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The Effect of State Fiscal Reform on Population Heterogeneity

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Abstract

This paper tests whether state fiscal policy alters neighborhood income homogeneity. One implication of the Tiebout model is that within-community homogeneity declines as a result of an exogenous decrease in the ability of jurisdictions to set local tax and expenditure levels. The property tax revolt and the school finance equalization reform of the 1970s and 1980s offer a test of the role of state fiscal reform on aggregate population sorting behavior. The results show that fiscal reform, especially tax and expenditure limitation laws and property tax reform, results in a small but significant decrease in neighborhood income homogeneity.

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I. Introduction

During the 1970s and 1980s, many states enacted laws to reduce property tax burdens, limit overall tax and expenditure levels, and shift public school revenue from local to state sources. By reducing reliance on local tax bases, these fiscal reforms led to less local tax and spending discretion. A further consequence of these reforms could be to shift the distribution of public service packages that communities offer, and, with it, lead to a reoptimization of household location decisions. This paper examines the effects of these reforms on population homogeneity, looking specifically for evidence that states with reform measures encountered larger increases in the dispersion of neighborhood income distributions (e.g. fewer poor households in the poorest neighborhoods and fewer wealthier households in the wealthiest communities) relative to states with no reform history. As such, this paper is a test of one implication of the Tiebout model; within community homogeneity decreases as a result of an exogenous decrease in the ability of jurisdictions to set local tax and expenditure levels.

Change in household location patterns is not a goal of fiscal reform. But it could be a byproduct that has important implications for public policy. Fernandez and Rogerson (1996) note that policies that increase the fraction of wealthier households in poorer communities are welfare enhancing because they increase the quality of education and decrease tax rates in every community. Benabou (1996) outlines a model whereby, under certain conditions, community income integration increases long-run growth.[1] If nonlinear peer effects like those described in

[1] The theoretical effects of community segregation on welfare and productivity are also discussed in Benabou (1993), Durlauf (1995,1996), Fernandex and Rogerson (1996), and Lundberg and Startz (1994). However, see Kremer (1996) for a critique of the importance of community sorting on income inequality.

Summers and Wolfe (1977) and Crane (1991) exist, it could produce further benefits from the dispersion of the population.

The move to reduce local tax burden has at least two different motivations. School finance reform was initiated by activists in the late 1960s and early 1970s out of concern for inter-school district disparities in public school funding that arise from the wide dispersal of U.S. income and wealth across neighborhoods.[2] The first state court to accept the arguments of school finance proponents was California, in *Serrano v. Priest.* The final decision, handed down in 1976, upheld a 1971 ruling that found school finance systems need to be fiscally neutral, meaning expenditures per pupil could not be related to a school district's wealth.[3] Shortly after California's financing scheme was overturned, public school funding from local revenue sources declined from over 60 percent to approximately 25 percent of total education outlays.

Other cases followed in the 1970s but with mixed results. In 1973, the U.S. Supreme Court overturned a Texas lower court ruling by denying to recognize education as a fundamental right nor allowing district wealth per pupil to be used as a suspect class. Within months, the New Jersey state supreme court concurred with the U.S. Supreme Court but still found the state's school system violated state constitutional requirements of a "thorough and efficient" public education system. The conflicting nature of state court decisions continued throughout the 1970s and 1980s; by 1990, school finance cases were decided in 26 states but only 12 overturned the

[2] For a full history, see Odden and Picus (1992) and LaMorte (1989).

[3] For a detailed discussion of the theoretical implications of equalizing aid, see Ladd and Yinger (1994), Oakland (1994), and Reschovsky (1994). A number of studies have explored the effects of these spending reforms on the level and growth of school funding (Downes and Shah 1995, Silva and Sonstelie 1995, Dye and McGuire 1996), the distribution of funding (Downes 1992, Bradbury 1993, Hoxby 1996), private school enrollment (Downes and Schoeman 1994), and the performance of students (Downes 1992, Figlio 1996, Downes, Dye, and McGuire 1996).

current financing system on the grounds that it either violated the state education clause, the equal protection clause or both.[4]

