THE CAPITALIZATION OF EDUCATION FINANCE REFORMS*

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Abstract

The education finance reforms encouraged by state court rulings over the past 25 years have led to increased state aid and educational spending in poorer school districts. This empirical study addresses whether these new resources were capitalized into the housing values and residential rents within those districts. Estimations based on district-level census data indicate that the new educational expenditures generated by the court mandates substantially increased median housing values and residential rents. This Tiebout response implies that court-mandated finance reforms increased the perceived quality of the poorer school districts in reform states. However, the existence and magnitude of this response also implies that these reforms had unintended distributional consequences. For example, these results indicate that for some the redistributive impact of education finance reform may have been sharply attenuated by the increased cost of residing in the districts that received new educational resources.

I. Introduction

In its landmark 1971 *Serrano* decision, the California Supreme Court ruled that the state's system of financing public education largely on the basis of local property taxation was unconstitutional since it did not provide equal opportunity for all of California's children. In the 25 years after the *Serrano* decision, similar constitutional challenges were mounted in over 30 other states.[1] By 1992, the state supreme courts in 11 of those states had issued rulings similar to the *Serrano* decision.[2] Recent research has sug-

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[1] A similar case was argued before the U.S. Supreme Court in 1973. However, the Court rejected the argument that education finance based on local property taxes violated the U.S. Constitution. This decision effectively returned judicial action regarding education finance to the states.

[2] The 11 other states were Arkansas (1983), Connecticut (1977), Kansas (1976), Kentucky (1989), Montana (1989), New Jersey (1973), Texas (1989), Washington (1978), West Virginia (1978), Wisconsin (1976), and Wyoming (1980). Since 1992 school finance systems have been found unconstitutional in seven other states: Alabama (1993), Arizona (1994), Massachusetts (1993), Minnesota (1993), Missouri (1993), New Hampshire (1993), and Rhode Island (1994).

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gested that these state court decisions had a dramatic impact on the structure of education finance.[3] In particular, prior studies indicate that court rulings that declared state systems of education finance unconstitutional encouraged states to increase their per-student aid to poorer school districts.[4] Furthermore, because this new aid led to relatively little or no reduction in locally generated school funding, overall educational spending increased in poorer school districts as a result of the court-ordered reforms. For example, William N. Evans, Sheila E. Murray, and Robert Schwab report that educational spending per pupil increased by 11 percent in the poorest districts in reform states.[5]

These findings provide important evidence on the efficacy of court-ordered reforms in influencing the equity with which educational resources are distributed. However, they also raise new and policy-relevant questions. Perhaps the most notable of these questions concerns the direct consequences of the increased educational spending for the quality of the poorer school districts in the reform states. Clearly, a central motivation for the reformers and courts that sought changes in the structure of education finance was that increased spending in poor school districts would increase school quality and economic opportunity. However, the relationship between educational spending and measures of school quality is notoriously controversial.[6] Therefore, it is by no means clear a priori that the new resources generated by court-ordered reforms have had the intended conse-

[3] William N. Evans, Sheila E. Murray, & Robert Schwab, Schoolhouses, Courthouses and Statehouses after *Serrano,* 16 J. Pol'y Analysis & Mgmt. 10 (1997); Sheila E. Murray, William N. Evans, & Robert Schwab, Education Finance Reform and the Distribution of Education Resources, 88 Am. Econ. Rev. 789 (1998). Similar results are reported by David Card & A. Abigail Payne, School Finance Reform, the Distribution of School Spending, and the Distribution of SAT Scores (Working Paper No. 387, Princeton Univ., Indus. Rel. Sec. 1997). Prior research had focused on the experiences within specific states like California and New Jersey: for example, Sharon B. Megdal, Equalization of Expenditures and the Demand for Local Public Education: The Case of New Jersey, 11 Pub. Fin. Q. 365 (1983); Thomas A. Downes, Evaluating the Impact of School Finance Reform in the Provision of Public Education, 45 Nat'l Tax J. 29 (1992); William A. Fischel, How *Serrano* Caused Proposition 13 (Working Paper No. 94-23, Dartmouth Coll., September 1994); Fabio Silva & Jon Sonstelie, Did *Serrano* Cause a Decline in School Spending? 47 Nat'l Tax J. 199 (1995).

[4] In general, state aid to wealthy districts was not influenced by court-ordered reforms, which suggests that states funded the new aid through higher taxes: Murray, Evans, & Schwab, *supra* note 3; Evans, Murray & Schwab, *supra* note 3. However, Caroline M. Hoxby, All School Finance Equalizations Are Not Created Equal (unpublished manuscript, Harvard Univ., Dep't Econ., May 1998), emphasizes the heterogeneous experiences within particular states.

[5] Evans, Murray, & Schwab, *supra* note 3.

[6] See, for example, Gary Burtless, Does Money Matter? The Effect of School Resources on Student Achievement and Adult Success (1996).

quence of increasing the quality of poor school districts.[7] Using data on the more than 10,000 unified school districts, this empirical study presents novel evidence on whether these court-ordered reforms increased school quality. Specifically, the possible existence of a reform-driven increase in school quality is tested by evaluating the extent to which the new educational resources were capitalized into housing values and residential rents within school districts.[8]

The motivation for this approach is rooted in the seminal model of Charles Tiebout.[9] The Tiebout model envisions a world in which the choice of where to live is influenced in part by the quality and tax cost of local public goods like public education. Beginning largely with a celebrated paper by Wallace Oates, a number of empirical studies have tested the Tiebout hypothesis by estimating the impact of the cost and quality of local public services on property values.[10] This well-established empirical approach provides a compelling opportunity to evaluate whether court-ordered reforms led to an increase in school quality. If the infusion of state aid into poor school districts improved school quality, the Tiebout model makes the straightforward prediction that some citizens would respond by ''voting with their feet,'' thereby increasing the value of houses and rental properties.[11] This approach has an appealing advantage over the traditional

[7] There is some preliminary evidence that these reforms improved test scores: Card & Payne, *supra* note 3; Thomas A. Downes & David N. Figlio, School Finance Reform, Tax Limits and Student Performance: Do Reforms Level-Up or Dumb-Down? (unpublished manuscript, Univ. Oregon, Dep't Econ., February 1997). Similarly, Caroline Hoxby, *supra* note 4, presents evidence on the capitalization of education finance incentives into property values. Daniel Aaronson, The Effect of School Finance Reform on Population Heterogeneity, 52 Nat'l Tax J. 5 (1999), also finds evidence of reform-driven Tiebout sorting.

[8] By exploiting the exogenous variation in school spending created by court reforms, this study also provides novel evidence on the more general question of whether ''money matters.''

[9] Charles Tiebout, A Pure Theory of Local Expenditures, 64 J. Pol. Econ. 416 (1956).

[10] Wallace Oates, The Effects of Property Taxes and Local Public Spending on Property Values: An Empirical Study of Tax Capitalization and the Tiebout Hypothesis, 77 J. Pol. Econ. 957 (1969). Recent empirical studies that provide some Tiebout-inspired evidence linking school quality and housing values include Sandra E. Black, Do Better Schools Matter? Parental Valuation of Elementary Education, 114 Q. J. Econ. 577 (1999); and William T. Bogart & Brian A. Cromwell, How Much More Is a Good School District Worth? 50 Nat'l Tax J. 215 (1997).

[11] The observed values, however, reflect the interaction of supply and demand. So, as in most hedonic studies, the effects of a demand shift could be attenuated by an elastic supply response: Dennis Epple, Hedonic Prices and Implicit Markets: Estimating Demand and Supply Functions for Differentiated Products, 95 J. Pol. Econ. 59 (1987). But, given the fixed supply of land and the presence of zoning restrictions, supply responses should not be fully elastic. Furthermore, in light of the results presented here, the downward bias implied by such attenuation does not appear to be problematic.

education production functions: by relying on the revealed preferences of parents choosing where to live instead of student outcomes, these evaluations do not need to make possibly unreasonable assumptions about what schools actually produce or what parents value.

