Strategic competition amongst public schools

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Abstract

Education reforms involving expanded school choice are increasingly viewed as a panacea for the US public education system. However, competition is present even absent such reforms through endogenous household residential choice. Utilizing panel data from Illinois from 1990–2000, we assess whether public school districts compete with neighboring public school districts. Specifically, we test for strategic behavior in the choice of educational inputs. The results confirm the strategic behavior of public schools. However, such strategic behavior only occurs during periods of binding financial constraints generated by ‘tax caps’ on aggregate local property tax revenues.

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1. Introduction

Improved student achievement is an important policy objective. Not only do educational investments affect a myriad of individual behaviors over the life cycle, but differences in achievement also explain significant variation in aggregate outcomes such as economic growth (Hanushek and Kimko, 2000; Hoxby, 2002). Despite its importance, however, student achievement in the US has stagnated over the past few decades (e.g., Hoxby, 1996). To reverse this trend, many policy proposals – vouchers and charter schools being the primary examples – have come to the forefront based on the notion that expanded school choice will improve the...
efficacy of public schools through heightened competition (e.g., Hoxby, 2000). Zanzig (1997, p. 431) states:

“With the rise to prominence of proposals like school choice and voucher programs, this neglect of market structure may be coming to an end. At the core of these proposals is the principle that increased school competition can make school systems more efficient, resulting in correspondingly stronger performance by students.”

To test such claims, researchers have mainly concentrated on assessing the empirical impact of access to private and charter schools on public school performance. However, as noted in Hoxby (2000), Barrow and Rouse (2004), and elsewhere, even absent private and charter schools, the US public educational system would not be devoid of competition; competition arises through the residential location decisions of households (Tiebout, 1956). Hoxby (2002, p. 17) notes that “this traditional form of [school] choice is by far the most pervasive and important form of choice in American elementary and secondary schooling today.” As a result, empirical investigations into the effect of competition arising from private and charter schools must (and do) recognize that some competition pre-dates the development of such schools. Thus, analyses of these sources of competition may understate the effect of competition on public school quality as such studies will only capture the impact of the marginal increase in competition. Barrow (2002, p. 156) writes:

“...[H]ouseholds with children appear to exercise school choice through location choice. One implication of these findings is public schools already face some competition from other public schools in the area. Thus, the promised benefits of competition from moving to complete choice with a full voucher program may be diminished.”

Consequently, an alternative, complementary approach to assess the impact of competition is to focus on competition arising from other public schools. Understanding the strength of the competitive forces emanating from alternative public schools districts may also shed light on the value added from additional competition that may be induced through expanded school choice.

This line of inquiry is not new, although empirical examinations are relatively sparse. Hoxby (2000) examines the impact of competition (measured by number of school districts within a metropolitan area) on student achievement, per student expenditures, and several other outcomes, finding positive effects on achievement and negative effects on per student expenditures and pupil–teacher ratios when competition is treated as endogenous. Similarly, Borland and Howsen (1992), Zanzig (1997), and Marlow (2000) find positive effects of competition (measured using either a Herfindahl index or number of neighboring school districts) on achievement, although competition is treated as exogenous. Moreover, Zanzig (1997) finds that greater competition is irrelevant after a certain competitive threshold (approximately three to four districts per county) is

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1 Empirical studies examining public versus private school competition include, among others. Hoxby (1994, 2002), Newmark (1995), Sander (1999a), and Brasington (2000) and others study the empirical effects of charter school competition. For theoretical analyses, see Epple and Romano (1998), who analyze the impact of private schools on public school quality, and Cardon (2003), who analyzes the effect of charter schools on public school quality.

2 Hoxby (2002) also notes that empirical investigations of competition arising from other public schools are beneficial in that the medium- and long-run impacts of competition cannot be detected from choice enhancing reforms only recently enacted.

3 Similarly, Hoxby (2000, p 1210) notes: “[R]eforms are layered on top of the existing system. Most reforms would extend, not, introduce, choice. If one ignores Tiebout choice, one neglects the fact that some of the predicted effects of reforms are attained by Tiebout choice already.” (emphasis in original)
attained, and Marlow (2000) reports a negative impact of competition on per pupil spending. Recently, Hanushek and Rivkin (2003) use metropolitan data from Texas, finding that public school competition (measured using a Herfindahl index) has positive effects on teacher quality. Brasington (2000) and Barrow and Rouse (2004) offer different approaches. Brasington (2000) estimates a supply function for public school quality—conditioning on the implicit price of public school quality—in Ohio metropolitan areas. The author finds no impact of public school competition (as measured by the number of school districts within the metropolitan area and several other indices). Barrow and Rouse (2004) estimate the impact of increased school spending per pupil obtained through state aid on aggregate house values per pupil, arguing that a positive (negative) effect indicates that school districts are underspending (overspending). Incorporating public school competition (measured using a Herfindahl index) into their model, the authors find that districts facing greater (less) competition tend to underspend (overspend).

Finally, in the study most similar to ours, Blair and Staley (1995) estimate a mixed regressive, spatial autoregressive model (or, spatial lag model), regressing achievement in a given school district on the average achievement of bordering school districts. The authors find a positive association, but the treatment of achievement in neighboring districts as exogenous ignores the simultaneity that arises in models of spatial reaction functions (Anselin, 2002).

In this study, we build on the work in Blair and Staley (1995), as well as the rich literature examining strategic interaction of public policies in general. The intuition underlying these strategic interaction models is quite straightforward: since local ‘economies’ are spatially linked (in the present case, through the Tiebout process), under certain realistic assumptions decision-makers may interact strategically when setting policies. Although the various theoretical models and the accompanying empirical literature are diverse, the resulting empirical specifications are quite similar: estimate spatial reaction functions and test whether the slopes are significantly different from zero. A finding of a non-zero slope is regarded as evidence that strategic interactions exist.

Using panel data from the state of Illinois over the period 1990–2000, we apply this type of analysis to the provision of public school quality. Specifically, we examine if the decisions concerning school inputs are spatially linked across school districts within the same county. Moreover, we assess the relationship between this spatial linkage and financial constraints confronted by public school administrators. Beginning in 1991 and expanding since, certain public schools in Illinois are restricted in their ability to raise revenues through ‘tax caps’ on aggregate local property taxes. The caps—implemented under the Property Tax Extension Limitation Law (PTELL)—place a ceiling on the increase in total property tax revenue that a district can collect above the revenue collected in the previous year. Thus, Illinois provides a unique venue in which not only to assess strategic competition within public school districts, but also the impact of school financing constraints on such strategic behavior. Our specifications are estimated via instrumental variables (by Generalized Method of Moments), thereby eliminating the possibility of confounding strategic competition with spatial error correlation.