The mixed results of the courts resulted in proponents taking their case to state legislatures. By 1990, 27 states enacted school finance changes via the legislature. Many of these states were those in which judicial action failed. Three states even implemented new financing formulas without any action by the courts or legislatures. Evidently, there is no standard path to finance reform. This is not a trivial distinction. As Downes and Shah (1995) and Hoxby (1996) note, there may be important differences in how reforms are enacted. For example, Downes and Shah argue that court-ordered reform has tended to allow less local discretion and larger reductions in inter-district inequities than legislated reform. States that overturned school finance schemes through the courts saw, on (unweighted) average, local education revenue sources decrease by over 10 percentage points between 1970 and 1990. By comparison, states that used the legislature to pass reform policies decreased local education revenue shares by approximately four percentage points, and states with no official funding reform increased local revenue share by 0.4 percent. Therefore, distinguishing heterogeneity in reform activity is an important component of analyzing school funding reform effects on population patterns.

[4] These issues are being fought in 21 or so states during the 1990s. For descriptions, see LaMorte (1989), Harvard Law Review (1995a), Harvard Law Review (1995b). The politicization of this issue is well summarized in recent articles in the Chicago Tribune and New York Times. The Tribune article began "If the experience of other states that have tried to overhaul their school-funding formulas is any indication, Governor Jim Edgar's dramatic new effort to narrow the gap between rich and poor Illinois districts while rolling back property taxes will be tricky to pull off." (Haynes and Martinez 1996). An August 12, 1996 article in the New York Times highlighted the concern among wealthy neighborhoods. "Many parents in affluent New Jersey suburbs are worried that Governor Whitman's new school financing plan threatens the very reason they were willing to pay a premium for their houses and high property taxes: a quality education...The debate has shifted away from the needy school districts to the wealthy suburbs."

A second motivation of the fiscal reforms was simply to decrease taxes and government activity in what became dubbed the "taxpayer revolt." Beginning around 1979, many states imposed a variety of such statutory and constitutional limitations on local fiscal policy. The laws were designed to control and reduce property taxes, limit the growth of government and public spending, and improve fiscal accountability.[5] Tax and expenditure limitation laws (TELs) constrained overall spending and taxing at the local and state level by placing ceilings on tax rates, capping total revenue or expenditure levels, or restricting allowable growth to a function of inflation and population changes. Property tax relief was limited to local property tax collection. Although these laws typically allowed overriding mechanisms, such as voter overrides and exemptions, in most states the laws are binding. Figlio (1996) shows that the degree to which tax and spending constraints bind can play a role on local spending autonomy and, therefore, population distributions.

This paper analyzes a national dataset of census tracts to see if populations within states with these fiscal reform measures reacted differently than populations within states with no such reform. Reform is shown to have a limited impact on the stability of communities. While the raw data suggest a consistently significant impact of fiscal reform on population dispersion, especially among low income groups, parametric results that control for neighborhood characteristics are mixed. There is some evidence of significant dispersion effects on low income populations in states with tax and expenditure limitation laws and property tax reform but no impact from school finance equalization. None of the fiscal reforms have a consistently

[5] For details on state-specific rules and general policy descriptions, see Mullins and Cox (1995). Preston and Ichniowski (1991), Joyce and Mullins (1991), Poterba and Reuben (1995), Figlio (1996), and Dye and McGuire (1996) study the effectiveness of these laws on government financing. See Dye and McGuire for a summary of the literature.

significant impact on high income populations. Furthermore, only property tax reform appears to decrease overall neighborhood income variance.

The paper proceeds as follows. Section II sketches a model of fiscal reform and household location decisions. The narration emphasizes the potential ambiguity of the model's prediction. The census and state reform data used is outlined in section III. Section IV presents initial descriptive statistics on fiscal reform's impact on neighborhood income dispersion. A parametric strategy and resulting findings are described in section V. Finally, section VI concludes with a brief discussion and ideas about future research.