However, in this context, a Tiebout response may have important implications beyond the issue of how education finance reform directly influenced school quality. In particular, the existence of a Tiebout response to the new educational resources also raises the possibility that the court-ordered reforms had important and unintended distributional consequences. For example, because Tiebout responses would imply increased property values and residential rents, the redistributive impact of the new state aid may have been attenuated for those who did not own their homes.[12] Put simply, a poor person who rented his residence could, to some degree, be implicitly paying for the increased quality of local schools through higher residential rents. Since this study provides evidence on the degree to which the new educational resources were capitalized into property values and residential rents, the estimates presented here also suggest the extent to which private Tiebout responses may have attenuated the intended redistributive impact of the court-ordered reforms.

Drawing on the results of prior research, this study first presents an empirical specification that can evaluate the consequences of court-ordered education finance reforms by relying on the relatively detailed cross-sectional data currently available on school districts. Estimations based on this unique specification closely replicate prior findings: court-ordered finance reforms increased per-pupil educational spending, particularly in the reform states' poorest school districts. In the next section, similarly specified reduced-form models of residential housing values and rents are presented. The results of these estimations indicate that the new resources created by court-ordered reforms were capitalized. In particular, in the poorest school districts of the reform states, housing values and rents rose by at least 8 percent. Furthermore, these large and statistically significant capitalization effects appear robust to several specification changes. Notably, these robustness checks include the results of evaluations based on repeated cross sections of district-level census data, which are effectively identified by within-state variation over time. Overall, these results suggest that the new educational resources directed toward the poor school districts in reform

[12] And, of course, for those who did own their homes, an increase in school quality would constitute an increase in wealth. These theoretical possibilities were identified in an earlier study, Paul Gary Wyckoff, Capitalization, Equalization and Intergovernmental Aid, 23 Pub. Fin. Q. 484 (1995). According to the 1990 census, the overall rate of home ownership in the United States is 64.2 percent. However, among African Americans and Hispanics, the rates are only 43.4 and 42.4 percent, respectively.

states did have their intended consequence: they substantially increased perceived school quality. However, the magnitudes of these effects also imply that for some residents the redistributive impact of these reforms may have been sharply attenuated by the higher cost of residing in the improved school districts.

II. Educational Resources and Court-Ordered Reforms

Several recent studies have addressed the efficacy of education finance reforms in influencing the distribution of education resources.[13] Relying on repeated cross sections of district-level revenue and expenditure data, these studies found that court-ordered education finance reforms significantly increased state support and per-pupil spending in poorer school districts. In this section, these important, motivating results on educational spending are replicated by employing the unique identification strategy that also characterizes some of this study's novel empirical evidence on capitalization. The strong correspondence between these expenditure results and the prior research on the links between court-ordered reforms and school resources provides an important validation of this research design.

A. Data

Most of the district-level data used in this study were drawn from the information available in the National Center for Education Statistics' (NCES's) School District Data Book (SDDB) and the 1989–90 Common Core of Data (CCD). The CCD contains information from annual universe surveys of schools, school districts, and state education agencies. The SDDB is a unique education database that combines district-level information from the 1989–90 CCD, the Census Bureau's 1989–90 Census of Local Government Finances (F33), and the 1990 decennial census.[14] By merging these contemporaneous cross-sectional data sets, the SDDB combines institutional data on school districts with population and housing characteristics that have been defined by school district boundaries through the Census Mapping Project. In particular, using data from the 1990 census, the SDDB identifies the median of housing values and of gross monthly residential rents within school districts.

[13] Murray, Evans, & Schwab, *supra* note 3; Evans, Murray, & Schwab, *supra* note 3; Card & Payne, *supra* note 3.

[14] For an overview of available data on school districts, see Thomas S. Dee, William N. Evans, & Sheila E. Murray, Datawatch: Research Data in the Economics of Education, 13 J. Econ. Persp. 205 (1999). Additional CCD data were merged with the SDDB data because certain key variables were unavailable in the SDDB data.

A merger of 1989–90 CCD data and the SDDB data produced a data set with 14,952 matched school districts. As in prior studies, four states (Alaska, Hawaii, Vermont, and Montana) and the District of Columbia were omitted because they have either state-based systems of education or a limited number of unified districts (that is, districts that offer both elementary and secondary education). This reduced the data set to 14,098 districts. Also as in prior studies, the data were edited to include only those districts with similar organizational characteristics: regular, unified school districts that were operational and locally controlled. This reduced the data set to 10,641 observations.[15] Eliminating school districts that were missing key variables reduced the data set to 10,559 observations. Prior researchers have also noted that extremely high and low values of per-pupil revenues or expenditures are likely to reflect incorrect enrollment or finance data. Eliminating school districts with outlying per-pupil expenditure data reduced this sample to 10,476 observations.[16] Most of the estimations presented in this study are based on a sample of 10,341 school districts that also excludes the 135 school districts with median housing values in excess of $239,400.[17] The motivation for excluding these 135 districts is that the distribution of housing values in the full sample is highly skewed. In particular, in some specifications of the housing value equations (that is, those that include potentially endogenous covariates), these 135 outliers are quite influential.[18] However, models based on the full sample of 10,476 districts are also presented here to demonstrate the impact of these outliers. Furthermore, since a disproportionately high share of the 135 school districts with the highest median housing values come from California (roughly 39 percent), the results presented here are also replicated using the full sample excluding California ($N = 10{,}239$).[19]

Of the 46 states represented in this data set, the nine considered reform states are those with a pre-1989 court ruling that found the state's school finance system unconstitutional (that is, Arkansas, California, Connecticut,

[15] Evans, Murray, & Schwab, *supra* note 3, notes, however, that over 90 percent of public school students are in such unified school districts.

[16] School districts with values greater than 150 percent of the ninety-fifth percentile or less than 50 percent of the fifth percentile were deleted: Evans, Murray, & Schwab, *supra* note 3. Card & Payne, *supra* note 3, adopts a similar approach.

[17] This cutoff number is 150 percent of the ninety-fifth percentile value for median housing value.

[18] This is despite the use of the standard semilog specification. Linear models generate results similar to the semilog models reported here, but, not surprisingly, those models are somewhat more sensitive to the impact of these outliers.

[19] This exclusion is useful not only for isolating outliers but also because California's experience with court-ordered education finance reforms is arguably unique.

Kansas, New Jersey, Washington, West Virginia, Wisconsin, and Wyoming).[20] Over 17 percent of the school districts in this data set are in these reform states (Table 1). In order to identify the consequences of the court-ordered reforms, it is also necessary to characterize a school district's relative affluence within its state. In other words, since the intent of the court-ordered reforms has been to ameliorate the inequitable distribution of education resources, it is necessary to know the position of each district within a given state's distribution. As in some prior research, this ranking of districts within states was done according to the amount of per-pupil revenues generated locally.[21] More specifically, the school districts were grouped into quintiles based on their position within their state's distribution of locally generated revenue per pupil.[22] Descriptive statistics on the key variables are presented for all 10,341 districts and by quintile in Table 1.

One potential problem with using contemporaneous local revenues to identify the districts that might have been influenced by court-ordered reforms is that such districts may have passed some of the new state resources along to taxpayers in the form of lower taxes. If this ‘‘fiscal substitution’’ were dramatic and heterogeneous, it could introduce measurement error into the quintile rankings. Furthermore, empirical models of housing values and residential rents might confound the capitalization of the lower taxes with that of potential changes in school quality. However, this concern should not prove to be empirically relevant. Evans, Murray, and Schwab found only limited and qualified evidence that there was any fiscal substitution of the resources generated by the court rulings.[23] Similarly, David Card and A. Abigail Payne, using a similar data set, report little or no evidence of a fiscal substitution.[24] Also, as demonstrated in the next section, the expenditure results based in part on this ranking procedure closely replicate the prior findings that could draw on more expansive sources of data. Furthermore, as a robustness check, this study's results are also replicated in models that employ historical as opposed to contemporaneous data on lo-

[20] Texas and Kentucky had similar court rulings in 1989. However, since the data employed here were collected during the 1989–90 school year, they are not considered reform states here.