The results are striking. We find support for the notion that public school districts in Illinois engage in strategic competition with neighboring school districts. Specifically, we find that the decision calculus employed by district administrators concerning pupil–teacher ratio, teacher salary, current and capital expenditures per pupil, and school size directly incorporates information on the levels of these inputs in neighboring districts. Moreover, the strategic response

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4 For an excellent survey, see Brueckner (2003), who divides the literature into two sub-groups: (i) spillover models, which include yardstick competition models, and (ii) resource flow models.
to changes in neighboring inputs appears to operate within a two-year window. However, the
degree of strategic behavior depends crucially on the presence of the ‘tax caps.’ Interestingly,
while public school districts in counties with ‘tax caps’ are highly responsive to the policy choices
of neighboring districts, public school districts in counties without ‘tax caps’ are unresponsive to
policy changes in neighboring districts. Furthermore, there is no evidence that counties engaged
in a high degree of strategic competition are more likely to enact such revenue constraints; the
spatial linkages that exist in counties with ‘tax caps’ did not exist prior to the imposition of the
caps. Interpreting the presence of ‘tax caps’ as a constraint on the financial flexibility of districts,
the results suggest that strategic competition, originating most notably from Tiebout choice, is an
attribute entrenched in the status quo public school system even absent policies such as vouchers
and charter schools, but only in periods of tight fiscal budgets; absent legislative limits on (the
primary source of district) revenues, districts make policy decisions without regard to the
decisions of nearby districts. While perhaps surprising initially, the results support the hypothesis
that school administrators become particularly wary of lagging behind neighboring districts when
facing ‘tax caps,’ given the difficulty that administrators would confront in closing the gap in the
future, leading administrators to pay closer attention to the actions of neighboring districts.

The remainder of the paper is organized as follows. Section 2 motivates the empirical
specification via a theoretical model of interjurisdictional competition for students. Sections 3 and
4 present the empirical models and the data. Section 5 discusses the results. Section 6 offers some
concluding remarks.

2. Theoretical model

To motivate the empirical specification, we present a simple model of strategic interaction
between public school districts. The model builds on the resource competition models discussed in
Brueckner (2003), and is consonant with the particular features of school finance in Illinois (from
where the data come). The relevant unit of observation is the district, rather than the school, as
schools do not have financial autonomy in the US; revenues are (primarily) raised and allocated at
the district level (Hoxby, 2000, 2002; Hanushek and Rivkin, 2003). Let \( n_j \) denote the number of
school districts (indexed by \( i, i=1, \ldots, n_j \)) in educational market \( j \) (\( j=1, \ldots, m \)). The objective
function of the administrative unit of district \( i \) in educational market \( j \) is given by

\[
U(\Pi_{ij}, Q_{ij}; X_{ij})
\]  

(1)

where \( \Pi_{ij} \) denotes profits, \( Q_{ij} \) is the level of educational quality provided, and \( X_{ij} \) is a vector of
district attributes. Utility is increasing and concave in \( \Pi \) and \( Q \). We make no assumptions about the
cross-partials. The form of the objective function in Eq. (1) follows from Borland and Howsen

Profits equal the difference between revenue, \( R \), and costs, \( C \). Public school district revenues in
Illinois are obtained from three sources: local property taxes, state aid, and federal aid.\(^5\) Federal aid
primarily supports students from low-income households or targets special programs or
populations. Such aid for Illinois is granted to the Illinois State Board of Education (ISBE) and
then distributed to local school districts. The two largest federal funding sources are for special
education and the US Department of Agriculture’s school food program. Other significant federal
funding is provided for the Improving America’s Schools Act, Title 1 (IASA) program and

\(^{5}\) For further details, see the Illinois State Board of Education (http://www.isbe.net/sfms/slf01/slf.pdf).
vocational education. A portion of state aid is allotted through the Illinois General State Aid (GSA) formula, which grants aid to districts on the basis of the number of students and the wealth of the district (as measured by property values). Additional state aid is given in the form of categorical aid and special grants. Local property taxes come from two sources: residential taxes and business property taxes. Business property taxes are collected under the Corporate Personal Property Replacement Tax (CPPRT), which imposes a state-collected tax on the net income of businesses (corporations, partnerships, and trusts) and on invested capital of public utilities. The majority of the CPPRT revenues are then allocated to local school districts. Local residential property taxes are controlled at the local level. However, some local districts in Illinois are restricted by ‘tax caps’ implemented under PTELL. PTELL is implemented on a county-by-county basis (and thus is reflected in $X_j$) and caps aggregate property tax increases at 5% or the rate of inflation, whichever is lower. PTELL was first implemented in five counties in 1991; 34 counties had ‘tax caps’ by 2000. As a result, revenues, $R$, depend on the number of students, $s_j$, attending schools in the district and district attributes, $X_j$. Costs are increasing in both educational quality and the number of students served (Hoxby, 1996; Cardon, 2003). Thus, profits may be expressed as

$$\Pi_j = R_j - C_j = R(s_j, X_j) - C(Q_{ij}, s_j)$$

(2)

As in Hoxby (1996) and Cardon (2003), assume that excess revenues are utilized by the administrative unit to further its own personal welfare, while having no impact on educational quality. Alternatively, one could define $\Pi_j$ as ‘rewards’ earned by administrators for the provision of educational quality (i.e., $\Pi_j = II(Q_{ij})$, $\partial II/\partial Q > 0$), as discussed in Hoxby (1996, 2000). In either case, since quality is unobserved, we must specify a production function. Assume $Q$ depends on a vector of inputs, $Z_{ijk}$, $k = 1,\ldots, K$,

$$Q_{ij} = Q(Z_{i1}, \ldots, Z_{iK})$$

(3)

Finally, in the spirit of Tiebout choice, assume that the number of students served by a school district is a function of its own educational quality and the educational quality in other districts within the same educational market. Thus, the size of the student body is given by

$$s_{ij} = s(Q_{ij}, Q_{-ij})$$

(4)

where $Q_{-ij}$ is the educational quality of other districts in educational market $j$ besides district $i$.

To derive the reduced form of the model, Eqs. (2)–(4) are substituted into Eq. (1), yielding

$$U(R(s(Q_{ij}, Q_{-ij}), X_j) - C(Q_{ij}, s(Q_{ij}, Q_{-ij}))Q_{ij}, X_j) = \tilde{V}(Q_{ij}, Q_{-ij}; X_j)$$

(5)

given that $Q_{-ij} = Q(Z_{-ij1}, \ldots, Z_{-ijk})$. The administrative unit’s objective function is to choose a vector of inputs, $Z_{ijk}$, $k = 1,\ldots, K$, to maximize Eq. (5) by equating $\partial V/\partial Z_{ijk} = 0$ for all $k$. This system of first-order conditions implicitly defines the equilibrium value of each input, $Z_{ijk}$, as a

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6 Public Act 89-718 permits a country to bring PTELL to a referendum in its country. In addition, Public Act 89-718 allows the country board of a country that previously implemented PTELL to repeal the law by the same referendum process. See [http://www.revenue.state.il.us/LocalGovernment/PropertyTax/pio62.htm](http://www.revenue.state.il.us/LocalGovernment/PropertyTax/pio62.htm).

7 For example, profits may be used to increase the salaries of administrators, to decorate offices, hire additional staff to reduce work effort, etc.
function of all input choices made in other districts within the same educational market, $Z_{-ijl}$, $l=1,\ldots,K$. Applying the implicit function theorem to the first-order conditions, treating $Z_{ijk}, k=1,\ldots,K$, as parameters as in Fredriksson et al. (2004), yields the following set of spatial reaction functions:

$$\frac{\partial Z_{ijk}}{\partial Z_{-ijl}}, \quad \forall k, l = 1,\ldots,K$$

which depicts the best response of school district $i$ in educational market $j$ in terms of input $k$ to all input choices made by alternative districts within the same educational market. With the exception of knife-edge cases and when no competition for students occurs, the vector of reaction functions can take either sign.

