Demand Matters: School District Concentration, Composition, and Educational Expenditure

Miguel Urquiola *
Department of Economics
UC Berkeley

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Abstract

Whether inter-jurisdictional competition improves governments' efficiency is an enduring question in Local Public Economics. Based on Tiebout's framework, recent results suggest this effect is present in the case of school districts: *decentralization* (greater district availability) may reduce expenditures without sacrificing performance. Other research, however, draws on Tiebout to warn that school finance reforms, which effectively *centralize* educational provision, may also lower expenditure. These literatures disagree because they emphasize different aspects of a Tiebout system: competition, on the one hand, and the effects of stratification on demand patterns, on the other. The endogeneity of district formation makes it difficult to test their claims. To address these issues, this paper notes that metropolitan areas in fact contain two educational markets, one at the primary and one at the secondary level. Because in many cases different numbers of districts operate at each level, it is possible to identify the effects of concentration using differences in district availability *between levels, within areas.* This approach introduces significant controls for unobserved heterogeneity. Additionally, it requires the use of educational *level-specific data*, which allows controls for systematic between-level differences that previous work ignored. The results suggest district availability does lead to Tiebout-style segregation, making relevant the mechanisms emphasized by the School Finance Reform literature. Their presence may explain why greater district availability is also found to be associated with greater average expenditure.

JEL classification: H10, H40, I20.

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1 Introduction

In recent years, concern over the quality of public schools has prompted initiatives to increase educational spending and reduce class size. A large body of research suggests, however, that raising expenditure may not improve students' outcomes. It warns that school districts may not allocate funds efficiently enough for the money to make much difference.[1]

This situation has generated interest on how increased competition might enhance districts' efficiency. Borrowing from a well-established Industrial Organization paradigm, one can view this research as analyzing how the number of school districts in a given area (a measure of market *structure*) influences their efficiency (a measure of *conduct or performance*).

Drawing from Tiebout's notion of competition between local jurisdictions, this literature suggests that, all else equal, areas with more districts spend less money without necessarily sacrificing schooling outcomes, presumably because they avoid "bureaucratic waste". In other words, this "competition" literature attributes the relationship between concentration and expenditure to a *supply side* effect: greater district availability intensifies competition, making districts more efficient.

These results are related to another literature on how school finance reforms affect expenditure. With an equity objective, these reforms seek to reduce inter-district spending differences that naturally arise in Tiebout settings. Because they restrict districts' ability to differentiate themselves, one can view these measures as effectively reducing district availability.

Some economists have warned that by centralizing educational provision, these reforms can lead to a *reduction* in educational expenditure. Their theoretical basis is also Tiebout's framework, but their focus is on a *demand side* effect. Centralization, they argue, may lower the income and educational spending preferences of hypothetical pivotal voters. Thus, *if* households in fact segregate into districts based on demand, as Tiebout predicted, greater district availability may be associated with *higher* educational expenditure.

To summarize, the "competition" and "school finance reform"

[1] For an overview of the controversy over the effectiveness of educational expenditure, see Burtless (1996).

literatures concentrate on different aspects of a Tiebout system's operation. Emphasizing a supply side argument, the "competition" approach predicts areas with more districts will have lower educational expenditures. Stressing a demand side motivation, the "finance reform" view predicts the opposite outcome. The difference is that the "finance reform" literature stresses the way Tiebout segregation affects demand patterns.

Empirically evaluating these claims has proved difficult, and there are conflicting results both within and between these literatures. A central problem is that district configuration, like market structure, is not randomly assigned, so areas' unobserved characteristics may affect the number of districts they choose to have, at the same time as they influence expenditure. Alternately, states that undertake school finance reforms may be systematically different from those that do not.

This paper attempts to circumvent some of these problems using a new research design. The approach starts with the observation that metropolitan areas (the markets generally considered in the literature) in fact contain two educational markets, one at the primary and one at the secondary level, and that these often have different numbers of districts. It is therefore possible to identify the effects of changes in district availability relying on differences in the number of districts *between levels, within areas.* Two factors underlie this research design's contribution:

1) the use of within-area variation in district structure introduces significant controls for unobserved heterogeneity across metropolitan areas.
2) the approach requires the use of *educational level-specific* data, which allows the analysis to control for systematic and significant differences between the primary and secondary sectors. Combined data (covering both levels) led other studies to ignore these, introducing a type of aggregation bias that is shown to be empirically important. Indeed, the significant results presented below arise as much from this data improvement as from the new identification strategy.

Applying this research design suggests increased district availability does lead to significant Tiebout-style segregation. In other words, the source of the demand-side mechanisms emphasized by the school finance reform literature is shown to be relevant. Its presence may account for the fact that this study also finds greater district availability is associated with *greater* average educational expenditure, a result with important implications for the literatures listed above.

The remainder of the paper contains five sections. The next presents a theoretical framework and literature review. Section 3 introduces the research design, and Section 4 describes the data used to apply it. Section 5 presents results, and Section 6 concludes.

2 Theory and literature review

Tiebout (1956) provides the theoretical framework for the literature on district concentration and educational expenditure. His framework contrasts situations where multiple jurisdictions provide a service with those where a single government is the provider, suggesting that decentralized local provision will have a number of efficiency properties.[2] These arise as households settle in the jurisdiction that comes closest to satisfying their demand.

School district-based educational provision has become the central example of a Tiebout mechanism. In this context, the framework suggests that all else equal, areas with greater district availability will display:

- *Greater stratification.* Districts are likely to be more homogeneous as similar households group together and jointly procure the type of schooling they prefer.
- *Greater competitive pressures.* Greater district availability will increase competition, and inefficient districts will be unable to attract and retain residents and resources.

These consequences of the system's operation, which can be labeled "first stage" effects, will affect public educational expenditure through supply and demand channels:

- *Supply effects.* Here the causation in clear: competition produces cost savings which, all else equal, will lead to lower expenditures.
- *Demand effects.* Stratification will create high and low-spending districts, so while it clearly increases inter-district expenditure

[2] This result requires assumptions concerning inter-jurisdictional spillovers, household mobility, information, and jurisdictional supply.

differences, it has ambiguous effects on aggregate expenditure. Additionally, by reducing costs and thus the effective price of quality, competition could lead households to spend more on education.[3]

Thus, when both demand and supply effects are considered, the theoretical impact of district availability on expenditure is ambiguous.

Changes in private enrollment provide an additional channel through which both supply and demand effects may affect expenditure. Both greater stratification (because it allows districts to more closely satisfy residents' tastes) and greater competition (because it makes districts more efficient) should reduce private enrollment. In Industrial Organization terms, private schools may be a "competitive fringe" that becomes less attractive as the public sector becomes more efficient or produces a greater variety of goods. District availability may also increase expenditure, therefore, if by reducing private enrollment it incorporates high income or high demand households into the public sector.

The following sections explore the theoretical basis and previous evidence on demand and supply effects in greater detail. Because the demand channels depend crucially on the presence of Tiebout stratification, the first one briefly reviews evidence in this area.

2.1 Tiebout sorting or stratification

The central implication of Tiebout's framework is that, in equilibrium, jurisdictions should contain households homogeneous in their demand for the local public good provided. Because this implication is difficult to test, empirical research has taken several other approaches.[4] One starts by observing that if households in a jurisdiction are homogeneous in their demand for a public good, then they should be homogeneous along observable characteristics related to this demand. For instance, Eberts & Gronberg (1981) indicate that if the income elasticity of demand for public goods is nonzero, then homogeneous grouping by demand implies homogeneous grouping by income. Hamilton (1975) suggests this tendency may be strength-

[3] Most previous work does not emphasize the latter, indirect channel.

[4] Hoyt & Rosenthal (1997) develop a test addressing this prediction. They find results that satisfy a necessary but not sufficient condition for efficiency. Taking a different approach, Gramlich & Rubinfeld (1982) find that the variance in individuals' stated willingness to pay for local public services is less within individual jurisdictions than at the state level.

ened as households seek to avoid redistribution toward lower-income neighbors, and Grubb (1982) points out that preferences for isolation along racial or other socioeconomic characteristics will have a similar stratifying effect.

Perhaps surprisingly, there is mixed evidence on the link between district concentration and district homogeneity. With respect to income, Eberts & Gronberg (1981), and Schmidt (1992) present results that, though generally supportive of the Tiebout hypothesis, are not always statistically significant.

These studies do not address the endogeneity of school district formation. Hoxby (1997) deals with this by instrumenting district concentration with topographical variables.[5] Her results suggest students' *school-level* peer groups are not affected by the number of districts in their MA's. She hypothesizes that districts are large enough for sorting to essentially occur at the school level, *within* districts.

Grubb (1982) relies instead on panel data, noting that given an area's district structure, one should observe increasing homogeneity developing over time. His results on income homogeneity are consistent with the Tiebout framework, but contradict it in the case of racial structure.

2.2 Tiebout sorting: demand side effects

Though the evidence on Tiebout stratification is mixed, the "school finance" literature relies on it to warn that reducing districts' financial independence could lower spending.[6] To see the logic behind this argument, suppose all households, indexed $h = 1, ..., H$, are identical except for their income level, y_h, distributed between y_{min} and y_{max} with density $f(y_h)$. Households consume one unit of "schooling", but have a choice as to the quality, q, they buy at a constant price, p. This situation would arise, for instance, if households have one child, there are compulsory attendance laws, and there is some form of choice among schools.

Households consume q and a numeraire composite good, z. That

[5] Hoxby instruments district concentration with the number of rivers and streams in different areas, hypothesizing that because these were natural barriers to student transportation patterns, they influenced the number of districts created during European settlement periods. Eberts & Gronberg (1981) also apply an instrumental variables approach (as described below).

[6] See for instance Silva & Sonstelie (1995), on which some of the discussion below is based.

is, they solve:

$$max \quad u(q,z) \quad s.t. \quad pq + z = y_h.$$

The marginal condition $u_q/u_z = p$ generates a quality demand function, $q(p,y)$, which is assumed to take the following form (with $\beta_p < 0$ and $\beta_y > 0$):

$$q_h = \beta_0 + \beta_p p + \beta_y y_h \qquad (1)$$

Even in this particularly simple scenario, the impact of district concentration on expenditure is not clear. To see this, consider first a completely decentralized educational system, where each household buys the precise quality level it desires. This resembles private provision, but can also be viewed as a situation with "perfect" sorting (one could assume that h denotes a household type, or that there are as many districts as households). In this decentralized scenario, mean educational expenditure is given by

$$p\overline{q} \;=\; p\int_{y_{min}}^{y^{max}} [\beta_0 + \beta_p p + \beta_y y_h] f(y_h) dy_h, \;=\; p[\beta_0 + \beta_p p + \beta_y \overline{y}] \qquad (2)$$

that is, mean expenditure is essentially a function of mean income.