[21] Evans, Murray, & Schwab, *supra* note 3.

[22] The next section discusses how these rankings are used econometrically. Note that to incorporate information on district size in these rankings, they were effectively based on the student instead of the district. This is why a relatively high share of districts is in the first quintile (that is, lowest levels of locally generated per-pupil revenues). Those school districts tend to be the rural ones with low enrollments (Table 1). Rankings that divide a state's districts into roughly equal groups of 20 percent return results similar to those reported here.

[23] Evans, Murray, & Schwab, *supra* note 3.

[24] Card & Payne, *supra* note 3.

TABLE 1

VARIABLE MEANS, 1990 DISTRICT-LEVEL CENSUS DATA

Variable	All Districts	Quintile in State's Local Revenues Per Pupil				
		5	4	3	2	1
Current expenditures per pupil ($)	4,351	5,344	4,487	4,112	3,921	3,973
	(1,261)	(1,593)	(1,089)	(901)	(923)	(1,014)
Court reform	.173	.194	.191	.221	.142	.139
	(.378)	(.396)	(.393)	(.415)	(.349)	(.346)
Median housing value ($)	58,047	73,643	67,660	56,590	51,267	46,271
	(37,873)	(54,085)	(43,265)	(33,574)	(24,782)	(20,275)
Median gross rent ($)	347	396	377	343	329	308
	(121)	(164)	(132)	(111)	(90)	(77)
Suburban	.345	.390	.416	.347	.325	.280
	(.475)	(.488)	(.493)	(.476)	(.468)	(.449)
Rural	.615	.566	.526	.608	.637	.692
	(.487)	(.496)	(.499)	(.488)	(.481)	(.462)
Black	.052	.038	.049	.051	.055	.064
	(.120)	(.097)	(.103)	(.112)	(.122)	(.145)
Hispanic	.043	.042	.043	.031	.041	.053
	(.114)	(.092)	(.102)	(.078)	(.107)	(.153)
Other race	.021	.019	.019	.016	.016	.030
	(.065)	(.048)	(.102)	(.039)	(.038)	(.103)
Number of observations	10,341	2,177	1,542	1,781	2,168	2,673

NOTE.—Standard deviations are reported in parentheses. These sample statistics are unweighted.

cally generated revenues per pupil. Data on 1976–77 locally generated revenues per pupil were drawn from the NCES's Merged Federal Files, a unique data file that matched responses to the 1976–77 F33 Survey of Local Government Finances to NCES school district identifiers. There are two important caveats associated with the use of these historical finance data. One is that the data could be matched for only 9,113 districts, a significant reduction in sample size of roughly 12 percent. Second, a district's historical amount of local revenues per pupil may be a noisy indicator of its current relative affluence and, by implication, its current receipt of reform-driven state aid.

B. Specifications

Evaluations of state-level court decisions based on the cross-sectional data presented in the previous section could be problematic since the state court's decision may itself be influenced by relevant and unobserved attributes of the state. Fortunately, this concern can be addressed, in part, by exploiting the results from previous empirical research that has employed data with both cross-sectional and time-series variation. Using repeated cross sections of school districts, prior studies demonstrated that court-ordered finance reforms generated unequal changes in state financial support.[25] In particular, they found that the amount of new state aid directed to a district as a result of the court mandate varied systematically with the amount of revenue it generated locally. State aid to the wealthiest districts (that is, those that generated considerable local support for education) was unaffected by the court mandates. However, the poorest communities in reform states (that is, those at the bottom of their state distribution for locally generated revenues) received the most new funds. This heterogeneity in the impact of the court reforms allows reforms consequences to be evaluated in a cross-sectional estimation that conditions on shared but unobserved state-specific attributes. More specifically, those districts that benefited from the reforms can be identified, ceteris paribus, by the interactions of a binary indicator for whether their states had court-ordered reform with binary indicators that reflect their position in the within-state distribution of locally generated revenues. The key equation to be estimated takes the following form:

$$Y_{dms} = X_{ds}\beta + Q_m\alpha_m + Q_m R_s \delta_m + \mu_s + \varepsilon_{dms}. \tag{1}$$

In this specification, Y_{dms} represents the natural logarithm of the dependent variable (that is, current expenditures per pupil, median housing values, or

[25] Evans, Murray, & Schwab, *supra* note 3; Murray, Evans, & Schwab, *supra* note 3.

median gross rents) for district d in state s and quintile m. The term X_{ds} represents district-level regressors that also influence the dependent variable.[26] The term μ_s represents state fixed effects that control for the unobserved state-specific determinants of the dependent variable. In this specification, the expression Q_m represents four binary indicators that equal one if the given district is in the mth quintile of its state distribution for locally generated per-pupil revenues. The reference districts in these estimations are those that are in the top quintile of their state's distribution (that is, the fifth quintile) for generating local revenues per pupil ($m \in (1, 2, 3, 4)$). The term R_s is a binary indicator for whether the state has had court-ordered education finance reform.[27] Some specifications will also employ an alternative expression where R_s represents the years since the state's first court-ordered reform. This formulation is clearly an ad hoc one. Nonetheless, evaluations based on this measure may provide an important complement to those based on a simple binary indicator since the court-ordered reforms could plausibly take some time to influence both school expenditures and subsequent adjustments in housing and rental markets.[28] The four key coefficients of interest in this equation are δ_m since the interaction of Q_m and R_s identifies those districts that were ostensibly influenced by the court reforms (that is, the relatively resource-poor districts in reform states). For example, the coefficient δ_1 identifies the effect of the court reforms in the poorest communities, conditional on the unobserved factors that make those communities and the given state unique.[29]

Relying on the results from various estimations of equation (1) raises a number of particular specification issues. Several of these issues (for exam-

[26] The socioeconomic and demographic covariates, which are based on the 1990 census, would clearly be endogenous regressors in this Tiebout-inspired specification. Therefore, in some estimations, X_{sd} will include only an intercept. Nonetheless, covariates representing the racial/ethnic composition of the school district and its urbanicity (Table 1) will be included in some estimations as a robustness check. Furthermore, some specifications will also include interactions between the urbanicity indicators and the state fixed effects as regressors.

[27] Since it would be perfectly collinear with the state fixed effects, R_s does not appear in this specification except in the interaction terms.

[28] A similar characterization of the court-ordered reforms was also employed by Evans, Murray, & Schwab, *supra* note 3. One difficulty with the ''years since court reform'' measure is that it may be plagued by measurement error. For example, this might occur if states reformed education finances in anticipation of a pending court ruling. However, given the capitalization results reported here, the attenuation bias implied by measurement error does not appear to be a particularly salient specification issue. Furthermore, in the evaluations based on repeated cross sections, this admittedly crude formulation does prove particularly insightful.