The goal of the empirical analysis is to test if the reaction functions in Eq. (6) have slopes significantly different from zero. Non-zero slopes indicate the presence of strategic competition amongst public school districts. Finally, as the positions of the reaction functions depend on the attributes of each school district, $X_{ij}$, it is necessary to control for such characteristics in the empirical specifications (Brueckner, 2003). Nonetheless, our focus is on the slopes of the reaction functions.

Prior to continuing, it is important to highlight two salient aspects of the theoretical model. First, Eq. (4) holding in practice (i.e., that the residential location choices of households with students is related to the educational quality of all districts) is neither necessary nor sufficient to generate non-zero slopes of the reaction functions in Eq. (6). Instead, what is relevant is the perception among the administrative units of schools districts. If administrators believe that households will exercise Tiebout choice – even if that belief is incorrect (e.g., due to liquidity constraints faced by poorer households) – then (according to the model) districts will incorporate the educational input decisions of alternative districts within the same educational market into their decision calculus, yielding non-zero slopes of the reaction functions in (6). On the other hand, if households do exercise Tiebout choice (or ‘threaten’ to), but administrators naively assume that households will not relocate to another school district, then the reaction functions in Eq. (6) will have zero slopes despite equation Eq. (4) holding in practice. This implies that the empirical results below must be interpreted carefully. A finding of non-zero slopes of the reaction functions is consonant with competition (real or perceived) altering the behavior of administrators. Consequently, educational reforms that accentuate the perceived level of competition (e.g., charter schools, private school vouchers, greater district choice) may induce further modifications in the behavior of administrators. Conversely, if we fail to find evidence of non-zero slopes of the reaction functions, one should not necessarily conclude that competition does not affect the behavior of public school administrators; rather, it may signify only that administrators mis-perceive the level of competition in the status quo.

Second, the system of reaction functions in Eq. (6) indicates that districts may not necessarily respond to a change in a specific input by (only) altering its level of that input (own-policy

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8 While non-zero slopes indicate the presence of strategic competition, they do not necessarily affirm the validity of the theoretical model posited herein. Specifically, since school board members in Illinois (where the data come from) are elected, a yardstick competition model, whereby voters judge the success of board members through comparisons with neighboring public school districts, would yield the same set of reaction functions (Brueckner, 2003). Thus, one typically cannot distinguish between spillovers arising from the effects of competition over students versus yardstick competition, unless one explicitly tests the Tiebout assumption in Eq. (4). Nonetheless, since both sources of competition provide incentives for administrators to provide quality schools, distinguishing between these two sources is not salient for the current analysis. We shall return to this point later.
effects); they may (also) alter other inputs (cross-policy effects). This result contrasts with the majority of previous analyses of spillovers across jurisdictions. The lone exception (of which we are aware) is Fredriksson et al. (2004), who analyze interactions across US states in the choice of tax effort, environmental regulatory stringency, and infrastructure spending in order to attract mobile capital.

3. Empirical methodology

To test for horizontal interactions, we analyze the spatial patterns of five district-level inputs: pupil–teacher ratio, average teacher salary, current per student expenditure (PSE), capital PSE, and school size (defined as average enrollment per school). Each input potentially impacts the quality of education, and reflects (at least partially) policy choices undertaken by districts.9 Pupil–teacher ratio, and more specifically class size, garners the most amount of attention by administrators, policymakers, and parents, despite the fact that the efficacy of class reduction policies is questionable (Hanushek, 2003; Maasoumi et al., 2005). Higher teacher salaries, as a means of improving teacher quality, general expenditures per pupil, and capital expenditure per pupil are also commonly hypothesized to influence educational quality (Figlio, 1997; Hanushek et al., 1999; Sander, 1999b; Unnever et al., 2000). Finally, some evidence links school size to educational quality (Bradley and Taylor, 1998).

Our multi-dimensional approach entails the following log-linear estimating equation for each educational quality input $k$, $k = 1, \ldots, K$:

$$ \ln(Z_{ijkt}) = \sum_{g=1}^{K} \delta_{kg} \left[ \sum_{h=1, h \neq i}^{n_j} \omega_{ijh} \ln(Z_{hjgt}) \right] + \bar{X}_{ijt} \beta_k + \epsilon_{ijkt}, \quad k = 1, \ldots, K \quad (7) $$

where $n_j$ is the number of districts in educational market $j$; $Z_{ijh}(hjgt)$ is the level of input $k(g)$ in district $i(h)$ in educational market $j$ at time $t$; $\omega_{ijh}$ is the weight assigned to district $h$ by district $i$ for input $k$ at time $t$; $\delta_{kg}$, $k, g = 1, \ldots, K$ is the vector of parameters of interest, as it represents the elasticity of the reaction functions; $\bar{X}_{ijt}$ is a vector of district characteristics with parameter vector $\beta_k$ (in logarithms where appropriate; discussed below); and, $\epsilon_{ijkt} = \alpha_{ijk} + \gamma_{kt} + \epsilon_{ijkt}$, where $\alpha_{ijk}$ and $\gamma_{kt}$ are district-by-input and year-by-input fixed effects, and $\epsilon_{ijkt}$ represents idiosyncratic shocks potentially correlated across time and space within educational markets, but uncorrelated across markets. Implicit in the specification in Eq. (7) is the restriction that districts assign non-zero weight only to other districts within the same educational market, indexed by $j$.

As stated previously, most of the literature on strategic policymaking assumes a uni-dimensional framework (i.e., changes in policy $k$ in neighboring jurisdictions yield changes only to a jurisdiction’s own policy $k$). This implies restricting $\delta_{kg} = 0$ for all $k \neq g$. Although we estimate both the uni- and multi-dimensional models, we present and discuss only the uni-dimensional results as the restrictions are not rejected in the majority of cases; multi-dimensional results are available at http://faculty.smu.edu/millimet/pdf/tables_multi.pdf.

Before proceeding, several estimation issues merit discussion. First, in constructing the weights, $\omega_{ijh}$, we assign equal weight to all other districts in the same educational market; zero weight is assigned to all districts in other markets. Therefore, the weighting scheme does not vary

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9 Note that while the exact impact of many supposed inputs into educational quality is still an open question (eg. Hanushek, 1986, 2003), the issue in the current context is whether administrators and, more importantly households view the inputs we analyzes as productive. Brasington (1999) finds that the housing prices are decreasing in the pupil–teacher ratio and increasing in per student expenditures.
by input or by year (if the number of districts per county is constant). Thus, the weight matrix, $W$, with representative element $w_{ijhkt}$, is a row-standardized, block diagonal matrix. Second, we must define an educational market. Hoxby (2000) argues that districts are in the same educational market – for the purpose of defining competition – if they are within commuting distance to the same center of employment. Thus, Hoxby (2000) defines educational markets at the metropolitan area level (see also Brasington (2000)). In similar spirit and following Borland and Howsen (1992), Zanzig (1997), Barrow and Rouse (2004), and others, we define education markets at the county level. Consequently, the relevant right-hand side variables in Eq. (7) are the average values of inputs chosen by all other districts in the same county (and year) as the dependent observation.\footnote{The definition of an educational market as a country implies that a district located on a country border is restricted to only be concerned with other districts within the country and not those in the neighboring countries. If spillovers do cross country borders (which presumably they do), then the measures of neighboring input choices on the righthand side of the estimating equation will be measured with error. The corresponding estimates will then represent lower bounds.}

Third, the system of equations defined in Eq. (7) is estimated one equation at a time. In the multi-dimensional model, the control variables are identical across equations. As a result, there is no efficiency gain from joint estimation of the system. Moreover, in both the uni- and multi-dimensional models there is a potential cost to joint estimation in that mis-specification of one equation may have adverse consequences on the estimates for other (well-specified) equations in the system.