Now consider the polar opposite situation, where all households are merged into a *single* district. Assume they select the district's expenditure level by voting for a uniform tax, t. All children now receive the district quality, $\tilde{q}$, financed with tax revenue such that $p\tilde{q} = t \int y_h f(y_h) dy_h = t\overline{y}$. In deciding what level of t to vote for, each household solves:

$$max_{\ t} \quad u(q,z) \quad s.t. \quad q = \tilde{q} = \frac{t\overline{y}}{p} \quad and \quad (1-t)y = z,$$

with the solution implicitly given by:

$$\frac{u_q}{u_z} = p\left(\frac{y}{\overline{y}}\right).$$

This lowers the effective price of quality for households with income below the mean, reflecting their incentive to "free ride" on their higher income neighbors. Relying on the median voter theorem, the district quality level, $\tilde{q}$, will be selected by the household with the median income, $\tilde{y}$, so mean expenditure will be:

$$p\tilde{q} = p\left[\beta_0 + \beta_p p\left(\frac{\tilde{y}}{\overline{y}}\right) + \beta_y \tilde{y}\right]. \quad (3)$$

Comparing equations (2) and (3) shows a reduction in district availability has two effects on mean expenditure. First, there is a price effect. If the median is smaller than the mean income, this will tend to *increase* expenditure. Second, there is an income effect, which will go in the opposite direction. The aggregate effect is therefore ambiguous.

The school finance literature has relied on such a demand-side argument to explain spending reductions observed in some states. This research has also produced mixed results. Manwaring & Sheffrin (1996) and Evans, Murray & Schwab (1997) suggest school finance reforms do not reduce aggregate educational funding, while Downes & Shah (1995), and Silva & Sonstelie (1995), argue such reductions are likely to take place.[7]

2.3 Tiebout competition: supply side effects

An extensive literature emphasizes the supply rather than demand-side effects of decentralized educational provision. This literature begins from the premise that when governments enjoy market power, they can behave like "Leviathans", exploiting it to engage in wasteful expenditure. In contrast, inter-jurisdictional competition may encourage efficiency.

To incorporate this view in the above framework, note that the "demand side" analysis assumed educational quality was available at a constant price, p. It is possible to characterize the "competition" literature as pointing out this price varies not only with the price of inputs (teachers, facilities), p_0, but also with the number of school districts, N, in the market under consideration. That is, there is a function[8]

$$p(p_0, N), \quad where \quad \frac{\partial p}{\partial N} < 0. \quad (4)$$

[7] Hoxby (1996*a*) emphasizes these different results may explained by the *marginal* expenditure disincentives implicit in some finance reforms. This appropriately emphasizes the variety of reform designs.

[8] It is possible to more formally motivate why $\partial p/\partial N < 0$. For instance, this can arise from a "circle" model of product differentiation as suggested by Salop (1979), where districts are viewed as firms that produce differentiated services, and consumers' location denotes their preference for educational expenditure.

Assuming constant demand for quality therefore, it follows that areas with fewer jurisdictions should display greater expenditure.[9]

A large literature is based on this "competition" motivation. Oates (1985) proposed and tested the hypothesis that the size of the public sector is inversely related to the extent of fiscal decentralization, measured as the number of jurisdictions in each state. Using state-level data, he found no significant relation.

Focusing on single-purpose governments, Nelson (1987) found the expected relation, but some of his key estimates are statistically insignificant. Focusing specifically on school districts, Burnell (1991) finds that counties with greater district availability in fact have *higher* per-pupil expenditures.

More recent studies of inter-jurisdictional competition have tended to focus on district-level expenditures. Couch, Shughart II & Williams (1993), Borland & Howsen (1992), Hoxby (1997) take this approach. Their results suggest that, all else equal, districts in areas with more districts have lower per-pupil expenditures, yet produce educational results that are just as good or better than areas with fewer districts.[10]

2.4 Net effects from demand and supply channels

Studies emphasizing the supply side effects of Tiebout systems generally expect an inverse relation between jurisdictional availability and expenditure. Efficiency effects, however, may increase expenditure through a *price-induced*, demand effect.[11] Combining equations (2) and (4) shows that in the case of complete decentralization, mean expenditure will be

$$p(p_0, N)\overline{q} \;=\; p(p_0, N)[\beta_0 + \beta_p p(p_0, N) + \beta_y \overline{y}] \tag{5}$$

In contrast, combining (3) and (4), shows that in the single district scenario this is

[9] This assumption is usually not explicitly stated. It may not, however, be necessarily unreasonable in the context of the educational finance literature. To the extent that additional expenditure in fact does not enhance educational outcomes, as much literature suggests, households may just choose to "pocket" any savings that come from increased efficiency. This point is discussed further below.

[10] There is also a recent literature on how school finance reforms have affected the level and distribution of student achievement. See Card & Payne (1997) and Downes & Figlio (1997).

[11] Earlier studies, such as Oates (1985), explicitly discussed this possibility.

$$p(p_0, 1)\tilde{q} = p(p_0, 1)\left[\beta_0 + \beta_p p(p_0, 1)\left(\frac{\tilde{y}}{\overline{y}}\right) + \beta_y \tilde{y}\right]. \qquad (6)$$

This efficiency effect is another reason why expenditure in the decentralized scenario (5) may be higher, since $p(p_0, N) < p(p_0, 1)$. Intuitively, if competition lowers the effective price of quality, households may decide to buy more of it. Despite this consideration, the net effect of district concentration remains ambiguous even in this simplified context.

2.5 Private enrollment

Changes in private enrollment are an additional channel through which district availability may increase expenditure. This arises because in practice centralization of the type described is not forced, and households can always enroll their children in the private sector, explicitly buying the quality level they can no longer procure through the local public goods system.

In the above framework, let $v(p, y)$ be the indirect utility function, indicating the utility achieved by a household with income y facing quality price p. Despite paying the head tax that gives them access to "free" public schools, households will exit the public sector if:

$$v[p, (1-t)y] > u[\tilde{q}, (1-t)y]$$

As high income households exit the public sector, $\tilde{y}$ and $\tilde{q}$ will fall. In the present framework, only the highest income households will exit the public sector, more generally, "high demand" households, independent of their income, will leave as well.[12]

There has also been mixed evidence on the relationship between district availability and private enrollment. Martinez-Vazquez & Seaman (1985) relate district concentration and private enrollment in 75 large MA's. They obtain mixed results depending on the concentration measure, and in some cases coefficients are of the "wrong" sign. Schmidt (1992) jointly models private school enrollment, intra-district income heterogeneity, and public school quality

[12] These effects make the relation between district concentration and total (public and private) expenditure ambiguous as well. This broader point was originally emphasized by Peltzman (1973).

(as measured by expenditures per-pupil). She finds a greater number of districts is associated with more homogeneous districts, and also with lower private enrollment.

2.6 Net effects: district-level expenditures

The preceding sections have shown that the link between district concentration and aggregate educational expenditure in the public sector is ambiguous, and that many of the "demand side" effects that introduce this ambiguity hinge on the presence or absence of Tiebout-style sorting. Before proceeding to investigate these issues, it is relevant to note that while most of the earlier "competition" literature focused on average expenditure levels, more recent research, such as Borland & Howsen (1992) and Hoxby (1997), considers *district-level* expenditures. If i and j index districts and the areas they belong to, respectively, their approach entails running reduced form regressions of the form:

$$pq_{ij} = \beta_0 + \beta_y \tilde{y}_{ij} + \beta_N N_j + \epsilon_{ij} \tag{7}$$

where $\tilde{y}_{ij}$ stands for median income, but could be interpreted as a vector of district socio-economic characteristics. N_j is a (sometimes instrumented) measure of district concentration. These studies generally find that $\beta_N < 0$, and attribute this effect to increased competition.

In the presence of Tiebout sorting, however, the expected sign of β_N is ambiguous. At least two reasons account for this. First, to the extent that changes in district concentration change the price of quality, this coefficient will also be capturing a price effect, as illustrated in the previous sections.

The second, more subtle reason is an implication of "Tiebout bias", as defined by Goldstein & Pauly (1981) and Rubinfeld & Roberts (1986). To illustrate this in the framework introduced above, assume $p = 1$ and relax the assumption that all households have the same propensity to spend on quality. This introduces variance in "tastes" for education, i.e., β_y will be a function of y so that demand, instead of (1), is now given by:

$$q_h = \beta_0 + \beta_p p + \beta_y(y) y_h \tag{8}$$

If each household privately purchased education and household level data was available, one could estimate a simple linearized Engel function:

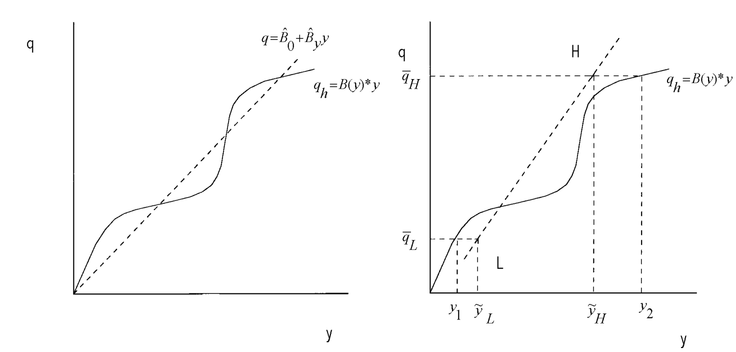
$$q_h = \beta_0 + \beta_y y_h + \epsilon_h. \tag{9}$$

Tiebout bias arises because in the presence of a local public goods system, *districts'* expenditure is used, matching each observation with the characteristics of a resident household, say, that with the median income. Instead of (9), a regression like the following is used:

$$q_{ij} = \beta_0 + \beta_y \tilde{y}_{ij} + \epsilon_{ij} \tag{10}$$

To see the source of Tiebout bias, consider panels A and B of Figure 1 (page 11), both of which show the "true" household-level Engel relation $q_h = \beta_y(y)y_h$.