[29] It should be noted, however, that the possibly heterogeneous effects associated with the reform experiences in particular states may be obscured by this research design. Hoxby, *supra* note 4, suggests that education finance reforms are better understood by their effects on local tax prices.

ple, downward bias due to supply-side responses, upward bias due to fiscal substitution) have been addressed already. An additional concern is that because the school districts are of widely varying sizes the regression errors in equation (1) are likely to be heteroskedastic. That concern will be addressed by employing heteroskedastic-consistent standard errors in all statistical inferences.[30] A more general concern is whether one can be confident that the unusual cross-sectional identification strategy represented by equation (1) will effectively identify the true effects of the court-ordered reforms. Most notably, though these specifications include state fixed effects, unobserved state attributes could still exert a confounding influence. For example, if the courts in states with a high variance in the distributions for per-pupil expenditures, housing values, or gross rents were more likely to adopt finance reforms, the inferences based on equation (1) would be plagued by a downward bias. Similarly, if states with relatively tight distributions in the dependent variable were more likely to have court-ordered reforms, the efficacy of the reforms would be overstated by estimations of equation (1). Given the pattern of the capitalization results presented here, this latter concern is of particular importance. This study addresses the empirical relevance of this key specification concern in several ways that are discussed in a subsequent section. However, this section presents indirect evidence on this question by using this specification to replicate prior empirical research on the effect of the court reforms on per-pupil spending in the public schools. More specifically, this evidence is based on estimations of equation (1) where the dependent variable is the natural logarithm of current expenditures per pupil. The key results of those estimations are reported in Table 2. Consistent with recent research, these estimates uniformly suggest that court-ordered finance reforms had positive and statistically significant effects on the per-pupil spending in the public schools of poorer communities. Furthermore, the correspondence of these evaluation parameters with those based on data that permitted unambiguous controls for state-specific attributes provides an important validation of the other inferences based on equation (1).

C. Results

The key results from estimating equation (1) where the dependent variable is the natural log of expenditures per pupil are reported in Table 2. These results indicate that court-ordered education finance reforms dramatically influenced school spending in poorer districts. For example, estimates

[30] Halbert White, A Heteroskedasticity-Consistent Covariance Matrix Estimator and a Direct Test for Heteroskedasticity, 48 Econometrica 817 (1980).

TABLE 2

CURRENT EXPENDITURES PER PUPIL AND COURT REFORMS

Independent Variable	Model 1	Model 2	Model 3	Model 4
Court reform:				
Quintile 4 × court reform	.032	.036	.035	.040
	(.013)	(.013)	(.013)	(.013)
Quintile 3 × court reform	.058	.061	.057	.057
	(.012)	(.012)	(.012)	(.012)
Quintile 2 × court reform	.050	.054	.050	.050
	(.013)	(.013)	(.013)	(.013)
Quintile 1 × court reform	.090	.097	.091	.087
	(.014)	(.013)	(.013)	(.013)
R^2	.706	.709	.729	.744
Years since court reform:				
Quintile 4 × years since court reform	.0019	.0022	.0021	.0025
	(.0009)	(.0009)	(.0009)	(.0009)
Quintile 3 × years since court reform	.0039	.0042	.0038	.0039
	(.0009)	(.0009)	(.0009)	(.0009)
Quintile 2 × years since court reform	.0036	.0039	.0033	.0035
	(.0010)	(.0010)	(.0009)	(.0010)
Quintile 1 × years since court reform	.0066	.0071	.0065	.0063
	(.0010)	(.0010)	(.0010)	(.0010)
R^2	.706	.709	.729	.743
Urbanicity indicators	No	Yes	Yes	Yes
Race/ethnicity variables	No	No	Yes	Yes
State fixed effects × urbanicity indicators	No	No	No	Yes

NOTE.—Heteroskedastic-consistent standard errors are reported in parentheses. All models include an intercept, binary indicators for quintiles 1–4, and state fixed effects.

from models 1 and 2, which are reported in the top panel of Table 2, imply that the court mandates increased per-pupil expenditures by 9–10 percent in the poorest school districts.[31] Furthermore, these effects have a rough but plausible monotonicity: for districts that were higher in the state distribution of locally generated per-pupil revenues, the estimated effects of the court-ordered reforms were generally smaller.[32] In wealthier districts (that is, quintiles 2, 3, and 4), court-ordered reforms increased per-pupil spending only by roughly 3–6 percent. In addition, the results are robust to changes in the set of regressors. Model 2 adds two urbanicity indicators to the basic specification in model 1. Model 3 adds to model 2 three variables represent-

[31] For the average school district in this quintile, this implies an increase of nearly $400 per pupil. Using data for slightly different time periods, both Evans, Murray, & Schwab, *supra* note 3, and Card & Payne, *supra* note 3, report similarly sized effects.

[32] The marginal effects in quintiles 2 and 3 do not exhibit a plausible monotonicity. However, in all of these models the distinction between these marginal effects is relatively small (that is, less than 1 standard error).

ing the racial and ethnic composition of the school district. Model 4 adds to model 3 over 90 binary indicators representing the unobserved determinants specific to the rural and suburban regions within each state (that is, interactions between the state fixed effects and the urbanicity indicators). Models that include these regressors should be interpreted with some caution since, given the Tiebout responses under study, the additional covariates are arguably endogenous. Nonetheless, they provide some important evidence on the robustness of the results from the sparsely specified model 1.

In the estimations reported in the bottom panel of Table 2, the reform measure is the number of years since the state's first court-ordered reform. The results of these estimations are consistent with those reported in the upper panel. Court-ordered reforms had statistically significant effects on school spending in poorer communities. For example, the results from model 1 suggest that 3 years after a court ruling declaring the state's system of education finance unconstitutional, educational spending in the poorest districts increased by roughly 2 percent. Ten years after such court rulings, per-pupil expenditures rose by roughly 6.6 percent. A comparison of models 1–4 indicates that these estimates are also quite robust to changes in the set of regressors. In sum, the estimates from all of the models in Table 2 confirm what has been reported in other recent research: court-ordered reforms have had a dramatic effect on the distribution of educational spending. Furthermore, these replications provide an important validation of the estimation strategy adopted for the unique evaluations presented here.

III. Housing Values and Rents

This section presents evidence on whether court-ordered education finance reforms were capitalized into housing values and residential rents. This evidence is based on estimates of the cross-sectional specification employed in the previous section as well as robustness checks that include evaluations based on additional cross sections of district-level data.

A. Results

The key results of estimating equation (1) where the dependent variable is the natural log of the school district's median housing value are reported in Table 3. These results suggest that court-ordered reforms had dramatic and statistically significant effects on the median housing values in poorer school districts. For example, model 1 implies that in the poorest school districts court-ordered finance reforms increased median housing values by 16.9 percent. Plausibly, in districts that received less new state aid, these estimated effects are monotonically smaller. Similar results emerge in

TABLE 3

MEDIAN HOUSING VALUES AND COURT REFORMS

Independent Variable	Model 1	Model 2	Model 3	Model 4
Court reform:				
Quintile 4 × court reform	.073	.032	.036	.020
	(.033)	(.030)	(.030)	(.029)
Quintile 3 × court reform	.083	.043	.052	.037
	(.032)	(.028)	(.028)	(.028)
Quintile 2 × court reform	.133	.075	.085	.066
	(.033)	(.029)	(.029)	(.029)
Quintile 1 × court reform	.169	.082	.095	.082
	(.032)	(.029)	(.029)	(.029)
R^2	.554	.639	.643	.657
Years since court reform:				
Quintile 4 × years since court reform	.0056	.0019	.0023	.0009
	(.0025)	(.0022)	(.0022)	(.0021)
Quintile 3 × years since court reform	.0067	.0027	.0036	.0026
	(.0024)	(.0022)	(.0022)	(.0021)
Quintile 2 × years since court reform	.0078	.0026	.0039	.0021
	(.0027)	(.0024)	(.0024)	(.0024)
Quintile 1 × years since court reform	.0109	.0038	.0053	.0039
	(.0025)	(.0023)	(.0023)	(.0023)
R^2	.553	.639	.643	.657
Urbanicity indicators	No	Yes	Yes	Yes
Race/ethnicity variables	No	No	Yes	Yes
State fixed effects × urbanicity indicators	No	No	No	Yes

NOTE.—Heteroskedastic-consistent standard errors are reported in parentheses. All models include an intercept, binary indicators for quintiles 1–4, and state fixed effects.

model 1 when the reform variable is the number of years since the first court reform instead of a binary indicator for court reform. More specifically, model 1 indicates that each year after a court-ordered reform median housing values in the poorest districts rose by more than 1 percent. And, again, these effects were smaller in the wealthier school districts that received less aid.

