these

[24] Theory predicts stronger fiscal spillover estimates in these models when districts are responding to widespread changes in a neighboring state. The estimates in Equation 2 are estimates of districts' best responses to changes in out-of-state neighboring districts, which include indirect effects operating through districts' responses to their in-state neighbors that are also responding to other out-of-state districts. If all out-of-state neighboring districts are experiencing similar fiscal trends, then similar responses may occur among districts' in-state neighbors. Fiscal spillover estimates could thus decrease if the models add controls for border-by-year fixed effects, isolating idiosyncratic predicted changes in districts' behavior on the other side of the state border.

[25] 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 expenditures far more than Ohio districts increased their expenditures. Differences in revenue growth between districts in eastern versus western Pennsylvania may be partially due to coincidental differences in omitted variables across the two regions of Pennsylvania. While this scenario may occasionally occur, there is little reason to believe that it would systematically occur because this would require that temporary shocks consistently extend more than 100 miles past one side of a state border but do not extend very far on the other side of that border.

[26] For each five year interval, border-by-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 from footnote 25, this

border-by-year fixed effect models produce positive and statistically significant estimates of fiscal spillovers—estimates of 0.188 in the first row and 0.143 in the second row.

The first two columns of Table 3 have confirmed the internal validity of positive estimates of fiscal spillovers, and it is also important to consider the external validity of these cross-state-border estimates of local spillovers. In theory, responses to out-of-state neighboring districts may differ from responses to in-state neighboring districts—i.e., β_1 might not be equivalent across Equations 1 and 2. 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 models from column 1 of Table 3 but restrict the sample to districts located in Metropolitan Statistical Areas (MSA's). 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. Column 3 of Table 3 displays estimates for this MSA-only sample. Previous work by Coomes and Hoyt (2008) finds that large differences in states' tax rates affect residential location decisions in multistate metropolitan areas. To better isolate metropolitan districts that are close substitutes, column 4 of Table 3 displays estimates from models further restricting the source of identification to responses among border districts with rates of inter-state residential mobility above the median rate for border districts.[27]

As expected, the estimated fiscal spillovers are greater for metropolitan districts. Estimated fiscal spillovers for districts in metropolitan areas are 0.258 and 0.354 for the two models in column 3 of Table 3. They are even larger for metropolitan districts with high inter-state residential mobility: 0.300 and 0.402. Greater fiscal spillover estimates where residents are relatively mobile suggest that cross-border spatial lag model estimates may understate fiscal spillovers between in-state districts. Taking all of these results together, one may view the 0.143 estimate in row 2 of column 2 and the 0.402 estimate in row 2 of column 4 as lower-bound and upper-bound point

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model would control for the average difference during each time period between the changes in expenditures in Pennsylvania districts bordering Ohio and the changes in expenditures in Pennsylvania districts bordering New Jersey.

[27] 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 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.

estimates for typical fiscal spillovers between all types of school districts. The baseline estimate of about 0.25 thus lies fairly close to the middle of this range.

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 equation 2 using observations from districts lacking control. This sample includes 1,431 observations from Nevada, New Mexico, and some Californian districts.[28] 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 attempting to adopt a parcel tax to fund local school operating expenditures, while this probability was extremely low for all other Californian districts.[29]

As expected, the estimates suggest that districts unable to locally determine their budgets do not respond to changes in neighboring districts' expenditures. The estimate of spillovers for these districts is -.076 (standard error of .169) using the model equivalent to row 1 of column 1 of Table 3. At the .10 level, one can thus reject equality of the spillover estimates for districts with and without local discretion.

5.5.2 Counterfactual predicted neighbor changes

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[28] 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.

[29] 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 slightly increases the estimates of fiscal spillovers among "no-local-control" districts.

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 in this context, given the dominance of state and local governments in educational decision-making in the U.S. and conventional notions of regional housing markets in the U.S. 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. Nonetheless, 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.[30] This counterfactual model never produces statistically significant, positive estimates of spillovers.