II. A Sketch of a Model

In the classic Tiebout (1956) model, jurisdictions compete for residents by offering packages of local public goods and taxes. Households choose communities that offer the most ideal combination. Aggregate sorting patterns depend on the composition of households and the range of packages offered by communities within a jurisdiction. In the extreme case, where all households have equivalent income and tastes for community amenities and each community is a perfect substitute, housing prices will be the same everywhere. Households randomly select a place to live and never move thereafter. At the other extreme, if there are two types of communities (high-quality and low-quality school districts) and two types of households (high and low tastes for education), households sort into communities based on taste for education.[6] Some residents might underconsume housing and other neighborhood-specific amenities in order to obtain higher-quality educational services. Other residents (say, those without children) might

[6] For a formal model, see Epple and Romer (1991).

move to low-quality school areas and overconsume housing and other neighborhood-specific goods.

Suppose there is a change in the package of amenities from, say, the school finance reform policies described in the previous section. Public schools are financed by several sources of revenues: federal grants, state taxes, and local taxes. These school reform plans shift financing away from local sources and towards state-based revenues. As a result, a community with a smaller tax base encounters an increase in public service expenditures, and a wealthier community experiences a reduction in local tax revenue used to spend on their public schools. In a perfectly mobile world, if there is heterogeneity in preferences within communities, this change could lead to community switching by residents who previously over or underconsumed neighborhood-specific goods.[7] Furthermore, among higher income neighborhoods, the diminished ability of local jurisdictions to determine revenue and spending levels might reduce the need to use zoning and other minimum income requirements to overcome the free rider problem.[8] As a result, there could be an increased flow of lower and middle income households into wealthier communities. Even with transaction costs, community switching could occur if, all else equal, the expected utility difference between the new and old neighborhood after the

[7] There is very little direct evidence on the impact of school quality on migration decisions. Jud and Bennett (1986) and Bartik, Butler, and Liu (1992) find that changes in local public school quality affect household mobility patterns. Furthermore, recent growth studies, such as (Glaeser et al 1992), argue educational spillovers can influence city formation and growth, suggesting a role for average education levels, and thus probably school quality, as an impetus to migration. Glaeser's model is tested and confirmed in Rauch (1993). In a similar vein, some researchers have tried to confirm the impact of local public service quality on housing prices. For example, Katz and Rosen (1987) find that community growth controls affect housing prices in a sample of San Francisco communities. There is also a series of papers that use whether fiscal services are capitalized into housing prices as a test of the Tiebout hypothesis. See Mieszkowski and Zodrow (1989) for a summary of the literature.

[8] See Mieszowski and Zodrow (1989) for a detailed description and critique of the role of zoning and minimum income requirements in obtaining Tiebout equilibrium in multidistrict models.

reform occurs outweighs the cost of moving. If I assume that this household is a representative consumer, a model of aggregate demand can be estimated.

However, there may be intervening factors that diminish the size or even reverse this prediction. As emphasized by Oates (1981), Clotfelter (1983), and Lamdin and Mintrom (1996), households might not be as "light on their feet" as the Tiebout model implies. On the supply side, jurisdictional choice in public service provisions may be limited. On the demand side, school funding schemes might not matter enough to instigate household movement. The impact of school quality on children's success remains a debated issue, but the empirical evidence suggests a weak relationship between school financing and student performance, especially relative to family factors.[9] If such a money-achievement link is wanting, it is likely that fiscal reforms will have few consequences on residential location decisions and, thus, the dispersion of households.

Furthermore, households with high tastes for educational services might find alternative ways to fund schools. This "bake sale" effect can offset state-mandated funding reforms and enable those with high income and high taste for educational services to continue to sort from the rest of the population. Downes (1992) suggests that such an effect played a role in the lack of education funding changes found in post-*Serrano* California. Likewise, as noted in Dye and McGuire (1996), the median voter model predicts that property tax and expenditure rule changes

[9] See Hanunshek (1986,1996), Card and Krueger (1992), Betts (1995a,1995b), and Altonji and Dunn (1995). Likewise, recent evidence on the impact of school finance reforms on student performance is mixed. Figlio (1996) finds that the performance of tenth graders is lower in states with revenue or expenditure limits, but, when controlling for unobserved jurisdictional effects, Downes, Dye, and McGuire (1996) find no decline in student performance.