Figure 1: Tiebout bias illustration



Panel A shows the fitted line that could be obtained by applying a regression like (9) to household level data. Now consider panel B. On the vertical axis are featured expenditure levels $\overline{q}_H$ and $\overline{q}_L$, corresponding to a high and a low spending district. As can be seen, household incomes y_2 and y_1 correspond to these expenditure levels.

Goldstein & Pauly (1981) illustrate Tiebout bias by asking if y_1 will actually be the median income of households in district L. Since L is composed of households that prefer low expenditure not only because they have low incomes, but *also* because they have low tastes for q, it is likely that $\tilde{y}_L > y_1$, as illustrated. By analogous reasoning, one can expect $\tilde{y}_H < y_2$, so that the actual observations using district data may be given by points H and L in panel B, which will generate an upward bias on $\hat{\beta}_y$. As they observe, "the Tiebout process is a *behavioral* method which results in a grouping of individuals by their constrained optimal values of [q]. If [q] is the dependent variable in an empirical *demand* estimation, the Tiebout process in effect groups observations by the dependent variable."

This implies that if Tiebout sorting is operative, regressions like (7) will capture at least two effects other than competitive pressures: i) changes in the effective price of quality as induced by competition, and ii) changes in the extent of Tiebout bias. Given these considerations, the sign of the coefficient β_N cannot be determined a priori.

3 Research design

Because the different views on the concentration / stratification / expenditure relationship share the same theoretical basis, empirically evaluating these claims should in principle be straightforward. Credible analysis is difficult, however, because of the endogeneity of district structure, i.e, areas' unobserved characteristics may affect the number of districts they choose to have, at the same time as they affect outcomes like private enrollment or expenditure levels.

More formally, suppose one considers how relevant district-level characteristics are related to district concentration in different areas. Using the notation introduced above (and assuming $p = 1$ for simplicity), one could use an equation of the form:

$$q_j = \beta_0 + \beta_y \tilde{y}_j + \beta_N N_j + \epsilon_j \qquad (11)$$

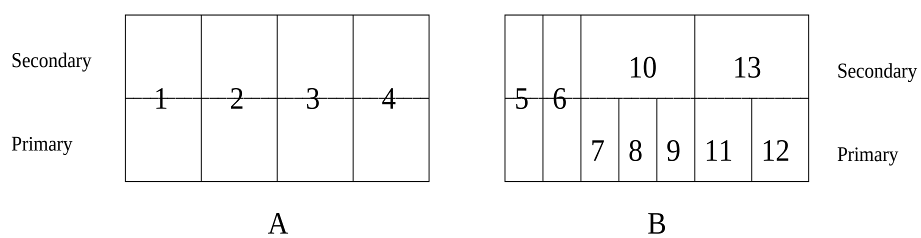
The dependent variable in this equation is expenditure, but it could also be a measure of Tiebout stratification or private enrollment, as will sometimes be the case below. Empirically, the difficulty arises because the estimate of β_N will be unbiased only if $E(N_j, \epsilon_j) = 0$.

As discussed, Borland & Howsen (1992) and Hoxby (1997) use an instrumental variables approach to address this problem. They derive arguably exogenous variation in N_j using variables Z that satisfy $E(Z_j, \epsilon_j) = 0$ and $E(Z_j, N_j) \neq 0$.

The alternate strategy used here is based on comparisons of outcomes when the *same* population is divided into different numbers of districts. This is feasible because while most MA's contain consolidated districts operating at both the primary and secondary level, roughly one third also contain some that specialize in only one of these.[13] This section explains that if primary and secondary are viewed as distinct markets, using educational-level specific data allows one to gain information from how concentration and outcomes vary *between levels, within areas.*

These observations are illustrated in Figure 2 (page 13), which compares two hypothetical MA's. Area A has four consolidated districts operating at both educational levels. Area B contains consolidated districts as well (5 and 6), but also has five primary-only and two secondary-only districts. The primary-only districts "feed" their students to the secondary-only ones.[14]

Figure 2: Hypothetical district structure



This figure suggests three modifications to equation (11), two related to measurement issues and one to the identification strategy

[13]In this paper, the term "consolidated" will be used to refer to districts that operate schools in *both* educational levels. Additionally, the term primary generally refers to grades K-8, and secondary to 9-12. These definitions are expanded on below.

[14]In California, for instance, the name of each district indicates its type. Districts like 7 are called "Elementary", those like 10 are labeled "Union High School", and those like 5 are called "Unified".

used. First, to the extent that primary and secondary are distinct markets, outcomes at each level should be related to district concentration at that level, rather than to the aggregate MA measurement. This is relevant because area B residents, for instance, have a greater degree of choice at the primary than at the secondary level. In using aggregate data, previous work may have mismeasured N_j.

There is evidence that market participants do indeed view these educational levels as distinct markets with different competitive environments. In an extensive study of catholic schools, for instance, Bryk, Lee & Holland (1993) report that the Church uses different types of institutions to serve them: primary schools are generally smaller and run by parishes, while high schools are larger and governed by religious orders or archdioceses. These responsibilities almost never overlap.

Additionally, Figure 2 suggests the need for level-specific information in the dependent variables of equation (11). To illustrate why this matters, it is useful to consider differences in costs across levels. Specifically, primary generally requires less resources than secondary schooling, partially because it entails less specialized instruction and lower overhead expenditures.[15] This is illustrated in the first three rows of Table 1 (page 14), which show that average student/teacher ratios are significantly lower in the secondary sector. Not surprisingly, average total and current expenditures are significantly higher.

Table 1: Mean inputs and private enrollment by educational level

	Primary	Secondary
District student/teacher Ratio	19.8	16.1
District total expenditures	\$5,830	\$7,911
District current expenditures	\$3,312	\$4,736
MA-level % private enrollment	13.8	7.3

Source: Based on districts in MA's as computed using the Census of Governments (1992) and the Common Core of Data (1993).
Note: These figures are based on specializing districts only.

These differences are also reflected in the private sector, where average tuition is significantly higher at the secondary level. If only

[15] A first-grade teacher, for instance, can carry out most instruction in a single classroom. Past the eighth grade, subjects like physics and vocational education require specialized instructors and infrastructure.

from a price effect, therefore, private enrollment, another dependent variable frequently used in the literature, can be expected to be lower at this level. As shown in Table 1, the average private enrollment ratio is almost twice as large at the primary level. Among roughly 300 MA's analyzed below, none display the opposite pattern.[16]

In terms of specification (11), using level-specific data data requires the introduction of a level subscript, $l = p, s$, for primary and secondary, respectively.

$$q_{jl} = \beta_0 + \beta_y \tilde{y}_j + \beta_N N_{jl} + \epsilon_{jl}. \tag{12}$$

For regressions at the MA level, for instance, this means there will be two observations in each area, one at each level. In (12), the MA socioeconomic characteristics, $\tilde{y}_j$, do not vary between levels.

In addition, as a control for constant between-level differences in dependent variables (such as those in Table 1), some regressions below add a dummy variable, D_s, when observations are at the secondary level:

$$q_{jl} = \beta_0 + \beta_y \tilde{y}_j + \beta_N N_{jl} + \beta_s D_s + \epsilon_{jl}. \tag{13}$$

Thus far, this discussion has emphasized adjustments related to the use of level-specific data. These adjustments, which could be easily incorporated in previous research designs, turn out to be highly significant, revealing and controlling for systematic between-level differences that previous research ignored. These conceptually simple changes in the data used are sufficient to generate several of the significant results presented below.

The district structure illustrated in Figure 2, however, also makes feasible an alternate identification strategy, based on comparisons of how outcomes vary as the number of districts changes between levels, within areas, *while holding MA characteristics constant.* Formally, this is accomplished by introducing MA fixed-effects in (11):

$$q_{jl} = \beta_0 + \beta_N N_{jl} + \beta_s D_s + \sum_{j=2}^{J} \beta_j Z_j + \epsilon_{jl}. \tag{14}$$

where Z_j are MA dummies, and the MA socioeconomic characteristics, $\tilde{y}_j$, "fall out". To see the relevance of this, suppose one is

[16] If private enrollment levels are at least partially a proxy for competitive conditions, as suggestively explored by Hoxby (1994), then these differences also point to the importance of using level-specific data in the dependent variables.

concerned that lack of data on religious affiliation biases inferences on the link between concentration and private enrollment. Because an MA's religious composition will be constant across educational levels, the strategy can control for this even in the absence of precise data. The same is the case regarding other characteristics that will be constant across levels and within areas, like state-level school finance policies.

Formally, the key parameter estimate, β_N, will be unbiased if

$$E(N_{jp}, \epsilon_{jp}) = E(N_{js}, \epsilon_{js}), \tag{15}$$

i.e., as long as the correlation between district concentration and unobserved characteristics is the same at both educational levels. To illustrate using private enrollment again, this condition might not hold if households in a given MA have a stronger preference (*relative to households in other MA's*) for religious schooling at the primary level, but have a weaker preference (again relative to households in other MA's) at the secondary level.

For further illustration on the variation used in this setting, Table 2 (page 17) presents a sample of real MA's, including areas like A and B. Columns (4) and (5) contain the total number of districts operating at the primary and secondary levels, respectively. In the complete sample, roughly one third of MA's exhibit differences in the number of districts between educational levels. These are not randomly distributed across the country. One third of those with differences are at least partially in California or New York.[17] Two thirds are at least partially in either of these two states or Washington, New Jersey, Texas, Oregon, Massachusetts and Illinois, with the rest being in 16 other states.

As stated, this suggests that while the research design introduced here allows significant controls for MA-level heterogeneity, it does not substitute for experimental evidence, since the differences in the number of districts between levels, at least at a geographical level, are not randomly assigned. In other words, one could object to the strategy proposed on the grounds that the fact that certain areas do display these between-level concentration differences, could be correlated with situations where condition (15) would not hold.

[17] MA's are defined as collections of counties (except in New England), so they often cross state lines.