5.5.3 Spillovers via enrollment changes

Given that the dependent variable in these models equals expenditures per pupil, one might worry that the apparent fiscal spillovers are actually 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 neighbors' expenditures per pupil have statistically insignificant effects on districts' own enrollment changes (p=.963).

6. Additional Results

6.1 Heterogeneous Effects

This section describes heterogeneity in fiscal spillovers along several dimensions: 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

[30] 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.

districts' state recently enacted a major school finance reform or tax limitation. Table 4 displays results from 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' expenditures. They add these additional variables to a model similar to the one used to determine the 0.247 estimate in the second row of column 1 of Table 3. Note that the inclusion of the indicator term as additional control variable can cause the average values of the estimated slopes in Table 4 to differ from this original 0.247 estimate.

The estimates in panels (i) and (ii) of Table 4 reveal that local fiscal spillovers are asymmetric. While the results in panel (i) do not suggest strong heterogeneity in spillovers based on whether a district is relatively wealthy, the results in panel (ii) suggest that fiscal responses are much stronger among districts that had already been outspending their neighbors. Districts initially spending more than their average neighbor raise an additional 53.7 cents per pupil for a one 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 20.7 cents per pupil. This large difference in slopes is statistically significant at the .001 level. Local spillovers are less often a matter of "keeping up with the Joneses" than a matter of "staying ahead of the Joneses."

Fiscal spillovers occur regardless of whether local school tax revenue decisions are determined through direct or representative democracy. Panel (iii) of Table 4 reveals statistically significant fiscal spillover estimates of more than 20 cents per pupil regardless of the form of local democracy. The estimate is slightly larger for direct democracy districts (28.3 cents), but this difference in slopes is not statistically significant. Strong spillovers in direct democracy districts suggest that yardstick competition between elected officials is not a necessary condition for local fiscal spillovers.

The final 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),[31] 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 simply assumes that their adoption is not related to

[31] Some of the classifications in Downes & Shah (2006) and Corcoran & Evans (2007) come from the earlier classifications of Murray, Evans, & Schwab (1998), Hoxby (2001), and Card & Payne (2002).

unobserved variables which influence the magnitude of fiscal competition. The estimates in panel (iv) of Table 4 suggests that responses are larger among districts in states that recently experienced school finance reforms, but this difference in slopes is not statistically significant. Districts respond to neighboring districts even if their state has not recently passed an official finance reform. In additional analyses, (not shown here in the interest of brevity), I confirm that the converse is also true—i.e., point estimates of spillovers remain similar if I limit the identifying variation to predicted changes for out-of-state districts in states that recently experienced major finance reforms.

States' local tax limitation rules are another potentially important mediating factor for local fiscal spillovers. Brueckner and Saavedra (2001), for example, find that tax competition between Boston-area municipalities disappeared after the arrival of Massachusetts' Proposition 2½ limited local tax rates. In additional analyses not shown here, I examine whether local fiscal spillovers disappear shortly after states adopt local tax limitation policies.[32] Fiscal spillovers between school districts are slightly *greater* shortly after states enact local tax limitations, though this difference is only marginally significant (p=.12). While this result may seem surprising, 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 out-of-state neighbors when deciding whether to raise the maximum allowable revenues.

6.2 Spillovers by Revenue Source and by Type of Expenditure

Data from recent years, (1992 to 2007), can provide insights into whether spillovers occur for specific types of categorical expenditures. I estimate models using predicted neighbor expenditure changes for 3-year periods, (i.e., setting k equal to 3 in equation 2). 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 thus cover changes from 1995 to 1998, 1998 to 2001, 2001 to 2004, and 2004 to 2007. Due to power limitations, I use 3-year changes rather than annual changes and I limit the sample to districts located in metropolitan areas—where spillovers should be relatively large if they do occur.

[32] 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.