will be overturned in states with voter override mechanisms. In this case, these limitation laws will have no effect on local spending or tax levels.[10]

Another possibility is that wealthy households opt out of the public school system after local discretion is reduced beyond some threshold. Downes and Schoeman (1994) show that California's increase in private schooling can be partly attributed to the state's education finance reform package. However, it is not clear if increased private schooling increases or decreases sorting behavior. Bearse, Glomm, and Ravikumar (1996) hypothesize that centralized schooling could result in an equilibrium where those with preferences for low taxes and low educational services -- the wealthiest (since they are opting out of the public school system in the model) and poorest households -- live together. However, Fischel (1993) argues a decline in local funding discretion could reduce general support for educational services, making it less likely that low and middle income residents will react to policy shifts.

Therefore, assuming there exists a range of heterogeneity in preferences for public services and other neighborhood-specific goods, fiscal reform measures like school reform, tax and expenditure limitation laws, or property tax relief, might affect location patterns by altering amenity packages between communities. However, the magnitude and perhaps even the sign of this effect is ambiguous given the reactions of households to the policy changes and the substitutability of neighborhoods within a given jurisdiction.

[10] The competing model predicts budget-maximizers will be reigned in by these laws, and therefore they will have consequences on actual spending practices. Dye and McGuire find compelling evidence for this government levitation model among Illinois counties.

III. Data

The Census

Detailed distribution data are needed to study neighborhood income homogeneity. The only source for such information at the neighborhood level is the census. The census data are derived from extracts created by researchers with the PSID and CIESIN, an environmental data clearinghouse in Michigan. These researchers extracted over 200 variables from the 1970 to 1990 STF3A census data files.[11] The STF3A database contains information on demographics, income, housing, mobility, education, and employment for a number of geographic levels, the smallest of which are census tracts, block numbering areas (BNAs), and enumeration districts (EDs). The unit of analysis in this study is the census tract or BNA, the basic statistical reporting unit in metropolitan areas.[12] Taking into account natural and manmade boundaries and population characteristics, local committees design census tracts to represent "neighborhoods." On average, tracts consist of about 4,000 people, but may range between 2,500 and 8,000 people.

The final sample includes 29,120 metropolitan tracts that are linked between 1970 and 1990.[13] For comparison purposes, the 1970 tracts that are split into multiple tracts by the 1990 census are included by taking the weighted average of the 1990 tracts and comparing this average to the original 1970 tract's composition. On average, the 3,355 tracts that split between 1970 and 1990 were divided into 2.67 tracts each by 1990. Therefore, roughly 8,900 additional 1990 tracts are included in the 29,120 tract sample.

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[11] The 1970 and 1980 extracts are from the PSID and the 1990 extract is from CIESIN.

[12] From here on, tracts mean census tracts and BNAs.

[13] To merge the censuses, two main problems emerge: the consistency of tract boundaries and the consistency of variables over time. Both problems are discussed in more detail in Aaronson (1996). The former problem arises because tract boundaries change across decades, particularly for rapidly growing areas. To see if the latter comparability problem is serious, a number of simple tests were performed in Aaronson. The results suggest that the comparability of neighborhood income and race characteristics across census years is a minor issue.

I consider the effects of state fiscal reform on several measures of neighborhood income dispersion. The first two dependent variables are measures of the sorting behavior of wealthy and poor households. In particular, I calculate the fraction of poor households that live in the poorest neighborhoods of each state and the fraction of the wealthiest households that live in the wealthiest households of each state. Poor households and neighborhoods are defined as those that fall in the bottom quintile of their state's income distribution. Likewise, wealthy households and neighborhoods are defined as those that fall in the top quintile of the state's income distribution. Therefore, when using these measures of poor and wealthy sorting as dependent variables, the sample size is approximately 5,800 census tracts or roughly twenty percent of the full 29,120 tract sample.