Table 2: School districts of different types in selected MA's

Metropolitan Area	State	Primary only districts (1)	Secondary only districts (2)	Consolidated districts (3)	Total prim. (4)	Total sec. (5)	Ratio (4)/(5) (6)
Kenosha	WI	10	2	1	11	3	3.67
Portsmouth	NH–ME	21	1	7	28	8	3.50
Yuma	AZ	7	2	0	7	2	3.50
Santa Cruz	CA	8	1	3	11	4	2.75
Burlington	VT	12	1	7	19	8	2.38
Joliet	IL	25	6	8	33	14	2.36
San Diego	CA	27	5	12	39	17	2.29
Atlantic City	NJ	8	2	4	12	6	2.00
Victoria	TX	2	0	2	4	2	2.00
Nashville	TN	3	0	8	11	8	1.36
Springfield	MA	12	3	23	35	26	1.35
Vancouver	WA	2	0	7	9	7	1.29
Nassau–Suffolk	NY	27	0	99	126	99	1.27
Altoona	PA	0	0	7	7	7	1.00
Baltimore	MD	0	0	7	7	7	1.00
Bradenton	FL	0	0	1	1	1	1.00
Bristol	CT	0	0	4	4	4	1.00
Clarksville	TN–KY	0	0	2	2	2	1.00

Source: Common Core of Data, 1990.

This may not be such a significant consideration, however, mainly because between-level differences in the number of districts, when they exist, seem to be due to institutional development that took place before this century. For instance, in southern states, county governments have a relatively wide range of responsibilities, which have historically included education. As a result, they often run single, consolidated school districts, so there is no scope for districts specializing at either level. In other states with many specializing districts (e.g. Montana), tax limitations encouraged the formation of such districts to raise enough funds to cover K-12 instruction.[18] Thus, it is possible to view between-level differences in district availability as a feature by which past institutional development endowed some areas with different degrees of choice at different educational levels. Current data will reveal how households in these areas have adapted to this situation.

Additionally, it is relevant that while the total number of school districts in the U.S. declined significantly in the first half of the cen-

[18] See American Association of School Administrators Commission on District Reorganization (1958).

tury, it has been relatively stable since the early 1970's (see Kenny & Schmidt (1994)).

Beyond this issue, there are other potential problems with this research design. One is that district structure is generally more complicated than Figure 2 suggests. For instance, this paper takes grades K-8 as primary and 9-12 as secondary, but the "break" in some states is not between the 8th and 9th grade, which in itself can introduce biases. Additionally, there are areas that do not contain secondary only-districts, so that students from primary-only districts "feed" into consolidated ones. Restricting the sample to MA's with "simple" structures that resemble those in Figure 2, however, does not qualitatively affect the results described below.

Additionally, the results presented or discussed below cover two samples: MA's in all the U.S. and MA's in California. This is useful because California's district structure is simple and actually resembles Figure 2. Additionally, the state is large enough to provide adequate samples, but small enough to describe in some detail what the sources of variation used are. Additionally, institutional factors are held constant. Table 4 in the appendix (page 38) presents relevant summary data on the 23 MA's in this state. For background information on both samples, Table 3 (page 18) presents summary information on school district sizes.

Table 3: Enrollment size by type of district

District type	No. of districts	Mean enrollment	Percentile of enrollment: 10th	25th	50th	75th	90th
California sample:							
Primary only	468	2,127	89	220	815	2,451	5,856
Secondary only	83	4,541	422	875	2,690	6,929	10,511
Consolidated	249	12,988	576	2,204	6,349	14,853	25,395
All	800	5,758	132	404	1,837	5,931	13,184
U.S. sample:							
Primary only	1,601	1,224	74	177	470	1,228	2,843
Secondary only	354	2,260	288	551	1,081	2,423	5,797
Consolidated	4,397	6,293	645	1,230	2,449	5,186	12,403
All	6,352	4,790	239	691	1,746	3,984	9,680

Source: Common Core of Data, 1990.

4 Data

The basic source of school district information is the National Center for Education Statistics' Common Core of Data (CCD) for 1990. The CCD includes information on districts' geographic location, grade levels of operation, and administrative characteristics.[19] The main source of socio-economic and demographic information is the School District Data Book (SDDB), which allows access to district-level tabulations of the 1990 Census. MA-level controls come from the regular Census Summary Tape Files (in this case, STF3C-1990).[20]

Using these inputs, the central data innovation in this paper is the construction of educational level-specific information. To illustrate some aspects of this process, it is useful to reconsider Figure 2 (page 13). Some types of information (e.g. administrative) is collected directly from districts, and can be used in a straightforward manner, taking into account the level at which each district operates. For instance, enrollment information for a district like 7 is assigned to the primary level. In the case of a district like 5, only the enrollment in grades 1-8 is counted as primary.

The richer type of census-based information is collected from households: the Census Bureau essentially "translates" the data from its usual geographies (like tract or place) into those given by districts' boundaries. In the case of an area like A (Figure 2), districts offer a complete set of geographical divisions, and information is relatively easy to extract and interpret. Among districts 11-13 in Area B, however, there is geographic overlap.

Using the SDDB, however, it is still possible to obtain level-specific information. This is done by specifying the *universe* under which data are extracted as "persons enrolled in public school" *in the appropriate grade levels*. To illustrate, suppose one is interested in racial composition. For districts 11 and 12, the universe can be specified as "persons attending public school in grades 1-8". Then, for the district 13 geography, "persons enrolled in public school in grades 9-12" is specified.[21] Similarly, if one specifies the geography of

[19] Part of the administrative information in the CCD comes from the Census of Governments.

[20] The controls used have a straightforward interpretation. An exception is the proportion catholic, proxied using the proportion of individuals of French, French-Canadian, Irish, Italian, Polish, Portuguese and Spanish ancestry (the proportion Hispanic enters as a separate independent variable).

[21] An important aspect of this procedure underlines its differences with the usual administrative district-level information. Namely, the census counts all children enrolled in public

district 12, selects the universe of children enrolled in public school, and chooses data on household characteristics, information on these children's households will be obtained.

Because of the need for level-specific data, the input measure used below is not expenditure per-pupil but the teacher/student ratio. This is because the former cannot be disaggregated for consolidated districts, while the latter is available on an educational level-specific basis. Furthermore, the teacher/student ratio is a widely used input measure, and has in fact been the focus of a number of recent state and federal spending initiatives.

A final difficulty arises because secondary does not begin with the ninth grade in all states, as the discussion of Figure 2 suggested. In some cases, secondary will begin at the eighth or even seventh grade. This would not be a problem if *all* data on districts were available on a grade-specific basis, but this is not always the case.

This issue affects the results on input-levels presented below (*not* those on Tiebout sorting). The difficulty arises not with the classification of students (enrollment is always available on a grade-specific basis), but with that of teachers. The data sets include the number of primary and secondary-level faculty, but do not specify the grades at which these levels begin and end. To obtain data on this separation, the Education Departments of all states were contacted. The problem is that for a few states the separation point was not clear, and/or a response was not obtained.

Several tests of the importance of this difficulty were implemented. The results found are not sensitive to selecting particular samples that abstract from the problem. For instance, a subsample of MA's in states with uniform definitions that have secondary always beginning in the ninth grade was used, and the results are close to those obtained using the full sample. Nevertheless, these data difficulties suggest that of all the results presented, it is those on input levels that should be viewed with greater caution.

A final note is that not all MA's are included in every analysis. This is because for some indicators, the SDDB does not contain information for a significant number of districts. Conversations with Census Bureau officials indicate the reason for this is that districts in these areas were not mapped in time for the SDDB project.

school who reside in a given district. This does not necessarily mean they are *enrolled* in that district. In practice, it is rare for students to attend public school in districts different from those in which they live.

5 Results

District concentration has a theoretically ambiguous effect on areas' educational expenditure. To explore it, most previous research has focused on a supply side approach suggesting that, all else equal, areas with more districts will display lower expenditures. Research that stresses the effects of school district structure on demand, however, sometimes suggests the opposite tendency. *This prediction rests on the existence of Tiebout stratification.*

To analyze these issues, this section first presents evidence on private enrollment, one of the few areas where theoretical predictions coincide. Because determining the prevalence of stratification is a key step in the analysis, it then presents evidence on Tiebout sorting. Finally, the impact of district availability on input levels is considered.

5.1 Private enrollment

Private enrollment is one channel through which district availability might positively impact expenditure. Unusually, this is a case in which supply (efficiency) and demand (stratification) effects should work in the same direction: all else equal, areas with more districts should have lower private enrollment. Surprisingly, there has also been mixed evidence in this area, with some studies finding either insignificant or counterintuitive results.

Tables 5 and 6 (pages 39 and 40) present regressions of MA-level private enrollment ratios for the US and California samples, respectively. Columns (1) and (2) present what will be labeled "cross sectional" evidence: i) the dependent variable is the *aggregate* private enrollment ratio in the MA, *without distinguishing between educational levels*, and ii) the independent variable is based on the *total* number of districts in each MA.

While the coefficient of interest is of the expected sign and significant in (1), it becomes insignificant when basic controls are added in (2). In the California sample, it counterintuitively suggests areas with more districts will have higher private enrollment.

Columns (3)-(6) use level-specific data: they incorporate two observations for each MA, one at each educational level, with each regressed on the number of districts per student operating in the corresponding level. Such data has rarely been used before, in part because few data sets permit educational level distinctions with significant geographic detail.

Using level-specific data, column (3) regresses private enrollment on a constant and the number of districts at each educational level. When regression (4) adds some controls, the coefficient is counter to expectation and significant in both samples.

As mentioned above, however, in analyzing outcomes like private enrollment or input levels, it is necessary to control for differences these dependent variables display across educational levels. In the present case this is necessary because, for instance, average tuition is higher at the secondary level, so that enrollments might be expected to be lower at that level. Beyond this price effect, other aspects of secondary education may lower parents' propensity to use private schools. To illustrate, beginning with the secondary grades, schools may intensify the separation of students into different groups according to "ability" levels (e.g., Advanced Placement, vocational education). To the extent these procedures separate students by socioeconomic characteristics, they may reduce the advantages of private schools as far as parents are concerned.

By including a dummy equal to one for observations at the secondary level, regression (5) introduces a rough control for these differences.[22] This suggests a number of observations:

- The coefficient on this variable is economically and statistically significant, reflecting the fact that in all MA's for which this study has data, private enrollment is lower at the secondary level.
- The coefficient is robust with respect to different combinations of control variables, and its value is similar in both samples. This is consistent with the possibility that, after controlling for district concentration, differences in private enrollment *between levels* arise from differences in tastes and technology that are constant across MA's.
- Its inclusion changes the sign of the coefficient on the number of districts per student, which is now of the expected sign and significant at the five percent level.

[22]Note that this type of control was not used in the stratification analyses above. This reflects that to the extent that stratification is a measure is a just a measure of districts composition, it is comparable across educational levels. This is not the case for outcomes like private enrollment or expenditure, addressed below.