The fourth estimation issue relates to the endogeneity of neighboring input choices (e.g., Anselin, 2002). According to the theoretical model, districts choose their inputs simultaneously, giving rise to concerns about the direction of causation. Furthermore, unobserved shocks that are correlated with the input decisions of several jurisdictions (e.g., spatial error autocorrelation arising from the presence of private schools, collusion amongst a subset of districts, common shocks related to vertical changes in state funding) may cause one to falsely conclude that strategic decision-making is occurring. To circumvent these potential problems, we undertake three measures. First, we include district-by-input and year-by-input fixed effects. Thus, unobserved, time invariant district-, county-, or state-level factors (e.g., the relative strength of parent–teacher associations or the existence of private schools over the entire sample period), as well as annual, state-level shocks (e.g., the educational tax credit passed in Illinois in 1999) that affect the choice of inputs of all districts are removed.\footnote{Illinois statute (Chapter 35, Article 2, Sec.201 (m)) established a tax credit equal to 25% of qualified educational expenses up to $500 per family per year in 1999 (see http://www.legis.state.il.us/legislation/iles/ch35/ch35act5articles/ch35act5).}

However, fixed effects may be insufficient to circumvent the endogeneity issue (e.g., annual variation in state-level funding, which constitutes 30%–40% of district revenues, may not be uniform across districts). Consequently, our second step is to instrument for neighboring input choices using a General Methods of Moments (GMM) approach. While other techniques are available (e.g., Brueckner and Saavedra, 2001), instrumental variables (IV) estimation remains consistent in the presence of spatially correlated error terms (Kelejian and Prucha, 1998; Brueckner, 2003), and offers the advantage of computational ease given the sample size and multi-dimensional framework. Thus, in line with previous studies (e.g., Figlio et al., 1999; Fredriksson and Millimet, 2002a, 2002b; Fredriksson et al., 2004), we use a subset of the variables in $X_{ijt}$ for neighboring districts as instruments, employing the same weighting scheme for the instruments as we do for neighboring inputs. In addition, we report standard errors that are robust to arbitrary heteroskedasticity and correlation (clustering) within counties (Baum et al., 2003).

Several diagnostic tests are conducted to assess the reliability and efficiency of the GMM estimates. First, we report Hansen’s $J$ statistic, an overidentification test for the validity of the
instruments. Second, we conduct the Pagan and Hall (1983) test for heteroskedasticity as GMM is more efficient in the presence of heteroskedasticity of unknown form (Baum et al., 2003). Third, since IV estimates based on weak instruments are biased toward the OLS estimates (e.g., Bound et al., 1995), we conduct several additional tests. First, we report $F$-tests for the joint significance of the instrument set. Second, we conduct the test proposed in Hall et al. (1996) for instrument relevance. The test examines whether the smallest sample canonical correlation between the instrument set and the endogenous variable(s) is significantly different from zero. Finally, we compute Staiger and Stock’s (1997) measure of the maximum squared bias of the IV estimates relative to the OLS estimates ($B_{\text{max}}$ in their notation; Eq. (3.6), p. 566).

Our final approach to circumventing endogeneity replaces the contemporaneous value of neighboring policies with its lagged counterpart (e.g., Fredriksson and Millimet, 2002b; Fredriksson et al., 2004). This approach eliminates concerns related to reverse causation since current input choices should have no direct implications for past input decisions in neighboring districts. Moreover, this approach has the added benefit of flexibility in that it allows lags in strategic interaction, perhaps due to delays in the flow of information or the ability of administrators to react. We allow for two distinct lag processes, replacing neighboring input choices in Eq. (7) with their (i) two-year lagged values and (ii) five-year lagged values.

4. Data

The data come from two sources. The first is the Common Core of Data (CCD), available from the National Center of Education Statistics (NCES), and used previously in, for example, Hoxby (2000) (US Department of Education, 2001). The CCD is a “comprehensive, annual, national statistical database of all public elementary and secondary school districts.” The CCD provides rich (district-level) student and staffing information. It also contains administrative data on guidance counselors and instructional, library and support staff. Membership counts (ungraded and total pre-kindergarten (PK) through 12 grade students), pupil–teacher ratio, and other counts are also available. In addition, data on number of schools and total number of students allow us to construct the average school size for each district. Finally, the CCD database contains data on revenues and expenditures for each public school district. Capital outlay expenditures, current core expenditures, and teacher’s salaries are some of the expenditure items that are included. Combined with the counts of total students and total teachers in each school district, we are able to form the input measures examined.

The second database is the special school district tabulation of the Census of Population and Housing (School District Demographics Book) (US Department of Education, 1994, 2003). The data contains information compiled from the questions asked of a sample of individuals and housing units for Census 1990 and 2000. Population items include: total population, as well as...

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12 Testing the validity of the instruments is particularly important in the current context given the reflection problem raised in Manski (1993). Specially, Tiebout sorting may generate correlation between unobservable determinants of districts input choices and demographic characteristics of the population, thereby invalidating their use as instruments. As discussed below, however, this does not appear to be a concern.

13 This approach also eliminates the need to instrument for neighboring input choices, thereby circumventing the reflection problem (see footnote 12).

14 Alternative data sources, such as the Schools and Staffing Survey (SASS) and National Educational Longitudinal Survey (NELS) may contain additional measures of school district policy choices beyond those utilized herein. However, such data are inappropriate for the current analyses given that they contain only a sub-sample of all school districts, not the complete universe. Thus, complete data on the behavior of ‘neighboring’ districts is not available.
Table 1