Models 2–4 provide evidence on the robustness of these results by including the additional regressors discussed earlier. These models indicate that the estimated effect of the court reforms on housing values is substantially smaller in models that include these additional covariates. For example, model 2, which includes urbanicity indicators, suggests that court reforms increased housing values by 8.2 percent instead of the 16.9 percent implied by model 1. However, this smaller marginal effect is still statistically distinguishable from zero (that is, nearly three times larger than its standard error).[33] Models 3 and 4, which add additional covariates to model 2, also suggest that the court reforms led to smaller increases in housing values. However, in the poorer districts most influenced by the reforms, these smaller marginal effects are still statistically distinguishable from zero.[34] Given the possible endogeneity of the additional regressors, there is some uncertainty as to how the sensitivity evidenced in models 2–4 should be understood.[35] Regardless, this sensitivity does not substantively alter the main findings in Table 3: the new resources generated by court-ordered reforms appear to have been capitalized into housing values. In light of this robustness, the results from models 2–4 can be understood as conservatively low estimates for the magnitude of this capitalization.

The results presented in Table 4 provide evidence on whether the new resources generated by court-ordered reforms were also capitalized into residential rents. The results of these models are entirely consistent with the housing value equations. For example, model 1 in Table 4 suggests that the new educational resources generated by the court reforms increased residential rents by nearly 14 percent in the poorest school districts (or, alternatively, by more than 1 percent each year after the court decision). Furthermore, these estimated effects are again monotonically smaller in the wealthier districts that were less influenced by the finance reforms. However, as in the housing value equations, the estimated magnitudes of these effects are somewhat sensitive to the inclusion of additional regressors. For example, model 4 suggests that court-ordered finance reforms increased residential rents in the poorest districts by only 7.5 percent (or, alternatively, 0.55 percent each year after the court decision). Nonetheless, even the smaller marginal effects are still statistically precise, particularly in the poorer school districts.

It was noted earlier that some of the capitalization results from the housing value equations were sensitive to including the 135 school districts with the highest median housing values. The results presented in Table 5 illustrate the impact of these outliers. The top panel in Table 5 simply repeats

[33] These estimated marginal effects are economically meaningful as well. The magnitudes of these estimates are discussed in a subsequent section.

[34] However, the estimated capitalization was particularly imprecise in models where the reform variable was "years since court reform." But, even in those models, the impact of the court reforms in the poorest districts can be distinguished from zero with at least 90 percent confidence.

[35] In states with court reforms, a somewhat smaller proportion of school districts are now classified as rural (roughly 59 percent). This could actually be due, in part, to the court reforms. However, to the extent these variables are independent of the court reforms, omitting them could lead to an overstatement of the reform's impact on property values in poor school districts.

TABLE 4

MEDIAN GROSS RENTS AND COURT REFORMS

Independent Variable	Model 1	Model 2	Model 3	Model 4
Court reform:				
Quintile 4 × court reform	.054	.026	.029	.017
	(.020)	(.017)	(.017)	(.017)
Quintile 3 × court reform	.075	.048	.053	.042
	(.018)	(.016)	(.016)	(.015)
Quintile 2 × court reform	.105	.065	.071	.058
	(.019)	(.016)	(.016)	(.016)
Quintile 1 × court reform	.136	.077	.084	.075
	(.020)	(.017)	(.017)	(.017)
R^2	.509	.624	.630	.641
Years since court reform:				
Quintile 4 × years since court reform	.0042	.0017	.0020	.0010
	(.0015)	(.0013)	(.0013)	(.0012)
Quintile 3 × years since court reform	.0065	.0038	.0044	.0036
	(.0014)	(.0012)	(.0012)	(.0012)
Quintile 2 × years since court reform	.0076	.0040	.0048	.0037
	(.0015)	(.0013)	(.0013)	(.0013)
Quintile 1 × years since court reform	.0104	.0055	.0064	.0055
	(.0015)	(.0013)	(.0013)	(.0013)
R^2	.509	.624	.630	.641
Urbanicity indicators	No	Yes	Yes	Yes
Race/ethnicity variables	No	No	Yes	Yes
State fixed effects × urbanicity indicators	No	No	No	Yes

NOTE.—Heteroskedastic-consistent standard errors are reported in parentheses. All models include an intercept, binary indicators for quintiles 1–4, and state fixed effects.

the key results from the previous three tables for the poorest school districts. More specifically, Table 5 reports the marginal effects of the court reforms (and, alternatively, years since the first court reform) in the poorest school districts for each of the three dependent variables and by all four model specifications. The middle panel in Table 5 reports the same results for a sample that includes the 135 deleted districts. In the expenditure and residential rent equations based on this full sample, the estimated marginal effects of the court reforms are substantively unchanged though somewhat smaller in magnitude. For example, these models suggest that the court reforms increased residential rents by 5–11 percent. Similarly, using the full sample to estimate model 1 of the housing value equation also leads to substantively unchanged though smaller marginal effects (that is, an increase of 11.7 percent). However, the additional regressors included in models 2–4 decrease the magnitudes of these marginal effects sharply. In those models, the smaller capitalization effects cannot be statistically distinguished from zero. In some models based on the full sample, the estimated effects of the number of years since court reform even has the wrong sign.

TABLE 5

EFFECTS OF COURT REFORM AND YEARS SINCE COURT REFORM IN QUINTILE 1 BY SAMPLE AND MODEL

	ln(CURRENT EXPENDITURES PER PUPIL)		ln(MEDIAN HOUSING VALUES)		ln(MEDIAN GROSS RENTS)	
SAMPLE AND MODEL	Court Reform	Years since Court Reform	Court Reform	Years since Court Reform	Court Reform	Years since Court Reform
Reported sample (N = 10,341):						
Model 1	.090	.0066	.169	.0109	.136	.0104
	(.014)	(.0010)	(.032)	(.0025)	(.020)	(.0015)
Model 2	.097	.0071	.082	.0038	.077	.0055
	(.013)	(.0010)	(.029)	(.0023)	(.017)	(.0013)
Model 3	.091	.0065	.095	.0053	.084	.0064
	(.013)	(.0010)	(.029)	(.0023)	(.017)	(.0013)
Model 4	.087	.0063	.082	.0039	.075	.0055
	(.013)	(.0010)	(.029)	(.0023)	(.017)	(.0013)
Full sample (N = 10,476):						
Model 1	.087	.0062	.117	.0053	.110	.0076
	(.013)	(.0010)	(.034)	(.0027)	(.020)	(.0015)
Model 2	.093	.0066	.037	−.0010	.056	.0033
	(.013)	(.0010)	(.031)	(.0025)	(.017)	(.0013)
Model 3	.087	.0060	.053	.0001	.064	.0042
	(.013)	(.0010)	(.030)	(.0024)	(.017)	(.0013)
Model 4	.082	.0057	.031	−.0016	.053	.0031
	(.013)	(.0010)	(.030)	(.0025)	(.017)	(.0013)
Full sample without California (N = 10,239):						
Model 1	.073	.0048	.147	.0090	.112	.0085
	(.014)	(.0011)	(.034)	(.0029)	(.020)	(.0016)
Model 2	.078	.0051	.085	.0042	.070	.0052
	(.014)	(.0011)	(.031)	(.0026)	(.017)	(.0014)
Model 3	.077	.0050	.089	.0046	.069	.0052
	(.013)	(.0011)	(.030)	(.0025)	(.017)	(.0014)
Model 4	.070	.0045	.079	.0036	.066	.0049
	(.014)	(.0011)	(.031)	(.0025)	(.018)	(.0014)

NOTE.—Dependent variables are ln(current expenditures per pupil), ln(median housing values), and ln(median gross rents). Heteroskedastic-consistent standard errors are reported in parentheses. All models include an intercept, binary indicators for quintiles 1–4, and state fixed effects. Model specifications are consistent with those in previous tables.