- It also affects the regressions' fit significantly: including this dummy raises R^2 from 0.26 to 0.60 in the U.S. sample, and 0.63 to 0.89 in the case of California.[23]

Once again, simply taking into account differences between levels and using appropriate data, produces results consistent with Tiebout's framework.

Finally, column (6) introduces the most controls by adding a dummy variable for each MA, so that the effect of changes in district concentration is identified only off changes between levels, within MA's. Taking MA-level heterogeneity into account strengthens results in the expected direction. The largest effect is for California, where a one standard deviation increase in the number of districts per student in associated with a decline in the private enrollment level of one third of a standard deviation. This is equivalent to a reduction of about 1.5 percentage points of private enrollment, a significant decline relative to mean private enrollment of 9.3 percent.

In all regressions, control variables enter with effects similar to those found in previous studies. The proportion of the population black, and the percentage catholic, positively affect private enrollment. The proportion Hispanic, as in other studies, does not have a consistent effect.

5.2 Tiebout sorting

The results on private enrollment could be the consequence of either the competition or stratification (or both) aspects of a Tiebout system's operation. Addressing this ambiguity, this section focuses directly on the presence of Tiebout sorting. To make results comparable with the previous literature, it relies on a number of stratification measures at the MA and district level. These measure sorting

[23] These points still hold in regressions that include the secondary level dummy but no controls:

	U.S. sample	California sample
Number of districts per student at given educational level	-10.7***	-3.0***
	(3.6)	(0.9)
Secondary level observation dummy	-7.8***	-6.7***
	(1.3)	(0.4)
R^2	0.476	0.347

As evident, the coefficients are of the expected sign and significant, and the R^2 measures move as predicted.

in public schools along: students' household income, their race, and the educational attainment of their household heads.

5.2.1 Income stratification

To explore income stratification, some previous research uses the Theil entropy measure of inequality, decomposing total MA inequality into the sum of *between*-district and *within*-district components.[24] Using this measure, Eberts & Gronberg (1981) focus on 33 MA's, using the ratio of within-district to total inequality as the dependent variable in a regression on the number of districts. The expectation from Tiebout's framework is that as the number of districts increases and these become more homogeneous, the proportion of inequality attributable to the within-district component should decline.

Table 7 (page 41) provides evidence on 266 MA's. Columns (1) and (2) use "cross-sectional data": the regressions they contain make no distinction between educational levels, and the independent variable is based on the *total* number of districts in each MA. These two columns replicate regressions of the type Eberts and Gronberg estimated.

Column (1) presents the simplest specification. The coefficient is of the expected sign (suggesting increased district availability is associated with greater district homogeneity) but not statistically significant. The number in brackets indicates a one standard-deviation increase in the number of districts per student reduces the percentage of total inequality attributable to the within-district component by roughly one tenth of a standard deviation.[25] In column (2) the

[24] Bourguignon (1979) shows that this is the only measure that can be satisfactorily decomposed into within and between-group contributions:

$$\sum_{i=1}^{M} Y_i \; log \frac{Y_i}{1/M} = \sum_{g=1}^{G} Y_g \; log \frac{Y_g}{M_g/M} + \sum_{g=1}^{G} Y_g \sum_{i \in S_g} \frac{Y_i}{Y_g} \; log \frac{Y_i/Y_g}{1/M_g} \qquad (16)$$

where

M: number of households in the MA,
G: number of districts in the MA,
Y_i: household i's share of total income in the MA,
Y_g: district g's share of total MA income,
N_g: number of households in district g,
S_g: set of families in district g.

The left hand term is total inequality, the first right hand side expression is the between-group component, and the second the within-group component.

[25] Though the results are not presented here, when the number of districts is used as the

coefficient is of the expected sign and significant, but as the change between columns (1) and (2) suggests, this result is sensitive to the specification used, as seems to have been the case in previous research.

Columns (3)-(5) implement the empirical strategy outlined above. Regressions now distinguish between educational levels, that is: i) they include two observations for each MA, one at each educational level, and ii) the dependent variable is regressed on the appropriate level-specific number of districts per student.[26] This explains the change in sample sizes.

Column (3) presents the simplest univariate regression. The coefficient on the number of districts is negative and significant. Column (4) adds some basic MA-level controls, and Column (5) implements the full identification strategy by adding MA dummies which absorb the control variables. In this last column, therefore, the effect of district concentration on stratification is identified off changes in district availability between educational levels, *within* MA's. Column (5) suggests that a one standard deviation increase in the number of districts per student reduces the percentage of inequality attributable to the within-district component by one fourth of a standard deviation. R^2 here is substantially higher than in any other specification.

Because these results are qualitatively similar to the rest presented in this section, it is useful to summarize their main characteristics: i) the use of level-specific information makes results uniformly consistent with Tiebout's theoretical predictions; as reflected in the previous literature, this is not always the case when purely "cross-sectional" evidence is used, ii) the use of the full identification strategy proposed in this paper (which relies only on between- level, within MA variation), stregthens results in the expected direction and improves the regression's fit significantly.

A final point is that in contrast with the private enrollment re-

independent variable, these results are close to Eberts and Gronberg's: the point estimate on the number of districts is virtually identical to their OLS specification. The normalization used here is common in subsequent work.

[26] To further illustrate using Figure 2 (page 13), there are two observations for each MA when level-specific data is used. For areas like B, the primary level observation contains data on districts 7, 8, 9, 11, and 12, and on the household incomes of children enrolled in primary in districts 5 and 6. The independent variable is based on the number of districts operating at that level. The secondary-level observations contain data on districts 10 and 13, and on the children enrolled in secondary in districts 5 and 6

gressions, a secondary dummy is never included in Table 7. This is because there is no a priori reason to believe stratification measures should be greater at either level. To illustrate, if Tiebout sorting simply did not take place, sorting measures would be the same regardless of which level one considered.

Table 8 (page 42) presents a different type of evidence on income stratification. In this case the measure is the inter- district range of the proportion of enrolled children (in each district) who come from poor households, where Tiebout's framework suggests district availability should positively affect this measure. Because this measure is concerned with the extreme districts (the best and worst in terms of poverty composition), however, it is not surprising that the effects found are greater in magnitude and uniformly positive. Further evidence of this type is not presented for reasons of space. However, robust results were also found with respect to the range of the proportion of children from households receiving public assistance income.

5.2.2 Racial stratification

Other research has focused on how district concentration affects racial homogeneity. Table 9 (page 43) focuses on this issue, where the ethnic origin groups considered are white, black, Asian, hispanic, American Indian, and other. The dependent variable, now at the district level, is the one used by Hoxby (1997), the ratio of each district's racial composition Herfindahl index to its MA's analogous measure.[27] The expectation from Tiebout is that increases in district availability should lead to *increases* in this ratio, as districts become more homogeneous with respect to their MA. This is because in Industrial Organization terms, a higher Herfindahl index means one racial group is coming closer to "monopolizing" a district.

Columns (1) and (2) present "cross sectional" estimates. As the

[27] The dependent variable is the ratio of two Herfindahl indices. Suppose district d is situated in MA m, and i indexes racial groups, of which there are N. If s_{id} is the share of enrollment corresponding to group i in district d, and s_{im} is the analogous measure for the MA, then the dependent variable is given by:

$$\frac{\sum_{i=1}^{N} s_{id}^2}{\sum_{i=1}^{N} s_{im}^2}.$$

If district availability did not lead to racial sorting, this measure would always be equal to one.

lower part of the table indicates, the regressions consider 4,994 districts in 267 MA's. In these regressions the coefficient of interest is either insignificant or contrary to the theoretical expectation.

Columns (3)-(5) implement the research design introduced in the paper: i) they include district-level information (two observations are included for consolidated districts), and ii) the dependent variable is regressed on the appropriate educational-level specific number of districts per student.[28]

Columns (4)-(5) contain results consistent with the theory. In the final case the coefficient of interest is statistically significant at the 1 percent level, and the fit once again improves significantly. Together, the imply that the presence of more districts is associated with increases in the racial homogeneity of their enrollments (relative to the racial composition of their respective MA's), as predicted by Tiebout's framework.

5.2.3 Stratification by educational attainment

Table 10 (page 44) presents similar district-level evidence on the educational attainment of students' householders. Instead of racial groups, this data considers four schooling categories: high school dropouts, individuals with high school degree but no college, householders with some college but no degree, and college graduates.

The results are broadly similar to those found above, mixed results emerge from the "cross-sectional" specifications, while those arising from level-specific data are consistent with the theory. In particular, the use of MA dummies, which fully implements the identification strategy introduced in this paper, produces the most statistically and economically significant estimates.

5.3 Results on input levels

The previous sections have shown that the sorting and private enrollment outcomes predicted by Tiebout are a significant feature of the data used here. As discussed above, when these characteristics are present, district availability may in fact lead to *greater* educational expenditure. To explore this possibility, this section presents

[28] To illustrate using Figure 2 (page 13), columns (3)-(6) include two observations for districts like 5. The dependent variable is calculated twice, each time using the enrollment at the appropriate educational level.

regressions on the relationship between district concentration and the teacher/student ratio at the MA and district level. This ratio is widely used as an input measure, and has the benefit of being available by educational level. Additionally, increases in the measure intuitively correspond to increases in educational expenditure.

5.3.1 Average input levels

Table 11 (page 45) analyzes *enrollment-weighted* average teacher / student ratios in 265 MA's. The first two columns once again present cross-sectional evidence, that is, they do not distinguish between educational levels in the construction of the dependent or independent variables. In both cases, the coefficient of interest suggests a larger number of districts is associated with an increase in the teacher/student ratio. These effects are relatively small: the numbers in brackets suggest an increase of one standard deviation in the number of districts per student leads to an increase of about one tenth of a standard deviation in the dependent variable. This corresponds to reducing class size from approximately 17.8 to 17.6 students.

Columns (3)-(6) gradually implement the research design introduced in this paper. All of them contain level-specific data: there are two observations for each MA, one at each educational level. Columns (3) and (4) repeat a simple "cross-sectional" specification. Here the point estimates suggest greater district availability reduces input levels, but the results are not significant.

As in the private enrollment regressions, column (5) introduces a dummy variable for observations at the secondary level, a rough control for constant between-level differences in the teacher/student ratio. As before, this raises several observations.

- The coefficient on this variable is economically and statistically significant, reflecting that teacher/student ratios are almost always higher at the secondary level. The point estimate suggests that relative to the constant, there are about 4 fewer students per teacher at the secondary level, which is consistent with the descriptive statistics for specializing districts presented in Table 1 (page 14).
- The coefficient is stable with respect to including different combinations of control variables. This is consistent with the differ-

ences in input levels between levels arising from technological differences that are constant across MA's.