To calculate these variables, I use the census' income brackets to compute each tract's fraction of households that fall into a particular state income quintile.[14] The income brackets are used to compute state income quintile breakpoints for each of the census years. To compute the breakpoints, it is assumed that the cumulative distribution function of individuals within an income bracket is linear. This assumption is likely to be incorrect if, for example, the income distribution is log-linear. Distributional assumptions are fairly innocuous for the 1970 data because the bands of the income categories are narrow. But, for the 1990 data, there are fewer income categories and thus less exacting estimates of income deciles when linear interpolation rules are employed.

[14] In 1970, the income categories from $0 to 10,000 are delineated by $1,000. Above $10,000, the categories are $10-12,000, $12-15,000, $15-25,000, $25-50,000, and $50,000 plus. In 1980, the income categories from $0 to $30,000 are delineated by $2,500. Above $30,000, the categories are $30-35,000, $35-40,000, $40-50,000, $50-75,000, and $75,000 plus. In 1990, the categories are $0-10,000, $10-15,000, $15-25,000, $25-35,000, $35-50,000, $50-75,000, $75-100,000, and $100,000 plus.

The first two measures examine the fraction of poor and wealthy residents in poor and wealthy neighborhoods. The next two dispersion measures use average household income in these poor and wealthy communities. The average income measures capture additional information on the characteristics of these neighborhoods. This measure is taken directly from the census files. Again, this stratified sample consists of twenty percent of the full census tract sample.

Finally, in order to assess other parts of the neighborhood income distribution, a fifth measure accounts for overall dispersion in within-neighborhood income distributions. Because the census does not report a measure of within-neighborhood household income variance, I calculate such a term using the census income brackets. However, assumptions must be made about the distribution of household income within brackets. For the purpose of this calculation, it is assumed that the household income is log normally distributed but other assumptions are also tried and used in the computations to follow.[15] Fortunately, the results are not sensitive to these distributional assumptions, and therefore I report results that assume a log normal distribution. Analysis with this variable is done on the full census tract sample.

To account for heterogeneity in neighborhood evolution, controls in the analysis include census tract information on racial composition, education levels, and family composition, such as fraction of households with children and with female heads of household. Table 1 reports descriptive statistics of these characteristics for 1970 and 1990.

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[15] I thank Lew Segal for providing the maximum likelihood program used to compute log normal variances from the census income brackets. Alternative variance measures were also computed assuming uniform distribution within brackets and using the population mass directly above and below each income bracket as a weighting scheme.

State Reform History

Reform history is developed from a number of sources. State rules on local property tax limits and TELs are obtained from Mullins and Cox (1995) and Poterba and Reuben (1995). Eighteen states passed TEL legislation in the 1970s and 1980s. Property tax limitations are much more common, dating back to the 1880s. However, laws that are decades old are less likely to be binding. Therefore, I concentrate on the 27 states that enacted property tax limits between 1970 and 1988.

A comprehensive list of state judicial and legislative action on school revenue through 1990 sources was obtained from LaMorte (1989), Odden and Picus (1992), and Downes and Shah (1995). Following the arguments of Downes and Shah (1995), the main school finance classification breaks down reform experience by court or legislative action. These authors argue that court-ordered reform has tended to allow less local discretion and larger reductions in inter-district inequities. Legislated reform tends to keep more local discretion. Consequently, these different paths are compared separately against the no reform states. It is hypothesized that the effect of school finance equalization on population sorting will be larger in states with judicial reform relative to states with legislated reform.

Correlation Between State Fiscal Laws

Table 2 reports the correlation between these various rules and the incidence of reform policies. These correlations are not well estimated but they do provide some rough information about a state's experience with constraining taxing and spending.[16] Surprisingly, they show that

[16] The correlations are simple Pearson coefficients and thus do not adjust for the variable being discrete. Sample size is the 50 states.

there is little significant correlation between passage of these different reform plans. Perhaps this is because their purposes often overlap. Such an explanation might explain why recent passage of property tax laws is less likely in states with school finance reform enacted through the legislature.