However, an examination of these influential outliers suggests that they have a spurious impact on the evaluations presented here. These school districts are located in extremely wealthy communities (that is, median housing values in excess of $239,400). In particular, a highly disproportionate share (roughly 39 percent) of these school districts comes from California, the first state with court-ordered education finance reform. The influence of these observations on the estimated capitalization may be a misleading reflection of the relatively high property values in wealthy California communities and that state's unique experience with these reforms. To provide evidence on this hypothesis as well as additional robustness checks, these models were estimated using the full sample but excluding all the school districts in California. The results are reported in the bottom panel of Table 5. The results are quite similar in magnitude and statistical significance to those based on the reported sample of 10,341 school districts. For example, these models suggest that court-ordered education finance reforms increased housing values and residential rents by roughly 8–15 percent.

Another important robustness check was to replicate these evaluations in models that relied on historical school finance data to identify each district's relative affluence within its state. As noted earlier, the current data were matched to 1976–77 district-level data on locally generated revenues per pupil. These finance data, which preceded many of the court-ordered reforms, were then employed to generate new quintile rankings of the school districts. There are at least two important concerns regarding these data. One is that the match to the 1976–77 survey data sharply reduced the sample size ($N = 9{,}113$ districts). Second, a district's historical affluence relative to its state may be a noisy indicator of its current affluence and, by implication, its receipt of state aid. Expenditure equations based on these new rankings indicate that these issues may be somewhat important. In particular, expenditure equations based on these rankings (and all four empirical specifications) suggest that the amount of new per-pupil resources directed to the poorest school districts in reform states was somewhat smaller (around 5 percent as opposed to the estimate of 9 percent in Table 2). However, these estimates, which are also smaller than the prior estimates based on repeated cross sections, could reflect an attenuation bias associated with the introduction of measurement error or the selectivity of the remaining matched districts. Regardless, the key capitalization results prove fairly robust to the use of these new quintile rankings. For example, these models suggest that court-ordered reform increased median housing values in the poorest districts by 8–18 percent and median gross rents by 9–15 percent. In general, the estimated effects for the less wealthy districts also indicate that the reforms were capitalized. However, those estimates are less stable and plausibly monotonic than those based on current quintile rankings. This may be because the introduced measurement error was particularly acute for those districts that were not extremely wealthy or poor. Nonetheless, the use of rankings based on historical finance data does provide consistent evidence for the capitalization of these reforms in the poorest school districts.

B. Evidence from Repeated Cross Sections

The evidence presented in the previous sections suggests that court-ordered education finance reforms increased spending in poorer school districts and that these new expenditures were capitalized into housing values and residential rents. However, a central concern with the quality of these inferences is that this identification strategy may confound the state courts' rulings in favor of reform with the character of the within-state distributions of school expenditures and property values. In particular, to the extent that states with tighter distributions for these variables are more likely to have court-ordered reforms, these results could be quite misleading. However, there are several kinds of relatively indirect evidence that this concern is not problematic. First, as already emphasized, the correspondence between the expenditure results in Table 2 and prior evaluations based on repeated cross sections provides an important validation of this research design. Second, the evidence from the estimations in Tables 3, 4, and 5 demonstrates that these results are quite robust to changes in the set of regressors. Third, simple state-level auxiliary regressions demonstrate that the 1970 state-level variability in median gross rents and in median housing values is actually uncorrelated with whether state courts later ruled that the state system of education finance was unconstitutional.[36]

Fourth, sample exclusions based on other possibly relevant state characteristics also underscore the robustness of these results. For example, the results in Tables 2, 3, and 4 are replicated in models that exclude school districts in states where education finance reforms were initiated by legislatures instead of courts ($N = 6{,}233$).[37] Models based on this sample suggest that in the poorest districts, court-ordered reforms increased per-pupil spending by 12–13 percent, median housing values by 11–20 percent, and

[36] These estimations, which are not reported here, are linear probability models for whether a state had court-ordered reform as function of the within-state variation in median rents and housing values as measured by the ratio of the ninety-fifth and fifth percentile values.

[37] Information on legislative reforms was drawn from Evans, Murray, & Schwab, *supra* note 3, which found that they were generally ineffective.

median gross rents by 9–16 percent. These results were also robust in models that exclude districts from states that introduced tax limitations ($N =$ 6,023). The evaluation results based on this sample suggest that court-ordered reforms increased per-pupil spending by 6–7 percent, median housing values by 17–27 percent, and median gross rents by 13–19 percent.[38] Furthermore, evaluations based only on suburban school districts ($N =$ 3,563), which are more homogeneous with regard to omitted variables, also lead to similar capitalization results. More specifically, the models based on that sample indicate that education finance reforms increased per-pupil expenditures in the poorest suburban districts by 8–17 percent. Those same reforms increased median housing values and gross rents by 10–15 percent.

These evaluations provide important evidence on the robustness of the capitalization results. However, more definitive evidence on these key results would have to be based on a research design that unambiguously purged possibly confounding cross-sectional heterogeneity by relying on the within-state variation over time in median housing values, rents, and education finance reform. Fortunately, as with the 1990 census, certain data from the 1970 and 1980 decennial censuses have also been defined by school district boundaries.[39] In particular, these census ‘‘mapping files’’ include information on median gross rents and housing values.[40] However, these census data also have a number of important limitations. For example, the 1970 census file consists of no more than 9,558 unified school districts with nonmissing data, since school districts with low enrollments were excluded.[41] Furthermore, there are not established provisions for matching the district records in all three census files, so these pooled cross sections do not constitute true district-level panel data. The inability to match district data across these files implies that the older district records cannot be

[38] Data on states with tax limits were drawn from David N. Figlio, Did the ‘‘Tax Revolt’’ Reduce School Performance? 65 J. Pub. Econ. 245 (1997). The robustness of these results to excluding states that also introduced tax limits over this period is particularly important since there is some evidence that tax limitations may have evolved in response to court-ordered education finance reforms: see Fischel, *supra* note 3.

[39] Dee, Evans, & Murray, *supra* note 14.

[40] Data on median gross rents are available in both the 1970 and 1980 census files. However, data on median housing values are available only in the 1970 file. The median value and rent data from these censuses are represented by categorical ranges for which the midpoints provide cardinal measures. Fewer than .5 percent of the unified districts have censored measures for median rent and value.

[41] The sample selection in the 1970 data is particularly sharp since there were actually more school districts at that time. The 1980 census file includes 11,068 records for unified school districts with nonmissing data. Unlike the relatively detailed 1990 data, these earlier files do not include information on the administrative or operating status of the district.

TABLE 6

CAPITALIZATION OF COURT REFORMS (1970 and 1990 Census Data)

Dependent Variable and Specification	Court Reform Parameter	Court Reform R^2	Years since Court Reform Parameter	Years since Court Reform R^2	Number of Observations
ln(median housing value):					
Year fixed effects	.2038 (.0165)	.6519	.0245 (.0013)	.6569	19,899
State and year fixed effects	.0869 (.0147)	.7926	.0085 (.0012)	.7928	19,899
ln(median gross rent):					
Year fixed effects[a]	.1227 (.0061)	.7399	.0156 (.0007)	.7418	30,788
State and year fixed effects[a]	−.0062 (.0075)	.8334	.0023 (.0007)	.8334	30,788
Year fixed effects	.0988 (.0092)	.7926	.0126 (.0007)	.7947	19,720
State and year fixed effects	.0391 (.0094)	.8699	.0022 (.0007)	.8699	19,720

NOTE.—Independent variables are court reform and years since court reform. Heteroskedastic-consistent errors are reported in parentheses.
[a] These equations include data from the 1980 census.

matched to the amount of per-pupil revenues generated locally. Nonetheless, the availability of these three cross sections presents an important opportunity to evaluate the robustness of the previous capitalization results to an identification strategy that relies on within-state variation over time.