- Its inclusion makes the coefficient on the number of districts per student positive and significant, suggesting district availability raises expenditure levels.
- It also affects the regression's fit significantly: simply including the secondary level dummy increases R^2 from 0.05 to 0.37 between columns (4) and (5).[29]

Finally, Column (6), which includes MA dummies, implements the full identification strategy. The coefficient of the secondary-level dummy is virtually unchanged from the previous specification, but the effect of district availability increases significantly. The figure in brackets suggests a one standard deviation in the number of districts per student leads to an increase in the teacher/student ratio of about one half a standard deviation. This corresponds to reducing the number of students per teacher from about 16 to 14.

To summarize, these results suggest greater district availability is in fact associated with higher educational expenditure. They also confirm that the results presented arise not only from the new identification strategy used, but also from the data improvements the paper introduces.

5.3.2 District-level inputs

As discussed above, Borland & Howsen (1992) and Hoxby (1997) analyze district rather than MA-level expenditures. Tables 12 and 13 (pages 46 and 47) present this type of information. Both tables are exactly alike except for the measure of district concentration used. The first focuses on the number of districts per student as the independent variable of interest. Mirroring earlier work, the second focuses on the enrollment Herfindahl index.

Columns (1) and (2) again implement "cross sectional" regressions. Though the results are not robust, it is interesting that the specifications using the enrollment Herfindahl index (Table 13) produce negative and significant point estimates. These suggest, as has some previous research, that districts in areas with greater availability spend less resources.

[29] A regression with only a constant, the number of districts per student, and secondary level dummy still produces an R^2 greater than 0.32. As was the case with private enrollment, in this simple specification the coefficients of interest are very similar to those in (5).

Columns (3)-(6) introduce level-specific data: there are now two observations for consolidated districts, one at each educational level. Columns (3) and (4) do not control for differences between levels and produce mixed results. This changes with Column (5), where the introduction of a secondary level observation dummy again changes the coefficient on the number of districts per student to be positive in both specifications, though only significant when the number of districts per student is used as the independent variable. As was the case with the MA-level data, the fit climbs significantly simply with this distinction (from about 0.08 to 0.25 in both tables). As before, this coefficient is stable with respect to changes in the combinations of control variables used.

Column (6) introduces the full identification strategy by featuring MA dummies. This causes the MA-level controls to drop out. The district-level controls remain and are significant, generally in the expected direction. The proportion of the population with a college degree tends to raise input levels, while the proportion non-white tends to lower them. An interesting result is that the proportion in poverty tends to raise spending, possibly reflecting compensatory policies.

As with the MA-level data, this step increases the economic and statistical significance of the coefficient on the concentration measure. It suggests that a one standard deviation increase in the number of districts per student raises the teacher/student ratio by roughly a quarter of a standard deviation, with this effect being similar in both cases.

To summarize these district-level results, when the number of districts per student is used as the key independent variable, these results mirror those for MA-level data: i) they suggest greater district availability in fact leads to greater expenditure, and ii) the source for this finding is as much the "data improvement" brought about by controlling for between-level differences as the identification strategy per se. When the independent variable is the enrollment Herfindahl index, the full identification strategy is needed to produce these results.

5.4 Some further tests

The main arguments underlying the presentation of results thus far has emphasized the following logic:

- Tiebout sorting is a significant feature of local educational provision.
- Its presence and the demand side mechanisms it induces - including effects on private enrollment - may account for the fact that greater district availability seems in fact to be associated with *greater* educational input levels.
- Two factors account for why previous research has often found the opposite associations:
  - By focusing on aggregate data, earlier work did not distinguish between educational levels. In areas where there are districts specializing in *only* one of these levels, this biases the measurement of key dependent (input) and independent (district concentration) variables. Part of this bias is due to the fact primary-level education on average is less costly, and that primary private enrollment levels on average are lower.
  - The endogeneity of district concentration biased studies based on purely cross-sectional evidence.

Focusing on these last two factors, it is possible to introduce some simple tests of this logic. The following subsections present two tests that can be derived from these observations.

5.4.1 Results using areas that contain only consolidated districts

The first test is based on the fact that in MA's that contain *only* consolidated districts (those that cover both primary and secondary), level controls should not be necessary to obtain the positive association between district availability and input levels (found above in all regressions that include such controls). This is because in these areas biases arising from level aggregation should affect all districts equally. In contrast, in areas that do contain specializing districts, one district may have a lower teacher/student ratio simply because it is a primary-only district.

This prediction is confirmed in Table 14 (page 48), which presents MA and district-level results for MA's that contain consolidated districts *only*. This table does not include level-specific data or "fixed effect" results with MA dummies (these last are not feasible, since there is no between-level variation in district concentration). The first two columns present MA-level regressions, while the last two contain district-level results. The control variables used in regressions (2) and (4) are not displayed.

As expected, the effect of district availability on the teacher/student ratios is positive and significant, at least at the 5% level, throughout. This same result was found when the Herfindahl index was used as key independent variable. Also as expected, results using areas which contain specializing districts (not shown) display an opposite tendency. Further results are not included here for reasons of space, but a similar pattern was found using stratification measures as well.

5.4.2 Effects in states with strong equalization measures

Another test starts from the observation that demand (and supply) side effects of district availability on input levels should not be significant in states with rigid equalization measures. The clearest example of this is California, although there are other states with such measures as well.

Table 15 (page 48) presents the evidence for California. As expected, the effect of district concentration on MA average input levels is never significant. The result remains independently of whether district rather than MA-level data is used, and when the enrollment Herfindahl index rather than the number of districts per student is used as the independent variable.

As indicated above, however, private enrollment and Tiebout sorting results *are* significant for the California sample. An implication is that even if rigid school finance reforms are effective at equalizing financial inputs, they may have a significantly smaller impact on important non-financial inputs, such as peer group composition. The ability to influence such factors may still provide households with sufficient incentives to sort, Tiebout style.

6 Conclusion

Concerns about public school quality have prompted calls for increased choice as a means to improve education. These have also renewed interest on how inter-district competition affects efficiency, since as Hoxby (1997) argues, observing this relation may be the best way of gaining information on the effects of incentives on educational productivity.

Several results in this area suggest a significant efficiency effect: areas with greater district availability may spend less resources without necessarily sacrificing educational outcomes.

As the school finance reform literature emphasizes, however, local public finance mechanisms affect expenditure not only through supply but also demand channels. Centralization may lower the income and educational spending preferences of hypothetical pivotal voters, so if households in fact segregate into districts based on demand, greater district availability may be associated with *higher* input levels. Further, by affecting private enrollment, district concentration influences the mix of households that actually use the public sector, reinforcing the previous effect.

In sum, both literatures agree expenditure levels are unlikely to be neutral with respect to district structure, but their implications as to the direction of the effect are opposite, and the aggregate effect is uncertain. The endogeneity of school district formation complicates any evaluation of these claims, which partially explains the mixed evidence that surrounds them.

This paper relies on two innovations to present evidence on these issues. The first is simply the introduction of data that distinguishes between educational levels, and can therefore control for systematic differences between the primary and secondary sectors. This straightforward adjustment is in many cases sufficient to generate significant results which, where applicable, are consistent with theoretical predictions.

Additionally, the paper introduces a new identification strategy which relies on within-area, between-level changes in district concentration. Despite not being a source of exogenous variation, this strategy has the advantage of introducing extensive controls for MA-level heterogeneity. When applied and where applicable, it further moves results in directions consistent with Tiebout's framework.

These two elements suggest that the demand mechanisms emphasized by the School Finance Reform literature are significant. Even in the case of California, where the financial incentives for sorting have been blunted, there is evidence of significant stratification by income, race, and householders' educational attainment. Additionally, district concentration was found to significantly affect private enrollment.

The presence of these demand mechanisms may account for the fact that these results also suggest greater district availability is associated with greater educational expenditure, at least as indicated by teacher/student ratios.

To summarize, this paper makes four contributions to the literature: i) it emphasizes (though this is not an original claim) that the theoretical effects of district structure on input levels are ambiguous, ii) it highlights that not making distinctions between educational levels not only ignores significant and systematic differences between them, but may also lead to mismeasurement of key variables, iii) it suggests that Tiebout stratification is indeed a central element of school districts' operation, and iv) it advances that this may account for an observed positive relation between district availability and input levels. Because of difficulties discussed in the Data section, this last claim may be the one to take with the greater caution.

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7 Appendix

Table 4: Metropolitan areas and districts in California

	Metropolitan Area	Total	Primary only	Seconda-ry only	Consoli-dated
1	Anaheim-Santa Ana	28	12	3	13
2	Bakersfield	48	36	4	8
3	Chico	16	9	2	5
4	Fresno	42	23	7	12
5	Los Angeles-Long Beach	83	33	7	43
6	Merced	22	15	3	4
7	Modesto	30	21	3	6
8	Oakland	38	11	2	25
9	Oxnard-Ventura	21	11	2	8
10	Redding	29	25	2	2
11	Riverside-San Bernardino	58	19	4	35
12	Sacramento	58	35	5	18
13	Salinas-Seaside-Monterey	26	17	3	6
14	San Diego	44	27	5	12
15	San Francisco	45	33	5	7
16	San Jose	34	22	5	7
17	Santa Barbara-Santa Maria-Lompoc	24	17	4	3
18	Santa Cruz	12	8	1	3
19	Santa Rosa-Petaluma	42	33	4	5
20	Stockton	18	9	1	8
21	Vallejo-Fairfield-Napa	13	2	1	10
22	Visalia-Tulare-Porterville	50	39	6	5
23	Yuba City	19	11	5	3
	Total	800	468	83	249

Source: Common Core of Data, 1990.