The top row also displays the correlation between 1970 to 1990 growth in local education revenue and state school finance reform experience.[17] This revenue variable is correlated with the school finance reform indicators in a clear way. There is a significant correlation of -0.34 between court reform status and local revenue shares, and a 0.24 correlation between no reform status and local revenue shares, but no correlation between local revenue shares and legislative reform status. Therefore, while legislative reform seems to eliminate the increase in local revenue shares over this time period, it does not result in a decrease in financing formulas. The revenue share data might contribute more understanding to this study if such subtleties can be uncovered. However, while revenue figures are a more direct measure of the actual change in a state's tax source and, consequently, local spending abilities, the measure could be more prone to endogeneity issues.

IV. Initial Descriptive Statistics on Neighborhood Income Dispersion

This section investigates the distributional properties of the raw census data to reveal whether population heterogeneity differences exist by state reform experience. In particular, I look for evidence that there is a significant difference in the change in neighborhood income

[17] State changes in the local share of education revenues between 1970 and 1990 are calculated from the three year average of local revenue shares around 1970 and 1990. The data are from the 1969-1971 and 1989-1991 issues of *Revenues and Expenditures for Public Elementary and Secondary Education*, published by the National Center for Education Statistics at the U.S. Department of Education.

dispersion after fiscal reform is enacted. Table 3 analyzes the across-neighborhood variance of household income, stratified by year and reform experience. This table displays several measures of the variation of low and high income household location. Decreases in a state's across-neighborhood location variance is consistent with more population dispersion by income level. Figures 1 and 2 display this variance measure by graphing detailed income distribution changes after the sample has been stratified by the average income of the community.

Table 3 reports several measures of neighborhood income sorting, stratified by state fiscal policy. The results in this table are from analysis on the full sample of 29,120 census tracts. Column (1) shows the coefficient of variation (CV) of the fraction of the state's poorest ten percent of urban households. Columns (2) to (4) display the same computations for the poorest quintile of households and the wealthiest quintile and decile of households. Column (5) reports the CV of neighborhood average income. These figures are calculated pre-reform (1970) and post-reform (1990) for eight groupings of census tracts: all, school finance through a court decision, school finance as a result of legislative action, no school finance, state tax and expenditure limitation, no state tax and expenditure limitation, property tax reform since 1970, and no property tax reform since 1970. The third row in each grouping represents the change in variance between 1970 and 1990. By using 1990 as the post-reform period, the full effects are extended several years after most reform is implemented. This delay is especially critical since residential location patterns are likely to respond slowly to any policy change.[18] Asterisks represent significance tests of the difference between reform state variance and no reform state

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[18] Nearly every state reform studied occurred by the early 1980s, leaving at least five years for the impact to resonate. As a result, adjusting for time delay by introducing a reform variable that measures years since passage has no effect on the results.

variance measures.

The results suggest that each of the state reform rules had a modest impact on neighborhood heterogeneity for certain groups of households. Among poor households, there is decreased locational variance (suggesting more dispersion across neighborhoods) in states that experienced school finance through the courts or legislature and in states with tax and expenditure limitation laws and property tax relief. In states with school reform, the fraction of households with income among the poorest twenty percent changed -0.5% and 0.2% for court and legislature-mandated states but increased 6.6% in states with no reform. These changes are statistically different from the no reform state change at the one percent significance level. In TEL states, the CV of the poorest 20 percent of households decreased 0.9%, while in states with no TEL, this variance measure increased 4.5%. No such difference is found in property tax reform states among the poorest 20 percent of households, but a significant change is found among the poorest 10 percent of households.

Among high income populations, TEL and property tax reform states exhibit decreased dispersion of the top twenty percent of households. However, only TEL state variance changes are statistically different from the same change among no reform states (5.8% versus 8.9%) between 1970 and 1990. Finally, only the property tax reform states exhibit significant changes in average income dispersion between 1970 and 1990 relative to no reform states.