First, these data are helpful for confirming that the estimated fiscal spillovers are truly due to local school districts' responses to their neighbors via changes in their local tax rates. I estimated a version of equation 2 with 3-year changes in *locally-funded revenues per pupil* as the dependent variable. The estimated coefficient on the contemporaneous predicted changes in neighbors' revenues equals 0.318 (.107 standard error), confirming that school districts increase their local tax effort for school funding when neighboring districts increase their overall operating revenues. The estimated coefficient on predicted neighbor revenue changes from the *prior* 3-year period is small and statistically insignificant: -0.026 estimate with a 0.127 standard error. This suggests that districts respond fairly quickly to neighboring districts' changes in revenues per pupil.

Next, these data provide tests for in-kind spillovers of specific categorical expenditures. Table 5 displays estimates of fiscal spillovers based on estimation of equation 2 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.

The estimates in Table 5 produce two notable findings: (i) spillovers appear to occur specifically for instructional expenditures and not for other types of expenditures (i.e., administration, student support services), and (ii) unlike overall expenditures, spillovers for instructional salaries may take more than three years to be fully realized. The result in column 1 might not be surprising to people who regularly read articles and letters to the editor in their town's local newspaper: districts respond quickly to changes in neighboring districts' class sizes. Districts hire an additional teacher for roughly every four additional teachers hired by the neighboring districts (p=.07). Including the lagged response raises this estimate to more than one additional teacher for every three additional teachers hired by the average neighboring district (p=.06). Districts also respond forcefully to changes in their neighbors' per pupil expenditures on instructional salaries—expenditures reflecting the number of employed teachers, teacher's aides, etc., as well as the size of their salaries. Instructional salary spillovers occur more gradually than class size spillovers, with a 0.15 response within the first three years and a 0.32 additional response within the next three years. In other words, districts increase spending on salaries by 47 cents for every one dollar increase in their average neighboring districts' salaries (p=.02). Teacher salary negotiations often occur only every few years, which might explain why districts respond more quickly to neighbors' changes in class sizes than their changes in salaries.

The remaining three columns of Table 5 reveal statistically insignificant results for estimates of administrative costs, student support services, and expenditures for capital construction and renovations of facilities. 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. Districts may be more concerned with yardstick comparisons of class sizes and teacher salaries. The lack of a statistically significant estimate for student support services salaries is not very surprising, given that districts may be less in tune with neighboring districts' employment of nurses, school counselors, etc.

Spillovers for capital outlays are estimated less precisely because these outlays are relatively infrequent. Nonetheless, the estimates suggest that there are not very large spillovers for capital outlays. This may help to explain a recent finding that some districts induce large increases in local house values by making investments in school infrastructure (Cellini et al., 2010). If districts quickly copied their neighbors' infrastructure investments, then these types of investments should not produce such large, immediate housing premiums.

7. Conclusion

School districts’ expenditures respond to changes in nearby districts’ expenditures. Fiscal spillovers between districts are in the neighborhood of 25 cents per a $1 change in *mean* neighbors’ expenditures, where the relevant set of neighbors is districts located within a thirty mile radius. The most conservative instrumental variables estimate is the 14 cent response from the cross-border spatial lag model that: (i) includes a sample of both metropolitan and non-metropolitan districts, (ii) controls for border-by-year fixed effects, and (iii) controls for neighboring districts' initial demographics. This conservative estimate 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 30 cents when identification is based only on metropolitan-area districts' neighbors with high rates of inter-state residential mobility. The main estimates of local fiscal spillovers suggest economically important effects, but smaller effects than one would estimate using other methodologies that do not adequately address issues such as spatially correlated omitted variables, spatially correlated measurement error, and common responses to changes in fiscal federalism (i.e., common vertical spillovers rather than horizontal spillovers).

The results also suggest that yardstick competition due to political agency problems—the focus of much of the prior empirical literature on fiscal spillovers—may not be essential for spillovers between local governments. Spillovers between in-state neighboring districts are actually slightly greater when districts use direct democracy rather than representative democracy, though this difference is not statistically significant. Note 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, 2008).