Table 5:

Dependent variable: MA-level percentage private enrollment
Sample: all MA's

	Cross-sectional data		Level-specific data			
	(1)	(2)	(3)	(4)	(5)	(6)
Number of districts per student	-2.0**	-1.3	–	–	–	–
	(1.0)	(0.9)				
	[-0.10]	[-0.07]				
Number of districts per student at given educational level	–	–	-0.6	2.2**	-2.1**	-3.2***
			(1.1)	(1.1)	(0.93)	(1.3)
			[-0.02]	[0.08]	[-0.07]	[-0.11]
Secondary-level observation dummy					-6.6***	-6.7***
					(0.3)	(0.2)
MA population: prop. black		10.8***		14.8***	11.6***	–
		(3.5)		(3.1)	(2.5)	
MA population: prop. hispanic		-10.1*		-7.3	-8.2**	–
		(5.2)		(5.1)	(3.7)	
MA population: prop. linguistically isolated		35.8**		34.1**	30.9***	–
		(16.3)		(15.2)	(11.8)	
MA adults: prop. with college degree		2.3		1.0	0.5	–
		(3.7)		(4.1)	(2.7)	
MA population: prop. catholic		10.2***		9.8***	10.5***	–
		(2.3)		(2.2)	(1.7)	
MA population: prop. poor		-10.2		-10.7	-11.0	–
		(12.1)		(11.6)	(8.7)	
MA households: median income		2.4**		2.4***	2.4***	–
		(1.0)		(0.9)	(0.7)	
MA households: prop. on public assistance		-2.1		0.5	4.2	–
		(11.5)		(11.3)	(8.3)	
MA households: prop. with own children		-13.7**		-16.2***	-14.0***	–
		(5.7)		(5.5)	(4.0)	
MA dummies	No	No	No	No	No	**Yes**
N	292	292	584	584	584	584
R^2	0.011	0.412	0.001	0.261	0.594	0.952

*, **, *** - significant at the 10, 5, and 1 percent level, respectively.
1) Huber-White standard errors are in parenthesis.
2) Brackets contain the proportion of a standard deviation change in the dependent variable brought about by increasing the independent variable by one standard deviation.

Table 6:

Dependent variable: MA-level percentage private enrollment
Sample: California MA's

	Cross-sectional data		Level-specific data			
	(1)	(2)	(3)	(4)	(5)	(6)
Number of districts per student	-7.2**	1.1*	–	–	–	–
	(2.8)	(2.2)				
	[-0.43]	[0.07]				
Number of districts per student at given educational level	–	–	0.2	10.3***	-4.0**	-8.1***
			(2.5)	(3.2)	(1.9)	(1.7)
			[0.01]	[0.40]	[-0.15]	[-0.31]
Secondary-level observation dummy					-6.7***	-7.3***
					(0.7)	(0.1)
MA population: prop. black		38.9**		43.5*	21.3**	–
		(18.0)		(24.4)	(10.7)	
MA population: prop. hispanic		23.2**		26.0	30.1**	–
		(10.5)		(26.4)	(13.3)	
MA population: prop. linguistically isolated		11.6		13.7	10.0	–
		(15.4)		(31.4)	(16.0)	
MA adults: prop. with college degree		-5.4		-4.1	4.4	–
		(17.1)		(27.7)	(13.5)	
MA population: prop. catholic		28.9		14.7	19.8	–
		(19.1)		(22.4)	(16.8)	
MA population: prop. poor		-62.4		-78.8	-116.4**	–
		(53.7)		(76.7)	(46.8)	
MA households: median income		1.6		1.6	-1.6	–
		(2.8)		(3.7)	(2.2)	
MA households: prop. on public assistance		52.1		62.9	86.8**	–
		(39.7)		(67.8)	(37.4)	
MA households: prop. with own children		-105.3***		-104.0**	-107.0**	–
		(29.7)		(39.5)	(21.9)	
MA dummies	No	No	No	No	No	**Yes**
N	23	23	46	46	46	46
R^2	0.191	0.913	0.000	0.629	0.890	0.966

*, **, *** - significant at the 10, 5, and 1 percent level, respectively.
1) Huber-White standard errors are in parenthesis.
2) Brackets contain the proportion of a standard deviation change in the dependent variable brought about by increasing the independent variable by one standard deviation.

Table 7:

Dependent variable: percentage of within-district inequality

	Cross-sectional data (1)	(2)	Level-specific data (3)	(4)	(5)
Number of districts per student	-3.6	-7.6***	–	–	–
	(2.6)	(1.9)			
	[-0.08]	[-0.17]			
Number of districts per student at given educational level	–	–	-4.2**	-12.8***	-13.1***
			(2.3)	(1.8)	(1.4)
			[-0.08]	[-0.24]	[-0.25]
MA population: prop. black		-32.9**		-36.8***	–
		(14.6)		(10.6)	
MA population: prop. hispanic		-25.8		-27.8**	–
		(17.0)		(12.1)	
MA population: prop. linguistically isolated		-5.5		-8.4	–
		(29.0)		(20.4)	
MA adults: prop. with college degree		-19.6		-21.9**	–
		(14.7)		(10.4)	
MA population: prop. catholic		-8.9**		-9.2***	–
		(4.1)		(2.9)	
MA population: prop. poor		62.4		66.3	–
		(53.8)		(37.9)	
MA households: median income		-2.8		-2.3	–
		(3.8)		(2.7)	
MA households: prop. on public assistance		-92.0**		-87.3***	–
		(30.6)		(20.7)	
MA households: prop. with own children		24.3		25.7***	–
		(13.6)		(9.5)	
MA dummies	No	No	No	No	**Yes**
N	266	266	532	532	532
R^2	0.007	0.304	0.007	0.308	0.792

*, **, *** - significant at the 10, 5, and 1 percent level, respectively.
1) Huber-White standard errors are in parenthesis.
2) Brackets contain the proportion of a standard deviation change in the dependent variable brought about by increasing the independent variable by one standard deviation.

Table 8:

Dependent variable: inter-district range of the percentage of students from poor households

	Cross-sectional data		Level-specific data		
	(1)	(2)	(3)	(4)	(5)
Number of districts per student	32.2***	38.0***	–	–	–
	(4.8)	(5.1)			
	[0.38]	[0.46]			
Number of districts per student at given educational level	–	–	34.2***	46.0***	60.7***
			(3.8)	(4.1)	(7.6)
			[0.37]	[0.50]	[0.67]
MA population: prop. black		29.5*		40.3***	–
		(15.2)		(10.0)	
MA population: prop. hispanic		74.3**		66.4***	–
		(34.4)		(20.7)	
MA population: prop. linguistically isolated		-111.7		-79.0	–
		(86.8)		(52.5)	
MA adults: prop. with college degree		14.4		15.9	–
		(21.0)		(13.3)	
MA population: prop. catholic		-9.4		-4.1	–
		(8.1)		(5.4)	
MA population: prop. poor		-25.6		-32.7	–
		(58.6)		(38.0)	
MA households: median income		5.0		4.0	–
		(4.6)		(3.0)	
MA households: prop. on public assistance		250.7***		198.8***	–
		(54.3)		(35.6)	
MA households: prop. with own children		-7.1		-5.8	–
		(21.4)		(13.8)	
MA dummies	No	No	No	No	**Yes**
N	266	266	532	532	532
R^2	0.150	0.357	0.141	0.343	0.877

*, **, *** - significant at the 10, 5, and 1 percent level, respectively.
1) Huber-White standard errors are in parenthesis.
2) Brackets contain the proportion of a standard deviation change in the dependent variable brought about by increasing the independent variable by one standard deviation.

Table 9:

Dependent Variable: $\dfrac{\text{racial composition Herfindahl index for district } j}{\text{racial composition Herfindahl index for district } j\text{'s MA}}$

	Cross-sectional data		Level-specific data		
	(1)	(2)	(3)	(4)	(5)
MA: Number of districts per student	-0.30***	0.12**	–	–	–
	(0.08)	(0.06)			
	[-0.17]	[0.07]			
Number of districts per student at given educational level	–	–	0.06**	0.13**	0.26***
			(0.03)	(0.06)	(0.06)
			[0.03]	[0.07]	[0.13]
MA population: prop. black		2.5***		2.3***	–
		(0.4)		(0.4)	
MA population: prop. hispanic		0.3		0.3	–
		(0.4)		(0.3)	
MA population: prop. linguistically isolated		2.1		1.8	–
		(1.5)		(1.4)	
MA adults: prop. with college degree		0.8**		0.7**	–
		(0.3)		(0.3)	
MA population: prop. catholic		0.0		0.1	–
		(0.1)		(0.1)	
MA population: prop. poor		-2.3**		-2.3**	–
		(0.9)		(0.9)	
MA households: median income		-0.0		-0.0	–
		(0.06)		(0.1)	
MA households: prop. on public assistance		2.5***		2.3***	–
		(0.7)		(0.7)	
MA households: prop. with own children		-0.2		0.1	–
		(0.4)		(0.4)	
MA dummies	No	No	No	No	**Yes**
N (districts)	4,994	4,994	8,446	8,446	8,446
MA's covered	267	267	267	267	267
R^2	0.029	0.291	0.037	0.261	0.443

*, **, *** - significant at the 10, 5, and 1 percent level, respectively.
1) Huber-White standard errors are in parenthesis.
2) Brackets contain the proportion of a standard deviation change in the dependent variable brought about by increasing the independent variable by one standard deviation.
3) All regressions adjust for district clustering in MA's. See Moulton (1986).

Table 10:

Dependent Variable: $\frac{\text{educational attainment Herfindahl index for district } j}{\text{educational attainment Herfindahl index for district } j\text{'s MA}}$

	Cross-sectional data		Level-specific data		
	(1)	(2)	(3)	(4)	(5)
MA: Number of districts per student	-0.05	0.18***	–	–	–
	(0.04)	(0.04)			
	[-0.03]	[0.12]			
Number of districts per student at given educational level	–	–	0.03	0.21***	0.45***
			(0.04)	(0.04)	(0.06)
			[0.02]	[0.13]	[0.27]
MA population: prop. black		0.3*		0.3**	–
		(0.2)		(0.1)	
MA population: prop. hispanic		0.6***		0.6***	–
		(0.2)		(0.2)	
MA population: prop. linguistically isolated		-0.8		-0.7	–
		(0.8)		(0.6)	
MA adults: prop. with college degree		0.9***		0.8***	–
		(0.2)		(0.2)	
MA population: prop. catholic		-0.1		-0.1	–
		(0.1)		(0.1)	
MA population: prop. poor		-0.1		-0.3	–
		(0.6)		(0.5)	
MA households: median income		0.1		0.1	–
		(0.0)		(0.0)	
MA households: prop. on public assistance		2.0***		1.7***	–
		(0.6)		(0.5)	
MA households: prop. with own children		-0.6**		-0.5**	–
		(0.2)		(0.2)	
MA dummies	No	No	No	No	**Yes**
N (districts)	4,984	4,984	8,427	8,427	8,427
MA's covered	266	266	266	266	266
R^2	0.001	0.060	0.000	0.062	0.143

*, **, *** - significant at the 10, 5, and 1 percent level, respectively.
1) Huber-White standard errors are in parenthesis.
2) Brackets contain the proportion of a standard deviation change in the dependent variable brought about by increasing the independent variable by one standard deviation.
3) All regressions adjust for district clustering in MA's. See Moulton (1986).