<table>
<thead>
<tr>
<th>Variable</th>
<th>Own values</th>
<th>Neighboring values</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>Standard deviation</td>
</tr>
<tr>
<td>Pupil–teacher ratio</td>
<td>16.63</td>
<td>2.97</td>
</tr>
<tr>
<td>Current per student expenditure</td>
<td>3508.98</td>
<td>1154.60</td>
</tr>
<tr>
<td>Capital per student expenditure</td>
<td>411.04</td>
<td>731.28</td>
</tr>
<tr>
<td>School size</td>
<td>444.27</td>
<td>407.95</td>
</tr>
<tr>
<td>Average teacher salary</td>
<td>24,950.00</td>
<td>6755.41</td>
</tr>
<tr>
<td>Number of neighboring districts</td>
<td>35.96</td>
<td>49.39</td>
</tr>
<tr>
<td>Median household income</td>
<td>26,905.01</td>
<td>10,268.06</td>
</tr>
<tr>
<td>Unemployment rate</td>
<td>0.05</td>
<td>0.03</td>
</tr>
<tr>
<td>Occupied housing</td>
<td>0.94</td>
<td>0.08</td>
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<tr>
<td>Owner-occupied housing</td>
<td>0.72</td>
<td>0.10</td>
</tr>
<tr>
<td>Average rent</td>
<td>315.34</td>
<td>102.14</td>
</tr>
<tr>
<td>Population, aged 20+, no high diploma</td>
<td>18,726.66</td>
<td>99,696.80</td>
</tr>
<tr>
<td>Population, aged 20+, college degree</td>
<td>953.99</td>
<td>8,027.40</td>
</tr>
<tr>
<td>% Population, white</td>
<td>0.91</td>
<td>0.15</td>
</tr>
<tr>
<td>% Population, Rural</td>
<td>0.45</td>
<td>0.42</td>
</tr>
<tr>
<td>% Population, aged 4–19</td>
<td>0.23</td>
<td>0.06</td>
</tr>
<tr>
<td>% Population, aged 4–19, white</td>
<td>0.90</td>
<td>0.18</td>
</tr>
<tr>
<td>% Population, aged 4–19, in poverty</td>
<td>0.10</td>
<td>0.07</td>
</tr>
<tr>
<td>% Population, aged 4–19, enrolled in school</td>
<td>0.47</td>
<td>0.26</td>
</tr>
<tr>
<td>% Population, aged 4–19, speak a language other than English</td>
<td>0.29</td>
<td>0.11</td>
</tr>
<tr>
<td>% Households, receiving public assistance</td>
<td>0.02</td>
<td>0.02</td>
</tr>
<tr>
<td>% Households, with children &lt; 18</td>
<td>0.22</td>
<td>0.08</td>
</tr>
<tr>
<td>% Households, with children in poverty</td>
<td>0.06</td>
<td>0.05</td>
</tr>
</tbody>
</table>

Notes: Number of observations is 9438.
disaggregated by urban/rural status, gender, age, race, education, and school enrollment status; number of households disaggregated by number of children, poverty status, public assistance recipiency, and ability to speak English. Housing items include average rent, percent of occupied housing, percent of owner-occupied housing, and other measures. Lastly, the data contains information on median household income and the unemployment rate. Because the data are taken from the US Census, they are available only for the years 1990 and 2000. Consequently, we linearly interpolate the values of these demographic attributes for the years 1991–1999; however, we also present results utilizing only the data from 1990 and 2000.

Using these two sources, we assemble a data set on all public school districts in the state of Illinois. We restrict the sample along two lines. First, public school districts that comprise an entire county are excluded, since these districts face no within-county competition from other public school districts; these districts are ‘islands’ or ‘unconnected observations’ in the terminology of Anselin (2002). This eliminates 13 districts (counties). Second, we restrict attention to only those districts in existence over the entire sample period. Since the empirical specification includes district-level fixed effects, we require at least two observations per district in the final sample. In addition, since demographic attributes are only available through Census 1990 and 2000, we require districts to be in existence in both of these years. Excluding districts that do not exist in both years eliminates an additional 93 districts. Finally, we also exclude four districts that are in the data set in 1990 and 2000, but do not have CCD data available in at least 1 year in the interim. Thus, in the end, we utilize a balanced panel of 858 public school districts dispersed across 89 counties, yielding a total sample size of 9438.

The district-level controls included in X are population, median household income, average rent, percentage of the population aged 20 and over without a high school diploma and with at least a bachelor’s degree, unemployment rate, percentage of households with children in poverty, percentage of households receiving public assistance, percentage of housing units occupied, percentage of housing units owner-occupied, percentage of the population that is white, percentage of households with children less than 18 years old, percentage of the population that is aged 4–19, percentage of the population aged 4–19 that is white, percentage of the population aged 4–19 in poverty, percentage of the population aged 4–19 enrolled in school, percentage of the population aged 4–19 that speaks a language besides English, percentage of the population that is rural, and a dummy for PTELL. Summary statistics are provided in Table 1.

Prior to continuing, we performed preliminary tests – based on the global Moran statistic (Moran, 1948) – designed to detect spatial dependence within each educational input.15 Using the same weight matrix as defined above, the statistic is given by:

\[
I_k = \frac{N \sum \sum \sum \omega_{ijkh}(\Delta Z_{ijk} - \mu_k)(\Delta Z_{hk} - \mu_k)}{\sum \sum \sum (\Delta Z_{ijk} - \mu_k)^2}
\]  

(8)

where \(\Delta Z_{ijk}(hjk)\) is the (first-differenced) change in input \(k\) in school district \(i(h)\) in educational market \(j\), \(N\) is the total number of observations (equal to 8580), \(\omega_{ijkh}\) is the element in the spatial weights matrix corresponding to the observation pair \((i,h)\) in educational market \(j\) for

15 Then Moran I statistics were computed using SpaceStat, version 1.91 (Anselin, 2001).
Table 2
Strategic interaction over public school quality inputs: uni-dimensional framework (full sample, 1990–2000)

<table>
<thead>
<tr>
<th>Independent variable</th>
<th>School quality measure</th>
<th>School quality measure</th>
<th>School quality measure</th>
<th>School quality measure</th>
<th>School quality measure</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Pupil–teacher ratio</td>
<td>Current PSE</td>
<td>Capital PSE</td>
<td>Teacher salary</td>
<td>School size</td>
</tr>
<tr>
<td></td>
<td>All years</td>
<td>1990 and 2000 only</td>
<td>All years</td>
<td>1990 and 2000 only</td>
<td>All years</td>
</tr>
<tr>
<td>In (neighboring school quality)</td>
<td>0.62*</td>
<td>0.46†</td>
<td>0.61†</td>
<td>0.57†</td>
<td>0.21†</td>
</tr>
<tr>
<td>[p = 0.00]</td>
<td>[p = 0.00]</td>
<td>[p = 0.00]</td>
<td>[p = 0.00]</td>
<td>[p = 0.00]</td>
<td>[p = 0.00]</td>
</tr>
<tr>
<td></td>
<td>[p = 0.61]</td>
<td>[p = 0.06]</td>
<td>[p = 0.40]</td>
<td>[p = 0.35]</td>
<td>[p = 0.74]</td>
</tr>
<tr>
<td>Hansen’s J-Statistic (overidentification test)</td>
<td>F = 17.72</td>
<td>F = 15.50</td>
<td>F = 13.49</td>
<td>F = 4.50</td>
<td>F = 2.83</td>
</tr>
<tr>
<td></td>
<td>[p = 0.00]</td>
<td>[p = 0.00]</td>
<td>[p = 0.00]</td>
<td>[p = 0.00]</td>
<td>[p = 0.00]</td>
</tr>
<tr>
<td>Hall et al. (1996) test of instrument relevance</td>
<td>ρ = 0.47</td>
<td>ρ = 0.68</td>
<td>ρ = 0.59</td>
<td>ρ = 0.69</td>
<td>ρ = 0.15</td>
</tr>
<tr>
<td></td>
<td>[p = 0.00]</td>
<td>[p = 0.00]</td>
<td>[p = 0.00]</td>
<td>[p = 0.00]</td>
<td>[p = 0.00]</td>
</tr>
<tr>
<td>Staiger and Stock (1997) measure of maximum relative bias</td>
<td>B_{max} = 0.002</td>
<td>B_{max} = 0.01</td>
<td>B_{max} = 0.001</td>
<td>B_{max} = 0.01</td>
<td>B_{max} = 0.004</td>
</tr>
<tr>
<td>Observations</td>
<td>8572</td>
<td>850</td>
<td>8572</td>
<td>850</td>
<td>8475</td>
</tr>
</tbody>
</table>