Further analyses confirm that fiscal spillovers are due to changes in districts' *locally-funded* tax revenues per pupil. This local budget response occurs rather quickly—within three years. Districts may take slightly longer than this to re-balance categorical expenditures in response to neighboring districts' recent changes in categorical expenditures. For example, districts immediately respond to changes in neighboring districts' average class sizes but take longer to respond to changes in neighboring districts' teachers' salaries.

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 similar spillovers occur in an environment, like the current one, in which many districts' expenditures are declining. Another 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 school districts 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 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.

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Figure 1

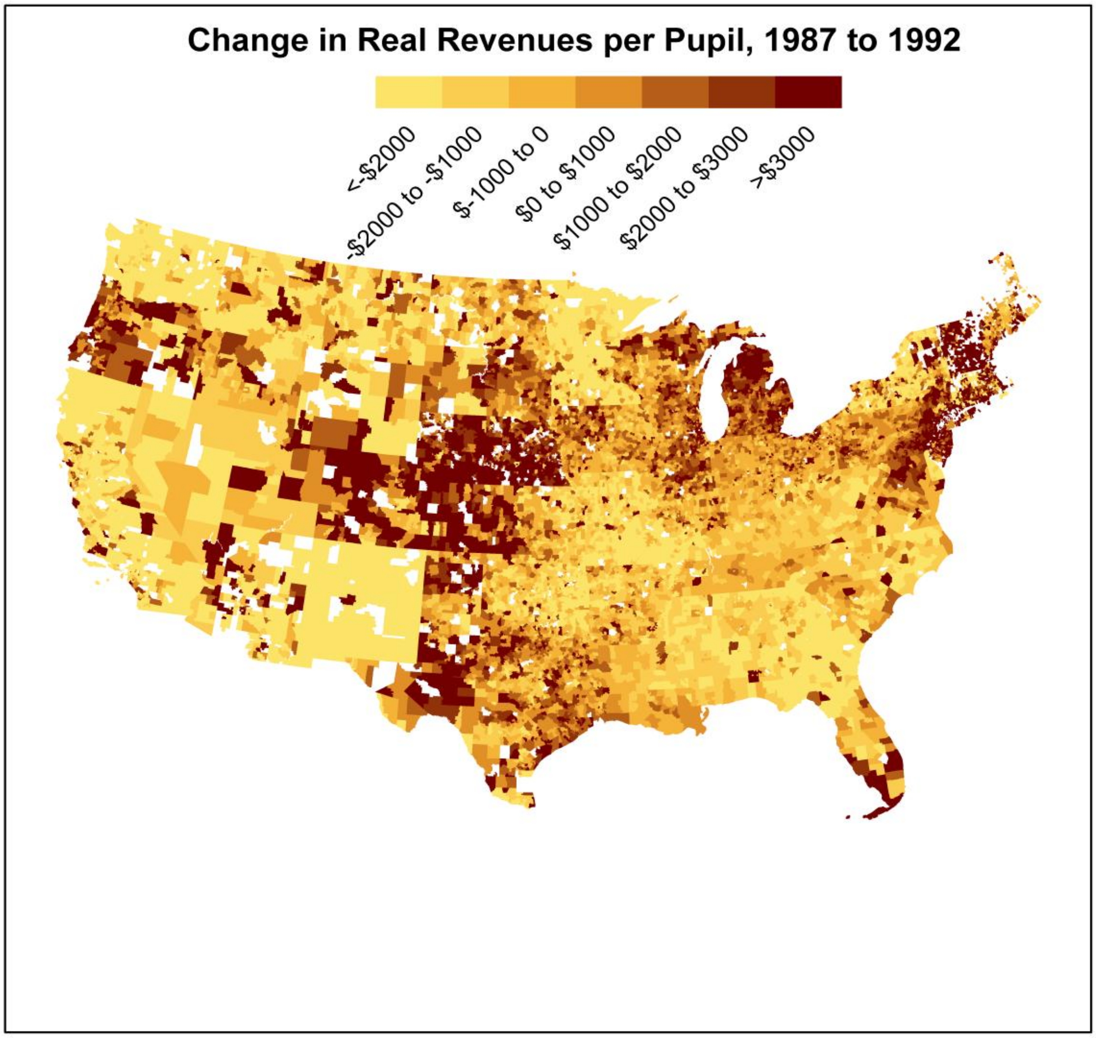
Change in Real Revenues per Pupil, 1987 to 1992
<-$2000
-$2000 to -$1000
$-1000 to 0
$0 to $1000
$1000 to $2000
$2000 to $3000
>$3000