In particular, a straightforward approach for evaluating the robustness of the prior evidence is to consider the results of simple two-way fixed-effects models that relate the median gross rents and housing values to the measures of court-ordered education finance reforms. The key results from such evaluations are presented in Table 6. The results in the top panel are based on median housing value data from the 1970 and 1990 censuses. In models that exclude state fixed effects, these estimates suggest that education finance reforms led to a 20 percent increase in median housing values (2.5 percent for every year after the court ruling). Given that these estimates are defined for the mean value of the dependent variable, they are substantially larger than those implied by the previous results in Table 3. However, the results of models that introduce state fixed effects provide an important validation of the prior cross-sectional results. In models that include both state and year fixed effects, the results suggest that court-ordered reforms increased median housing values by 8.7 percent, or .85 percent a year. These results, which are driven by within-state variation over time, are quite consistent with those results based on cross-sectional interactions.[42] The middle panel of Table 6 reports the results of similar estimations based on the median gross rent data from the 1970, 1980, and 1990 censuses. Again, models that omit state fixed effects overstate the capitalization of education finance reforms. However, in models that include state and year fixed effects, the simple binary indicator for court-ordered reform has a very small and statistically insignificant estimated effect. In contrast, the equation using the ''years since'' measure returns results very much in accord with the prior cross-sectional results, which suggests that residential rents rose by a statistically significant .23 percent in each year after the court-ordered reforms.

There are a number of possible explanations for the relative robustness of the ''years since'' measure in the model for residential rents in Table 6. In particular, these results could indicate that in data sets that include time-series variation a simple binary indicator for court-ordered reforms can be confounded with important dynamic adjustments. For example, many of the court-ordered reforms represented here began in the mid to late 1970s. A simply binary representation may fail to represent the limited effect that these reforms were likely to have on the data from the 1980 census. The results in the bottom panel of Table 6 lend support to this interpretation. In particular, these evaluations are based only on data from the 1970 and 1990 censuses—periods before and well after most of the court-ordered reforms. Unlike the prior results, the two-way fixed-effects model based on these data indicates that court-ordered reforms increased median gross rents by 3.9 percent, a statistically significant marginal effect quite close to those reported in Table 4. Notably, the estimated impact of the ''years since'' measure is robust across these sample changes. Overall, the results in Table 6 provide important evidence on the robustness of the capitalization results to unambiguous controls for unobserved state-specific attributes.

One of the limitations of the results presented in Table 6 is that they do not replicate the prior evidence of strong within-state heterogeneity in reform-driven capitalizations. This limitation is driven by the inability to match the older district-level census data to information on locally generated per-pupil revenues. However, this restriction can be circumvented with regard to the 1980 and 1990 census data that do include consistent NCES district identifiers. By matching 1980 and 1990 records, the 1980 records

[42] In particular, these marginal effects are quite close to those based on the third quintile in model 1, Table 3.

can be linked to information on their within-state ranking in locally generated per-pupil revenues. As emphasized earlier, important caveats are appropriate when employing historical district-level data files. In this context, it is particularly noteworthy that only 9,891 school districts could be matched across the 1980 and 1990 census files ($N = 19{,}782 = 2 \times 9{,}891$). Nonetheless, these qualified data facilitate additional robustness checks that can rely on the within-state and within-district variation in median gross rents over time. The evaluations based on these data focus on the ‘‘years since’’ measure since the results from Table 6 suggested that dynamic adjustments to the court reforms were potentially confounding in models that include the 1980 census data. More specifically, Table 7 reports the results of evaluations that employ these data to identify the heterogeneous effects of the years since court reform on median gross rents. Model 1 includes state and year fixed effects and the four quintile indicators. Models 2–4 present evidence on the robustness of these results by incrementally introducing suburban and rural indicators, quintile-specific year fixed effects, and, finally, interactions between state and year fixed effects. The latter two sets of included regressors are intended to capture in a highly unrestrictive manner the unobserved time-series variation specific to particular quintiles and to particular states. The results of these evaluations are strikingly consistent with those reported in Table 4. For example, these estimates indicate that median gross rents in the poorest school districts rose by 6–10 percent within 10 years of the court-ordered reform. Furthermore, these estimated effects exhibit a plausible monotonicity: they are generally smaller in the wealthier school districts, which received less new state aid.

However, these matched district-level data allow for even more severe regression controls by making it possible to include fixed effects specific to each of the 9,891 school districts. The remaining columns of Table 7 report the results of such fixed-effects specifications. Like the prior evaluations, model 5, which includes district and year fixed effects, does suggest that education finance reforms were significantly capitalized into median gross rents. Furthermore, the estimated effects in the third and fourth quintiles have magnitudes consistent with the prior evaluations. However, the estimated effects in the fourth and first quintiles are, respectively, larger and smaller than those based on prior specifications. Model 6, which introduces quintile-specific year fixed effects, generates results similar to those from model 5. For example, this model suggests that in the poorest school districts median gross rents rose by 2.5 percent within 10 years of court-ordered reform, while over the same period median gross rents for districts in the fourth quintile increased by 10 percent. Though the estimated capitalization in the poorest districts is relatively small, it is still statistically distin-

TABLE 7

MEDIAN GROSS RENTS AND YEARS SINCE COURT REFORM (Matched 1980 and 1990 Census Data)

Independent Variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Quintile 4 × years since court reform	.0054	.0045	.0042	.0021	.0102	.0100	.0022
	(.0012)	(.0011)	(.0013)	(.0013)	(.0012)	(.0013)	(.0013)
Quintile 3 × years since court reform	.0068	.0055	.0057	.0045	.0057	.0063	.0026
	(.0011)	(.0010)	(.0010)	(.0012)	(.0010)	(.0011)	(.0012)
Quintile 2 × years since court reform	.0060	.0045	.0048	.0036	.0054	.0061	.0027
	(.0011)	(.0010)	(.0010)	(.0013)	(.0011)	(.0011)	(.0012)
Quintile 1 × years since court reform	.0098	.0061	.0064	.0063	.0021	.0025	.0024
	(.0011)	(.0010)	(.0010)	(.0013)	(.0009)	(.0010)	(.0012)
R^2	.690	.751	.751	.767	.947	.947	.962
Urbanicity indicators	No	Yes	Yes	Yes	No	No	No
Year fixed effects × quintile indicators	No	No	Yes	Yes	No	Yes	Yes
State fixed effects × year fixed effects	No	No	No	Yes	No	No	Yes
District fixed effects	No	No	No	No	Yes	Yes	Yes

NOTE.—These results are based on the 1980 and 1990 census data from 19,782 matched school districts. Heteroskedastic-consistent errors are reported in parentheses. Models 1–4 include an intercept, binary indicators for quintiles 1–4, state fixed effects, and a year fixed effect. Models 5–7 also include a year fixed effect.

guishable from zero. But a more general concern is that the results of models 5 and 6 exhibit a somewhat implausible monotonicity since they suggest that the districts that received relatively few new state resources (those in the fourth quintile) substantially capitalized the court-ordered finance reforms. This pattern is not necessarily unreasonable since these districts may more efficiently convert new resources into educational outcomes valued by parents. However, the results of model 7 provide suggestive evidence that the sensitivity of these magnitudes may instead simply reflect specification error. More specifically, in model 7, which introduces year fixed effects specific to each state, the estimated amounts of capitalization in the wealthier districts are substantially smaller.[43] Notably, the estimated capitalization in the poorest districts appears quite robust. Overall, the results based on the district fixed-effects models indicate that the new resources generated by education finance reforms were capitalized into median gross rents. However, since some of these estimates are smaller than those based on other specifications, they can be understood as conservatively small bounds. But since these fixed-effects estimates also exhibit sensitivity to the set of included regressors, the varied results could simply reflect the influence of some collinearity. As evidenced by the sharply increased R^2, the introduction of 9,891 district fixed effects did remove a considerable amount of sample variation. In addition, the relatively small amount of capitalization evidenced in these models could plausibly reflect the exacerbated attenuation biases associated with the presence of measurement error and the introduction of such a large set of fixed effects.[44]

C. The Degree of Capitalization

These evaluations have suggested that the new resources made available to poorer school districts by court-ordered education finance reforms were reflected in increased housing values and residential rents. These novel findings provide important evidence that the court rulings in favor of education finance reform had their intended impact: increases in school quality for the poorer school districts that received new resources. However, the capitalization of these reforms also suggests that they may have had unintended distributional consequences. For example, for those who continued to rent housing in school districts that benefited from the court reforms, increased rents imply that, to some extent, they bore the financial burden of

[43] The interactions of state and year fixed effects that are introduced in model 7 are highly significant regressors.