Notes: Estimation is via GMM. Neighboring school quality is the average value of the dependent variable in all other school districts in the same county as the observation on the left-hand side. Single district counties are excluded. Dependent variables are in natural log form; thus, the coefficients represent elasticities. PSE = per student expenditure. Additional controls included in each specification: school district and year fixed effects, population, median household income, average rent, % of population aged 20+ without a high school diploma and with at least a bachelor’s degree, unemployment rate, % of households with children in poverty, % of households receiving public assistance, % of housing units occupied, % of housing units owner-occupied, % of population that is white, % of households with children less than 18, % of population that is aged 4–19, % of population aged 4–19 that is white, % of population aged 4–19 in poverty, % of population aged 4–19 enrolled in school, % of population aged 4–19 that speak a language besides English, the % of the population that is rural, and a dummy for PTELL. Instrument set utilized in GMM estimation includes the weighted average of neighboring values for: population, median household income, % of population aged 20+ without a high school diploma and with at least a bachelor’s degree, % of households with children in poverty, % of housing units occupied, % of housing units owner-occupied, % of population that is white, and % rural population. Standard errors in parentheses are robust to arbitrary heteroskedasticity and within-county correlation. † indicates significant at the 95% confidence level; ‡ indicates significant at the 90% confidence level.
input $k$ at time $t$, $\mu_k$ is the mean of $\Delta Z_k$, and $S_{0k}$ is a scaling constant equal to the sum of all weights:

$$S_{0k} = \sum_{l} \sum_{j} \sum_{i} \sum_{h} \omega_{ijkl}$$

(9)

Moran’s $I$ varies from minus one to plus one, with an expected value approaching zero (from below) as the sample size tends to infinity; a positive and significant value of the test statistic indicates positive spatial autocorrelation. The benefit of using the change in input levels is that we eliminate spatial correlation driven by correlation in time invariant district-level attributes. The global Moran $I$ statistics indicate substantial (own-policy) spatial correlation for each input – ranging from 0.17 (school size) to 0.47 (average teacher salary) – and we always reject the null of no spatial autocorrelation at the $p < 0.01$ level. To determine if this spatial dependence represents a causal relationship, we turn to the regression analysis.

5. Results

5.1. Baseline results

As stated previously, we discuss only the uni-dimensional results given the more straightforward interpretation of the results and the fact that the majority of the cross-policy effects are not statistically significant. The contemporaneous results using the full sample (pooling PTELL and non-PTELL observations) are presented in Table 2, while the lagged specification results are given in Table 3. Table 2 presents two sets of results: one using all years of data and another obtained using only the data from 1990 and 2000 (thereby avoiding the need to interpolate the Census data). In all cases, the estimated coefficients on neighboring inputs represent elasticities.

In terms of the contemporaneous results using all 11 years of data, we first note that each specification fares extremely well in terms of the Hansen overidentification test (except school size), $F$-test of the joint significance of the instrument set, Hall et al. (1996) test of instrument relevance, and Staiger and Stock (1997) measure of maximum squared bias. In addition, the Pagan and Hall (1983) test rejects homoskedasticity in three of the five cases (pupil–teacher ratio, current PSE, and capital PSE), affirming the efficiency gain from GMM relative to two-staged least squares. In terms of the actual estimates, we find a positive and statistically significant impact of neighboring inputs in each of the five equations estimated via GMM, with the elasticities ranging from 0.23 (school size) to 0.87 (average teacher salary). Moreover, as suggested by the magnitude of the elasticities, the degree of strategic behavior is economically significant as well. For instance, evaluated at the sample mean, a one standard deviation decline in neighboring pupil–teacher ratio (representing a decrease of 2.97 students per teacher) is expected to result in a decline in own pupil–teacher ratio of 1.84 students per teacher; a one standard deviation increase in neighboring average teacher salary (roughly a $7000/year raise) is expected to result in an increase in own average teacher salary of over $6100/year. As a result, there does appear to be a significant amount of competitive pressure exerted upon public school district administrators from neighboring public school districts.

16 First-range are not presented, but are available upon request. The instrument set is provided in the footnote in Table 2; there are seven overidentifying restrictions in each model.
Restricting the sample to only observations from 1990 and 2000, the results are virtually unchanged for four of the five inputs. The lone exception is capital PSE, where the elasticity falls from 0.57 (s.e. = 0.19) to 0.21 (s.e. = 0.27). In the remaining four equations, the elasticity remains statistically significant and is actually larger in magnitude, ranging from 0.27 (school size) to 1.00 (average teacher salary). Moreover, the diagnostic tests continue to suggest that the models are well-specified despite the decrease in sample size. The only noteworthy changes are in the Hansen overidentification test for the pupil–teacher ratio equation, where the $p$-value drops from 0.61 to 0.06, and in the $F$-test of the joint significance of the instrument set in the capital PSE and teacher salary equations, where the $p$-value increases from 0.01 to 0.07 and 0.41, respectively. Nonetheless, the Hall et al. and Staiger-Stock tests both suggest little concern about weak instruments. Overall, then, the results do not appear sensitive to the interpolation procedure.17

Turning to the lagged specifications (Table 3), we find a positive and statistically significant effect of two-year lagged neighboring inputs (Panel I) in four of the five equations (the lone exception being capital PSE). However, in all cases, the elasticities are much smaller in magnitude relative to the contemporaneous estimates, ranging from 0.11 (school size) to 0.23 (pupil–teacher ratio). The five-year lagged values (Panel II) are always insignificant. Thus, the uni-dimensional, full sample results indicate that public school districts act strategically when making input decisions, incorporating the input decisions of neighboring school districts, and that this strategic behavior operates within a two-year window.

Table 3
Strategic interaction over public school quality inputs with a lag: uni-dimensional framework (full sample, 1990–2000)

<table>
<thead>
<tr>
<th>Independent variable</th>
<th>School quality measure</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Pupil–teacher ratio</td>
</tr>
<tr>
<td>I. Two-year lag</td>
<td>ln(neighboring school quality)</td>
</tr>
<tr>
<td></td>
<td>(0.07)</td>
</tr>
<tr>
<td>Observations</td>
<td>7709</td>
</tr>
<tr>
<td>II. Five-year lag</td>
<td>ln(neighboring school quality)</td>
</tr>
<tr>
<td></td>
<td>(0.07)</td>
</tr>
<tr>
<td>Observations</td>
<td>5145</td>
</tr>
</tbody>
</table>

Notes: Estimation is via OLS. For further details, see Table 2.