Table 1: Changes in School District Operating Expenditures per Pupil, 1972-2007

Time Period	Mean (Year 2000 $)	Standard Deviation	Correlation with Change in Operating Expenditures per Pupil Among…		
			All neighbors	In-state neighbors	Out-of-state neighbors
1972-77	-1,869	1,760	.533	.542	.404
1977-82	2,624	1,866	.544	.545	.417
1982-87	1,526	3,202	.655	.675	.293
1987-92	225	3,325	.741	.753	.461
1992-97	563	1,493	.344	.360	.167
1997-2002	1,372	1,598	.275	.288	.117
2002-2007	614	1,696	.341	.352	.231

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

	OLS	Instrumental Variables Model Identified Using…	
		Traditional spatial lag model	Cross-border spatial lag model
Definition of Neighbors			
(1) Districts within a 30 Mile Radius (*N=72,767)*	.379 (.008)	.281 (.013)	.249 (.037)
(2) Districts within a 30 Mile Radius and with Similar Median Household Income (*N=71,501)*	.298 (.007)	.253 (.014)	.191 (.035)
(3) Contiguous School Districts (*N=72,299)*	.333 (.006)	.273 (.012)	.113 (.041)

Notes to Table 2: 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 2007. The sample sizes vary slightly across the rows, because observations are only included if they have non-missing predicted changes in expenditures 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 and for recent district reorganizations. The sample excludes districts which lack much local discretion over operating expenditures per pupil.

Table 3: Robustness Checks for Fiscal Spillovers between Districts

	(1)	(2)	(3)	(4)
Baseline controls	.249 (.037)	.188 (.052)	.258 (.068)	.300 (.078)
Adding controls for neighboring districts' demographics	.247 (.040)	.143 (.056)	.354 (.088)	.402 (.096)
Districts' Location	All	All	MSA-only	MSA-only
Controlling for Border-by-year Fixed Effects?	NO	YES	NO	NO
Using Only Neighbors with High Rates of Inter-state Residential Mobility?	NO	NO	NO	YES
Sample Size	72,767	72,767	34,511	34,250

Notes to Table 3: Each cell in the first two rows above reports an estimate of fiscal spillovers using a model similar to the one used in the last column of row 1 of Table 2—i.e., with a 30-mile radius between districts' centroid coordinates as a prerequisite for districts to be defined as neighbors. (The estimate in row 1 of column 1 above is identical to this estimate from 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. The MSA-only sample is limited to districts located in metropolitan statistical areas, based on classifications from 2000. The sample size in column 4 is smaller than in column 3 due to missing data concerning inter-state residential mobility for some school districts.

Table 4: Heterogeneous Fiscal Spillovers

	Estimated Spillovers	*p-value* of Difference
***(i)** Initial Median Household Income Compared to Neighboring Districts' Initial Median Incomes*		
>= Average Income Among Neighbors	.274	
	(.046)	*.239*
< Average Income Among Neighbors	.228	
	(.043)	
(ii) *Initial Expenditures per Pupil Compare to Neighboring Districts' Initial Expenditures*		
>= Avg. Neighbors' Expenditures per Pupil	.537	
	(.043)	*.001*
< Avg. Neighbors' Expenditures per Pupil	.207	
	(.042)	
***(iii)** Form of Local Democracy Determining Local School Revenue Levels*		
Direct Only	.283	
	(.057)	*.366*
Representative	.214	
	(.055)	
***(iv)** Official State-level School Finance Reforms*		
Recent Legislative or Court-ordered Reform	.274	
(current 5-year period or prior 5-year period)	(.047)	*.279*
No Recent Reform	.176	
	(.077)	