Figures 1 and 2 display detailed distributional characteristics of low and high income neighborhoods. In figure 1, the sample includes those neighborhoods with 1970 average income among their state's lowest quintile. From this poor neighborhood sample, I calculate the fraction of individual households with income among the lowest through the highest quintile in the state

for 1970 and 1990. The figure shows the simple 1970 to 1990 percentage *point* change in the fraction of quintile one through five households for states that experienced a particular reform and those that did not. Figure 2 replicates these calculations for the sample of neighborhoods with 1970 average income among their state's highest quintile. A measure of decreased sorting can be ascertained by looking at the left-most bars in figure 1 (i.e. the change in the fraction of the poorest residents residing in the poorest communities) and the right-most bars in figure 2 (i.e. the change in the fraction of the wealthiest residents residing in the wealthiest communities). Arrow below (above) the bars imply that the reform states' change is lower (higher) than the no reform states' change at the five percent significance level.

Like the previous table, decreased sorting of low income households surface in states with TELs, school finance reform, and property tax reform. In each of these cases, there is a significantly lower drop relative to the no reform states in the fraction of the poorest households in the poorest neighborhoods. However, among the wealthiest neighborhoods, decreased sorting seems to occurs only in states with legislated school finance.

Overall, the impact of fiscal reform on neighborhood distributional characteristics in the raw data is somewhat mixed but suggests that decreased sorting of low income households may have occurred in states with property tax limitations, TELs, and school finance reform. There is some evidence of decreased persistence of high income populations in states with legislated school finance and property tax reform.

V. Empirical Strategy and Results

The previous section finds evidence that reform measures that limit local revenue

discretion affect neighborhood income homogeneity. However, without controlling for other heterogeneity in state and neighborhood characteristics, it is difficult to decompose the effects of reform measures from other factors. This section presents several different specifications that control for neighborhood- and state-specific heterogeneity and thus allow more detailed analysis of the stability of neighborhood income distributions.

The basic estimating equation relates growth in neighborhood income dispersion pre- and post-reform to characteristics of the neighborhood and an indicator for whether the state experienced a fiscal reform:[19]

$$(1) \qquad Y_{ist} - Y_{ist-1} = \alpha X_{ist-1} + \beta R_{st} + \varepsilon_{is}$$

where Y_{ist} is a measure of household income dispersion in neighborhood i of state s in period t, X_{ist-1} is a vector of log neighborhood characteristics in period t-1 -- such as family, education, and racial attributes -- that might influence the growth of a community, and R_{st} is a vector of dummies indicating whether state s passed a reform by year t. Period t is 1990 and period t-1 is 1970, before any school finance or tax reform occurs.

Because of concern raised by Moulton (1986) about the presence of a state specific error term on the efficiency of OLS estimates of (1), I employ a two-stage estimator outlined in Borjas (1987).[20] In the first stage, I estimate equations like (1) but substitute state dummies for the

[19] The estimating equation is obtained from a simple model of neighborhood demand. A household chooses a neighborhood maximizes utility. The utility value is a function of neighborhood characteristics, the package of public services and taxes, and individual characteristics. The individual moves if utility in the new location exceeds utility in the old location plus the cost of moving. One can derive a model of the probability of choosing a particular neighborhood, conditional on the income of the household (for example, see Aaronson (1996) and cites within). In this paper, we use data on the fraction of individuals of each household income type that chooses each community. A model of aggregate demand can be developed by assuming demand in each community corresponds to a representative household. Although this is a common assumption, it is not innocuous if tastes are heterogeneous within communities. See Downes and Schoeman (1994) for a critique.

[20] Borjas and Sueyoshi (1994) describe the estimator in more detail.

reform dummies.

$$(2) \qquad Y_{ist} - Y_{ist-1} = \alpha X_{ist-1} + \gamma S_s + \varepsilon_{is}$$

In the second stage, the parameters $\hat{\gamma}$ are regressed on the reform indicators R_{st}

$$(3) \qquad \hat{\gamma} = \phi R_{st} + \varepsilon_s$$

From (2) and (3), the variance of the errors is calculated as the scalar covariance matrix derived from OLS estimates of equation (3) plus the proportion of equation (2)'s variance matrix corresponding to the state dummy variable coefficients (V_{ss}).

$$(4) \qquad \Omega = \sigma_u^2 I_N + V_{ss}$$

The sigma term in (4) is used to compute GLS estimates of the reform effect ϕ. In the results presented below, I report both the GLS and OLS estimates.