17 While not presented, we also conducted two additional sensitivity analyses. First, we re-estimated all the models in Table 2 assuming a linear (rather than log-linear) specification. The results are qualitatively unchanged. Using all years, the elasticities (computed at the sample means) are 0.60 (s.e. = 0.16) for pupil–teacher ratio, 0.59 (s.e. = 0.13) for current PSE, 1.08 (s.e. = 0.19) for capital PSE, 0.92 (s.e. = 0.15) for average teacher salary, and 0.25 (s.e. = 0.09) for school size. However, the first-stage diagnostics indicate the instruments are weak for capital PSE, and Hansen overidentification test rejects the validity of the instruments of the school size. Using only 1990 and 2000 data, the corresponding elasticities were 0.67 (s.e. = 0.09) for pupil–teacher ratio, 0.77 (s.e. = 0.15) for current PSE, 0.55 (s.e. = 0.15) for capital PSE, 1.05 (s.e. = 0.14) for average teacher salary, and 0.34 (s.e. = 0.09) for school size. Moreover, all models fair well in terms of the diagnostic tests. For our second sensitivity analysis, using all years of data, we added an interaction term in each model between neighboring input choice and a linear time trend to assess the temporal stability of the parameters of interests. We treated this variable as endogenous as well, using the original instruments interacted with a linear time trend as additional exclusion restrictions. The interaction term was statistically significant at conventional levels only for school size; however, the magnitude was extremely small (the elasticity increases by $5.38e−06$(s.e. = $1.18e−06$) per year).
Table 4
Strategic interaction over public school quality inputs: uni-dimensional framework (non-PTELL sample, 1990–2000)

<table>
<thead>
<tr>
<th>Independent variable</th>
<th>School quality measure</th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Pupil-teacher</td>
<td>Current PSE</td>
<td>Capital PSE</td>
<td>Teacher PSE</td>
<td>School size</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>All years</td>
<td>1990 and 2000 only</td>
<td>All years</td>
<td>1990 and 2000 only</td>
<td>All years</td>
<td>1990 and 2000 only</td>
<td>All years</td>
</tr>
<tr>
<td>ln(neighboring school quality)</td>
<td>0.14 (0.15)</td>
<td>0.11 (0.24)</td>
<td>0.06 (0.18)</td>
<td>0.37 (0.26)</td>
<td>0.04 (0.21)</td>
<td>0.17 (0.31)</td>
<td>0.57 (0.27)</td>
</tr>
<tr>
<td>Pagan and Hall (1983) heteroskedasticity test</td>
<td>$[p=0.00]$</td>
<td>$[p=0.00]$</td>
<td>$[p=0.00]$</td>
<td>$[p=0.00]$</td>
<td>$[p=0.00]$</td>
<td>$[p=0.00]$</td>
<td>$[p=0.00]$</td>
</tr>
<tr>
<td>Hansen’s J-statistic (overidentification test) $F$-test of joint significance of instrument set</td>
<td>$F=13.16$</td>
<td>$F=4.04$</td>
<td>$F=6.59$</td>
<td>$F=1.49$</td>
<td>$F=2.88$</td>
<td>$F=1.37$</td>
<td>$F=4.70$</td>
</tr>
<tr>
<td></td>
<td>$[p=0.00]$</td>
<td>$[p=0.00]$</td>
<td>$[p=0.00]$</td>
<td>$[p=0.00]$</td>
<td>$[p=0.01]$</td>
<td>$[p=0.00]$</td>
<td>$[p=0.01]$</td>
</tr>
<tr>
<td>Staiger and Stock (1997) measure of maximum relative bias</td>
<td>$B_{max} = 0.003$</td>
<td>$B_{max} = 0.03$</td>
<td>$B_{max} = 0.002$</td>
<td>$B_{max} = 0.04$</td>
<td>$B_{max} = 0.005$</td>
<td>$B_{max} = 0.03$</td>
<td>$B_{max} = 0.01$</td>
</tr>
<tr>
<td>Observations</td>
<td>5527 332</td>
<td>5527 332</td>
<td>5442 327</td>
<td>5518 332</td>
<td>5527 332</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: The non-PTELL sample includes all county-year observations for which PTELL was not in effect. For further details, see Table 2.
Table 5
Strategic interaction over public school quality inputs: uni-dimensional framework (PTELL sample, 1990–2000)

<table>
<thead>
<tr>
<th>Independent variable</th>
<th>School quality measure</th>
<th>Pupil/teacher ratio</th>
<th>Current PSE</th>
<th>Capital PSE</th>
<th>Teacher salary</th>
<th>School size</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>All years</td>
<td>1990 and 2000 only</td>
<td>All years</td>
<td>1990 and 2000 only</td>
<td>All years</td>
<td>1990 and 2000 only</td>
</tr>
<tr>
<td>ln(neighboring school quality)</td>
<td>0.907†</td>
<td>1.02†</td>
<td>1.38†</td>
<td>0.66†</td>
<td>0.73†</td>
<td>1.17†</td>
</tr>
<tr>
<td>Pagan and Hall (1983)</td>
<td></td>
<td>[p=0.00]</td>
<td>[p=0.00]</td>
<td>[p=0.00]</td>
<td>[p=0.00]</td>
<td>[p=0.00]</td>
</tr>
<tr>
<td>Hansen’s J-statistic</td>
<td></td>
<td>[p=0.98]</td>
<td>[p=0.99]</td>
<td>[p=0.97]</td>
<td>[p=0.99]</td>
<td>[p=0.97]</td>
</tr>
<tr>
<td>heteroskedasticity test (overidentification test)</td>
<td></td>
<td>[p=0.37]</td>
<td>[p=0.38]</td>
<td>[p=0.39]</td>
<td>[p=0.37]</td>
<td>[p=0.38]</td>
</tr>
<tr>
<td>F-test of joint significance of instrument set</td>
<td></td>
<td>F=-17.04</td>
<td>F=7.76</td>
<td>F=29.20</td>
<td>F=27.68</td>
<td>F=4.32</td>
</tr>
<tr>
<td></td>
<td>[p=0.00]</td>
<td>[p=0.00]</td>
<td>[p=0.00]</td>
<td>[p=0.00]</td>
<td>[p=0.00]</td>
<td>[p=0.00]</td>
</tr>
<tr>
<td>Hall et al. (1996) test of instrument relevance</td>
<td></td>
<td>µ=0.72</td>
<td>µ=0.76</td>
<td>µ=0.55</td>
<td>µ=0.85</td>
<td>µ=0.22</td>
</tr>
<tr>
<td></td>
<td>[p=0.00]</td>
<td>[p=0.00]</td>
<td>[p=0.00]</td>
<td>[p=0.00]</td>
<td>[p=0.00]</td>
<td>[p=0.00]</td>
</tr>
<tr>
<td>Staiger and Stock (1997) measure of maximum relative bias</td>
<td></td>
<td>Bmax=0.008</td>
<td>Bmax=0.02</td>
<td>Bmax=0.01</td>
<td>Bmax=0.02</td>
<td>Bmax=0.02</td>
</tr>
<tr>
<td>Observations</td>
<td>2521</td>
<td>518</td>
<td>2521</td>
<td>518</td>
<td>2510</td>
<td>511</td>
</tr>
</tbody>
</table>

Notes: The PTELL “All years” sample includes all county-year observations for which PTELL was in effect. The PTELL “1990 and 2000 only” sample includes all counties for which PTELL was in effect in 2000; 1990 observations for these counties are included as well, although PTELL was not in effect at that time. For further details, see Table 2.
5.2. Impact of PTELL

To assess whether there exist important interactions between school financial constraints and the strategic behavior of public school district administrators, we re-estimate the previous unidimensional specifications on two sub-samples of the data: district-year observations constrained by ‘tax caps’ under PTELL (hereafter referred to as the PTELL sample) and district-year observations not facing such constraints (hereafter referred to as the non-PTELL sample). Table 4 presents the contemporaneous results – estimated via GMM – utilizing the non-PTELL sample (using all 11 years of data as well as only observations from 1990 and 2000). Table 5 displays the analogous results for the PTELL sample.