[44] Zvi Griliches & Jerry A. Hausman, Errors in Variables in Panel Data, 31 J. Econometrics 93 (1986).

increased school quality. The empirical relevance of such unintended distributional consequences hinges critically on the degree of capitalization. The reduced-form estimates presented here allow us to construct some qualified evidence on the relative magnitude of this burden. Furthermore, a comparison of the magnitudes reported here to those in prior capitalization studies provides an important opportunity to evaluate the plausibility of this study's empirical results.

One particularly useful point of reference is the extensive literature on the capitalization of local property taxes. The results of these studies have varied widely with evidence of weak or partial capitalization as well as evidence of full or overcapitalization.[45] However, nearly all prior research employed data with variation both in taxes and in public services. Given that the quality and quantity of public services may be inherently difficult to observe but will have a positive covariance with property taxes, conventional evidence on the degree of tax capitalization may reflect important downward biases.[46] Oded Palmon and Barton A. Smith recently addressed this issue by exploiting a unique data set that had considerable tax variation but little variation in public services.[47] They found evidence that the degree of tax capitalization is relatively large (roughly 60 to over 100 percent).[48] The identification strategy exploited here is similarly well suited to circumventing the difficulties presented by the spurious correlation between local taxes and public services. This is because the new resources generated by education finance reforms constitute a putative increase in school quality without an observed change in the local tax burden. Given this, we would expect the degree of capitalization evidenced here to be within the high range of prior estimates.

However, a comparison of the degree of capitalization reported here with that in prior studies is not entirely straightforward. Given an assumption about household discounting, the present value of local tax differentials has an explicit monetary equivalent. But the present value of differentials in school spending will also depend on other heterogeneous factors like family

[45] See, for example, Timothy J. Gronberg, The Interaction of Markets in Housing and Local Public Goods: A Simultaneous Equations Approach, 46 S. Econ. J. 445 (1979); John Yinger *et al.*, Property Taxes and House Values: The Theory and Estimation of Intrajurisdictional Property Tax Capitalization (1988); and Raymond M. Reinhard, Estimating Property Tax Capitalization: A Further Comment, 89 J. Pol. Econ. 1251 (1981).

[46] For a more detailed discussion, see Oded Palmon & Barton A. Smith, New Evidence on Property Tax Capitalization, 106 J. Pol. Econ. 1099 (1998).

[47] See *id.*

[48] The estimated degree of capitalization depends on the assumed discount rate for households. Yinger *et al.*, *supra* note 45, assumed a rate of 3 percent; Palmon & Smith, *supra* note 46, assumed discount rates of 3 and 6.5 percent.

size, age, and the value placed on school quality.[49] For example, using the conservative estimates implied by model 2, we can infer that court reforms increased annual per-pupil spending by 9.7 percent in the poorest school districts (that is, $385 = .097 × $3,973). The value placed on the increased stream of school spending is likely to vary widely across households. Nonetheless, a comparison of this spending increase to the magnitude of the observed increase in capitalization is still illustrative and important. This comparison can be made most readily with the reduced-form results for residential rents since they can be used directly to generate an annualized present value for the capitalized reforms. For example, the results of model 2 suggest that in the poorest districts of reform states, monthly residential rents rose by at least 7.7 percent (that is, $23.72 = .077 × $308), or roughly $285 a year (that is, 12 × $23.72). These calculations suggest, therefore, that a fairly large share of the new per-pupil school spending (that is, $285/$385 = 74 percent) was capitalized into the residential rents of the poorest school districts. Evaluating the amount of capitalization into median housing values requires further assumptions. The results presented here (model 2) indicate that in the poorest school districts of reform states, median housing values rose by at least 8.2 percent (that is, $3,794 = .082 × $47,271). The relationship between the increased annual per-pupil spending in these districts ($365) and the increased median housing value in these districts ($3,794) depends critically on the household discount rate and the horizon over which this spending is valued. Under the standard assumptions that the lifetime of the house is large and the relevant discount rate is 3 percent, the value of the annual increase in per-pupil spending would be $12,833 ($385/.03). However, with a discount rate of 6.5 percent, the increased school spending would be valued at $5,923 ($385/.065). In these two examples, the implied capitalization ranges from 30 percent ($3,794/ $12,833) to 64 percent ($3,794/$5,923) of the present value of the new per-pupil spending.[50]

While the exact degree of capitalization is clearly sensitive to important assumptions, these rough calculations indicate that the evaluation results presented here are plausibly sized. Notably, other studies that relate school quality to property values also report similarly sized effects. For example, Sandra E. Black found that parents were willing to spend $3,948 more for a house in a school district with test scores that are 5 percent higher.[51] Simi-

[49] Furthermore, in the presence of production inefficiencies, a dollar of school spending would be valued at less than $1.

[50] The results from model 1 suggest that housing values rose by 16.9 percent in these districts. The implied degree of capitalization could then be 100 percent or more depending on the discount rate assumption.

[51] Black, *supra* note 10.

larly, William T. Bogart and Brian A. Cromwell found that the net-of-tax value of public schools in the Cleveland area differed by as much as $2,171.[52] But, more generally, this evidence that the capitalization of education finance reforms was nontrivially large underscores the potential empirical relevance of the unintended distributional consequences. In particular, the relatively large amount of capitalization suggests the intended redistributive impact of the court reforms may have been sharply attenuated by the private Tiebout responses of others ‘‘voting with their feet’’ in search of high-quality public schools.

One relatively minor but noteworthy caveat to these interpretations is that the full distributional implications of the court-ordered reforms may not be apparent in these data since they are aggregated at the district level. More specifically, the impressions left by evaluations based on these aggregated data might be somewhat incomplete if there were heterogeneous responses among the different types of housing within districts. For example, if the new resources were capitalized only into the relatively wealthy communities within a district, private Tiebout responses may not have attenuated the redistributive intent of court-ordered reforms quite as dramatically. However, the robustness of the capitalization results in the market for rented residential properties suggests this caveat may be overdrawn. Nonetheless, any future research that can exploit more disaggregated data will provide more definitive evidence on this question.

IV. Conclusions

Over the past 25 years, court-ordered education finance reforms have constituted one of the most ambitious attempts to redistribute resources and economic opportunity. Recent research has shown that these reforms were successful in achieving their initial goal: increasing the educational resources available to poor communities. However, there is no guarantee that the increased resources available to poor school districts increased school quality and subsequent economic opportunities. This empirical study provide novel evidence on this question by addressing whether these new resources were capitalized into housing values and residential rents. The results of these evaluations indicate that in the school districts that benefited the most from new state resources, there were dramatic Tiebout responses consistent with increases in school quality. In particular, in the poorest school districts (that is, those that received the most new aid), median housing values and residential rents rose by at least 8 percent. The capitalization of court-ordered education finance reforms provides compelling evidence

[52] Bogart & Cromwell, *supra* note 10.

that the new resources did increase school quality. However, the existence of private Tiebout responses also raises important new questions about the ultimate distributional implications of the court-ordered reforms. In particular, these results suggest that private responses to the court-ordered reforms may have sharply attenuated the normative goals that initially motivated the extensive state-level litigation on education finance.

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