Notes to Table 4: Neighboring districts are defined as those located within a 30 mile radius. Panels (i) through (iv) each lists two estimated coefficients from a single regression model. These models control for the independent variables described in the notes to Table 2, as well as controls for demographics in neighboring districts. The models in panels (i) through (iii) also include indicator variables equal to one if the district falls in the first listed category in each panel above; that type of indicator would be collinear with the fixed effect controls in panel (iv). The indicator variables' coefficients in panels (i) through (iii) respectively equal -28 (standard error of 20), -1,030 (s.e. of 12), and -71 (s.e. of 95). This coefficient for the indicator for whether districts previously outspent their neighboring districts is large, negative, and statistically significant, suggesting that there is substantial mean reversion in terms of district expenditures within local regions. While districts with relatively high expenditures per pupil have a smaller intercept due to this mean reversion, the slope associated with responses to changes in neighbors' expenditures is much larger for these relatively high spending districts and the average estimated slopes in panel (ii) are relatively high compared to the corresponding estimate of 0.247 in the second row of the first column of Table 3. These findings suggest that estimates of fiscal spillovers may increase in models that control for reversion to the regional mean.

Table 5: Estimates of Categorical Spillovers,
3-Year Changes for Metropolitan-area Districts from 1992 to 2007

	Three Year Change in the District's Per Pupil…				
	(1)	(2)	(3)	(4)	(5)
	Teachers	Instructional Salaries	School-level Administrative Costs	Student Support Service Salaries	Capital Construction/ Improvement Outlays
Avg. Neighboring Districts' Change During					
Last 3 Years	.23	.15	-.09	.13	-.38
	(.12)	(.13)	(.14)	(.13)	(.28)
Prior 3 Year Period (6 to 3 years earlier)	.14	.32	.10	.03	.39
	(.13)	(.15)	(.16)	(.14)	(.32)
p-value, cumulative effect	*.06*	*.02*	*.93*	*.41*	*.98*
R-squared	.34	.33	.26	.45	.08
Number of Obs.	21,921	24,318	23,745	23,551	24,320

Notes to Table 5: Each column displays the result of a single regression, the cross-border spatial lag model specified in Equation 2 using one year time intervals (i.e., with k=1). The control variables include demographic data from the 1990 and 2000 Censuses. The sample is limited to districts located in metropolitan areas and districts with some form of local discretion over the size of local education revenues. The sample size moderately varies across columns because teacher-pupil ratios are unavailable for some districts and because suspiciously large changes are removed from the sample. In particular, the models drop observations with three-year changes that, in absolute value, are greater than ten students per teacher (column 1) or greater than $5,000 per pupil for categorical expenditures (columns 2 through 4).

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

	Full Sample	Districts in sample with at least one out-of-state neighbor within 30 miles[a]
Number of Districts	72,767	28,476
Operating expenditures per pupil (year 2000 $) (at the beginning of each 5 year period)	6,269	6,585
	3,121	*3,479*
5-year change in operating expenditures per pupil	727	784
	2,480	*269*
<u>Values based on the 1980 Census</u>[b]		
Median Household Income (year 2000 $)	34,877	37,548
	11,230	*12,870*
Mean House Value (year 2000 $)	92,701	102,274
	42,600	*49,621*
Proportion of the Population Ages 5-17	0.22	0.22
	0.03	*0.03*
Proportion of the Population Ages 65 & over	0.13	0.12
	0.05	*0.04*
Population Density (people per km^2)	228	378
	707	*968*

[a] In this paper's main analyses, neighboring districts are defined as districts with centroid coordinates located within thirty miles of each other.
[b] Census variables are limited to 1980 Census data for year 1987 observations 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, 1990, or 2000).