The results in Table 4 are easily summarized: there exist little evidence of strategic behavior by public school districts in counties not constrained by PTELL. Of the 10 elasticities, only one is statistically significant (capital PSE using all 11 years of data). The statistically insignificant results in Table 4 occur despite the fact that the diagnostic tests all continue to validate the specifications and instrument set.

Utilizing the PTELL sample (Table 5), we now once again find strong evidence of strategic behavior. Specifically, we find a statistically significant effect of neighboring input decisions in all five cases, regardless of whether we utilize all 11 years of data or only 1990 and 2000 observations. Moreover, the diagnostic tests suggest the validity of all the equations estimated regardless of the inclusion/exclusion of the interpolated data. Finally, we note that the elasticities are all very large in magnitude, ranging from approximately 0.50 (school size) to over unity (current and capital PSE, depending on the sample).

The fact that we only find evidence of strategic behavior by public school administrators in counties constrained by ‘tax caps’ is perhaps somewhat surprising. There are several possible explanations. First, ‘tax caps’ may have a causal effect on the strategic behavior of school districts. Specifically, since various revenue sources – federal aid, state aid, and local property taxes – are substitutes in Illinois, then federal and state aid become relatively more important in the presence of binding tax caps. Moreover, as the distribution of federal and state aid is (at least partially) based on student enrollment (see Section 2), then one would expect competition for students to be more intense in counties that have enacted PTELL. Second, ‘tax caps’ may have a causal effect on the behavior of districts, not through competition for students, but rather by converting the budget constraint faced administrators from soft to hard. As a result, school administrators may become more fiscally responsible and/or wary of lagging behind neighboring districts in terms of ‘quality’ when constrained by PTELL. Both scenarios may lead to a situation whereby administrators ‘mimic’ neighboring districts in PTELL counties. Finally, ‘tax caps’ may be endogenous, being enacted only in certain counties as a means of curtailing unchecked taxation and spending. As a result, the strategic behavior of administrators may reflect unobservable attributes of counties, and therefore pre-date the enactment of PTELL.

Differentiating between the two causal explanations is beyond the scope of the current paper. However, to examine the hypothesis that the impact of PTELL is not causal, but rather reflects

18 Recall that no countries were constrained by PTELL until 1991. As a result, the specifications using only observations from 1990 and 2000 utilize data from 1990 and 2000 on districts constrained by PTELL in 2000.
19 The 95% confidence intervals are never strictly above unity for any parameter; thus, we, do not find any robust evidence of a spatially ‘explosive’ process.
20 For, sensitivity, we added an interaction term between neighboring input choice and a linear time trend in each model using all years of data to asses the temporal stability of the parameters of interest. See footnote 17. The interaction term was never statistically significant at conventional levels.
unobserved county attributes, Table 6 presents the results from contemporaneous specifications obtained using districts in PTELL counties but only in years prior to the enactment of PTELL. If rampant competitive pressure results in strategic behavior by administrators prior to PTELL, then this suggests that the ‘tax caps’ may be the result of, as opposed to the cause of, the strategic behavior. For four of the five inputs (pupil–teacher ratio being the exception), however, neighboring input decisions have a statistically insignificant impact on own input decisions (although the point estimates are all positive). Thus, the evidence is consistent with ‘tax caps’ contributing to the strategic behavior of public school administrators. As this is the first empirical assessment (to our knowledge) of the interplay between competition and school financial constraints, this is an extremely important finding as it suggests that competition (at least from other public schools) only affects the behavior of public schools when the ability to raise revenues is severely constrained, or (virtually) all sources of revenue are tied to student enrollment. If such a result extends to the effects of competition arising from other sources, such as private or charter schools, then clearly policymakers must be aware of the heterogeneous effects of competition; expanded competition alone may be insufficient to alter the incentive structure faced by school administrators. As such, this appears to be a fruitful avenue for future research, both theoretically and empirically.

6. Conclusion

Improving educational quality has become an important policy goal in the 21 century. Success, however, requires identifying factors associated with student achievement. While still in its

21 An alternative approach to assess the casual effect of PTELL is through the use of instrumental variables. Since no obvious instrument exists, we leave this for future research.

22 Interestingly, the lack of strategic behavior in the non-PTELL sample suggests that competition over students (as in the theoretical model) when revenue derived from student enrollment is at a premium (because local property tax revenue is capped) is likely to be the underlying cause of the strategic interaction in the PTELL sample. Specifically, yardstick competition cannot explain the absence of strategic behavior in countries without ‘tax caps’. See footnote 8.
relative infancy, there is growing support for the notion that public school quality cannot be improved without a fundamental altering of the incentive structure faced by school administrators. Consequently, many recent proposals call for new modes of school choice in order to bring the ‘invisible hand’ of competition to the public education arena. As of December 2002, five states already had a publicly funded voucher program, six states offered tax credits or deductions for educational expenses or contributions to scholarship programs, 39 states and Washington, D.C. had implemented charter school laws, and nine (21) states offered statewide (limited) public school choice.23

To understand the effects of school competition on the behavior of public school administrators, as well as assess the level of strategic competition arising ‘naturally’ in the present education system through the Tiebout process, we present a theoretical model predicting that public school administrators incorporate the educational input decisions of neighboring school districts into their own decision calculus, thereby acting strategically when setting their own input levels. Testing this prediction on panel data from Illinois over the period 1990–2000, we find robust evidence consonant with the theoretical model. Specifically, we find substantial evidence that public school districts respond strategically to changes in the pupil–teacher ratio, average teacher salary, current and capital per pupil expenditures, and school size in other public school districts located in the same county, and this response occurs within a two-year window. However, this strategic behavior depends crucially on the fiscal environment confronted by school district administrators. In counties where local property tax revenues are constrained by ‘tax caps,’ such strategic competition is statistically and economically significant; in counties without ‘tax caps,’ the input choices of school districts are unresponsive to the behavior of neighboring districts.

Given the important linkage uncovered between fiscal environment and competition, more research is clearly warranted. Understanding potential theoretical foundations for such an interaction, as well as further empirical analysis to confirm the robustness of the relationship as well as the underlying source of such interactions, are necessary. That said, our findings (i) provide support for the claim that competition can alter the behavior of public school administrators, but also indicate that (ii) proposed school choice expansions are implemented in a system that is not devoid of competition and (iii) competition alone may be insufficient to alter the quality of public schools in the US.

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