Table 11:

Dependent variable: MA enrollment-weighted average teacher/student ratio

	Cross-setional data		Level-specific data			
	(1)	(2)	(3)	(4)	(5)	(6)
Number of districts per student	3.7*	5.2**	–	–	–	–
	(1.9)	(2.1)				
	[0.11]	[0.16]				
Number of districts per student at given educational level	–	–	-1.3	-0.8	5.9**	35.2***
			(3.6)	(4.1)	(2.9)	(11.8)
			[0.02]	[0.01]	[0.08]	[0.50]
Secondary-level observation dummy					0.019***	0.020***
					(0.001)	(0.001)
MA population: prop.black		0.008		0.015	0.021**	–
		(0.006)		(0.010)	(0.009)	
MA population: prop. hispanic		0.010		0.009	0.020	–
		(0.008)		(0.014)	(0.013)	
MA population: prop. linguistically isolated		-0.033		-0.078	-0.070*	–
		(0.027)		(0.060)	(0.038)	
MA population: prop. with college degree		-0.016		-0.016	-0.017	–
		(0.010)		(0.021)	(0.014)	
MA population: prop. catholic		0.017**		0.014**	0.015**	–
		(0.004)		(0.010)	(0.006)	
MA population: prop. poor		0.051**		0.062	0.067**	–
		(0.024)		(0.052)	(0.033)	
MA households: median income		0.003*		0.002	0.002	–
		(0.002)		(0.004)	(0.002)	
MA households: prop. on public assistance		-0.090***		-0.115**	-0.128***	–
		(0.025)		(0.042)	(0.037)	
MA households: prop. with own children		-0.010		-0.024	-0.029**	–
		(0.011)		(0.023)	(0.015)	
(MA population in millions)		-0.002		-0.002	-0.001	–
		(0.001)		(0.003)	(0.002)	
(MA population in millions)^2		0.000		0.000	0.000	–
		(0.000)		(0.000)	(0.000)	
MA dummies	No	No	No	No	No	**Yes**
N	293	293	586	586	586	586
R^2	0.012	0.184	0.000	0.051	0.367	0.609

*, **, *** - significant at the 10, 5, and 1 percent level, respectively.
1) Huber-White standard errors are in parenthesis.
2) Brackets contain the proportion of a standard deviation change in the dependent variable brought about by increasing the independent variable by one standard deviation.

Table 12: Dependent variable: district teacher/student ratio

	Cross-sectional data		Level-specific data			
	(1)	(2)	(3)	(4)	(5)	(6)
Number of districts per student	-7.3	12.0**	–	–	–	–
	(4.6)	(5.9)				
	[0.12]	[0.20]				
Number of districts per student at given educational level	–	–	2.0	0.6	11.5***	21.6***
			(4.5)	(3.9)	(3.7)	(3.6)
			[0.02]	[0.01]	[0.11]	[0.21]
Secondary-level observation dummy					0.016***	0.016***
					(0.001)	(0.001)
District population: prop. non-white		-0.008***		-0.007**	-0.005**	-0.005***
		(0.003)		(.003)	(0.003)	(0.002)
District population: prop. with college degree		0.008**		0.003***	0.002	0.009***
		(0.004)		(0.003)	(0.004)	(0.002)
District households: median income		0.001*		0.000	0.001*	-0.000
		(0.001)		(.000)	(0.001)	(0.000)
District population: prop. poor		0.015***		0.019***	0.026***	0.012***
		(0.000)		(.005)	(0.004)	(0.003)
MA population: prop. black		0.022**		0.024**	0.037***	–
		(0.010)		(0.011)	(0.008)	
MA population: prop. hispanic		0.024**		0.041**	0.042**	–
		(0.010)		(0.015)	(0.013)	
MA population: prop. linguistically isolated		-0.074**		-0.122***	-0.070**	–
		(0.031)		(0.040)	(0.034)	
MA population: prop. with college		-0.009		0.011	0.016	–
		(0.014)		(0.016)	(0.014)	
MA population: prop. catholic		0.030***		0.050***	0.047***	–
		(0.004)		(0.005)	(0.005)	
MA population: prop. poor		0.024		0.023	0.006	–
		(0.032)		(0.034)	(0.029)	
MA households: median income		-0.003*		-0.004	-0.005**	–
		0.002		(0.002)	(0.002)	
MA households: prop. on public assistance		-0.040***		-0.158***	-0.167***	–
		(0.015)		(0.040)	(0.032)	
MA households: prop. with own children		0.020***		-0.025	-0.027*	–
		(0.016)		(0.020)	(0.015)	
(MA population in millions)		0.001		-0.002	0.001	–
		(0.001)		(0.001)	(0.001)	
(MA population in millions)^2		-0.000		0.000	-0.000	–
		0.000		(0.000)	(0.000)	
MA dummies	No	No	No	No	No	**Yes**
N	4,150	4,150	7,220	7,220	7,220	7,220
R^2	0.014	0.203	0.000	0.084	0.254	0.413

*, **, *** - significant at the 10, 5, and 1 percent level, respectively.
1) Huber-White standard errors are in parenthesis.
2) Brackets contain the proportion of a standard deviation change in the dependent variable brought about by increasing the independent variable by one standard deviation.
3) All regressions adjust for district clustering in MA's. See Moulton (1986).

Table 13: Dependent variable: district teacher/student ratio

	Cross-sectional data		Level-specific data			
	(1)	(2)	(3)	(4)	(5)	(6)
Enrollment Herfindahl index	-0.012**	-0.009***	–	–	–	–
	(0.005)	(0.003)				
	[-0.14]	[-0.11]				
Enrollment Herfindahl index at given educational level	–	–	-0.012**	-0.008**	0.011	0.027***
			(0.003)	(0.003)	(0.006)	(0.007)
			[-0.11]	[-0.07]	[0.08]	[0.24]
Secondary-level observation dummy					0.015***	0.015***
					(0.001)	(0.000)
District population: prop. non-white		-0.010***		-0.006**	-0.006**	-0.005***
		(0.003)		(0.003)	(0.003)	(0.002)
District population: prop. with college degree		0.006**		0.004	-0.001	0.008***
		(0.003.)		(0.004)	(0.004)	(0.002)
District households: median income		0.001**		0.000	0.001*	-.000
		(0.000)		(0.001)	(0.001)	(0.000)
District population: prop. poor		0.013		0.019***	0.025***	0.012***
		(0.007)		(0.005)	(0.004)	(0.003)
MA population: prop. black		0.015		0.029***	0.022**	–
		(0.010)		(0.010)	(0.010)	
MA population: prop. hispanic		0.027**		0.043	0.042***	–
		(0.013)		(0.015)	(0.013)	
MA population: prop. linguistically isolated		-0.089***		-0.110***	-0.102***	–
		(0.032)		(0.038)	(0.037)	
MA population: prop. with college degree		-0.008		0.015	0.021	–
		(0.014)		(0.016)	(0.014)	
MA population: prop. catholic		0.032***		0.049***	0.048***	–
		(0.004)		(0.005)	(0.005)	
MA population: prop. poor		0.021		0.026	0.005	–
		(0.032)		(0.034)	(0.031)	
MA households: median income		-0.003		-0.005**	-0.006***	–
		0.002		(0.003)	(0.002)	
MA households: prop. on public assistance		-0.127***		-0.185***	-0.144***	–
		(0.031)		(0.039)	(0.032)	
MA households: prop. with own children		0.026		-0.031	-0.026	–
		(0.018)		(0.019)	(0.017)	
(MA population in millions)		0.000		-0.003*	-0.001	–
		(0.000)		(0.001)	(0.002)	
(MA population in millions)^2		0.000		0.000	0.000	–
		(0.000)		(0.000)	(0.000)	
MA dummies	No	No	No	No	No	**Yes**
N	4,150	4,150	7,220	7,220	7,220	7,220
R^2	0.020	0.188	0.011	0.088	0.254	0.412

*, **, *** - significant at the 10, 5, and 1 percent level, respectively.
1) Huber-White standard errors are in parenthesis.
2) Brackets contain the proportion of a standard deviation change in the dependent variable brought about by increasing the independent variable by one standard deviation.
3) All regressions adjust for district clustering in MA's. See Moulton (1986).

Table 14: Dependent variable: district teacher/student ratio

	MA data		District data	
	(1)	(2)	(3)	(4)
Number of districts per student	8.8***	7.0**	4.8**	11.0**
	(3.4)	(2.8)	(2.4)	(4.4)
	[0.25]	[0.20]	[0.08]	[0.17]
Control Variables	No	Yes	No	Yes
MA dummies	No	No	No	No
N	178	178	1,520	1,520
R^2	0.063	0.220	0.005	0.160

*, **, *** - significant at the 10, 5, and 1 percent level, respectively.
1) Huber-White standard errors are in parenthesis.
2) Brackets contain the proportion of a standard deviation change in the dependent variable brought about by increasing the independent variable by one standard deviation.
3) Regressions (3) and (4) adjust for district clustering in MA's. See Moulton (1986).

Table 15: Dependent variable: district teacher/student ratio.

	Cross-sectional data		Level-specific data			
	(1)	(2)	(3)	(4)	(5)	(6)
Number of districts per student	2.0	0.2**	–	–	–	–
	(4.9)	(3.2)				
	[0.12]	[0.13]				
Number of districts per student at given educational level	–	–	-3.1	-8.5	-0.7	-0.0
			(3.0)	(4.9)	(3.8)	(2.1)
			[0.16]	[0.44]	[0.06]	[0.00]
Secondary-level observation dummy					0.013**	0.012**
					(0.006)	(0.006)
MA dummies	No	No	No	No	No	**Yes**
N	21	21	42	42	42	42
R^2	0.057	0.820	0.026	0.490	0.612	0.740

*, **, *** - significant at the 10, 5, and 1 percent level, respectively.
1) Huber-White standard errors are in parenthesis.
2) Brackets contain the proportion of a standard deviation change in the dependent variable brought about by increasing the independent variable by one standard deviation.
3) All regressions adjust for district clustering in MA's. See Moulton (1986).