This model is estimated for each reform indicator and the five different measures of Y. In table 4, the sample is stratified by the 1970 average household income of neighborhoods. Two samples are derived: a low income neighborhood sample that includes all census tracts that are among the poorest 20 percent in their state in 1970 and a high income sample of tracts that are among the wealthiest 20 percent in their state in 1970. Columns (1) to (2) display equation (3) using the fraction of households that are among the poorest 20 percent as the dependent variable and the low income neighborhoods as the sample. As a result, a negative sign on the reform parameters can be thought of as a decline in poor household clustering due to reform policies. Columns (3) to (4) do the same computation for wealthy households in wealthy communities. Columns (5) to (8) use growth in average household income as the dependent variable with the two stratified samples. For the average income regressions, decreased sorting is consistent with a positive sign in the low income population regressions and a negative sign in the high income

population regressions.

The results suggest no impact of school finance reform on neighborhood stability.[21] For court reform states, there is no evidence of decreased sorting of low or high income populations. Legislated school finance reform states show evidence of decreased high income sorting, but this effect is only marginally significant with the GLS estimator. In TEL states, there is stronger evidence of decreased persistence of low income households. The results suggest that low income sorting decreases 7.4% further in states that pass such laws. However, the impact on average income in these neighborhoods is insignificant. There is also a small, insignificant drop in high income population sorting that contributes to a significant 5.8% decline in high income neighborhood's average income. Among property tax reform states, there is a 5.3% drop in low household income homogeneity but no effect on average household income in these poor communities. No effect is found among high income populations.

Table 4 employs only part of the sample to explore population shifts resulting from fiscal reform. In table 5, I use a dispersion measure that accounts for the entire population distribution of neighborhoods across time.[22] Columns (1) and (2) report results when using the change in the variance of household income as a dependent variable. Unlike the stratified poor and wealthy dependent variables, this regression employs the full sample. The prior is that fiscal reforms increase within-neighborhood variance and therefore the expected sign on the reform parameters is positive. The results suggest that only the property tax reform states experience such an increase in neighborhood income variance. This finding is roughly the same size, although

[21] See appendix 1 for first stage results.

[22] This table uses the variance measure calculated using the log normal distribution. Other distributional assumptions used to estimate neighborhood income variance do not alter the results.

measured with less precision, for samples that include only poor and wealthy neighborhoods (columns 3 to 6). In combination with the results in table 3 that only the property tax reform states experienced significant changes in across-neighborhood *average* income variance, it appears that the strongest dispersion effects outside the tails of the income distribution occur in states that enact property tax relief.

Overall, the evidence is mixed. The strongest impact of fiscal reform on population dispersion appears in property tax reform and TEL states. However, the patterns are weak and not consistent among all income groups. Furthermore, after controlling for neighborhood initial conditions, no evidence supports the hypothesis that school finance equalization matters to population patterns.[23]

Endogeneity of State Rules

A common econometric concern in the school finance reform literature arises from the potential endogeneity of state reform experience. Most studies of state revenue laws assume limitations and funding reforms are exogenously-determined. However, while including a vector of neighborhood characteristics controls for some selection issues, reform-minded states may be those with more unobservable concern about education or education equity or with more extreme funding disparities due to population homogeneity within school districts. Therefore, if it can be shown that the states that experience reform have unusually high levels of pre-reform income sorting, endogeneity bias could be problematic.

[23] Income composition may not be the appropriate measure to identify the impact of fiscal reform on population homogeneity. In particular, a possible alternative is to look at the persistence of neighborhood education distributions since this variable might better capture heterogeneous household preferences for educational services. I ran regressions similar to those in table 4 and 5 but used the fraction of adults that are high school dropouts and college graduates in place of the fraction of poor and wealthy households. The sample was stratified to look at the 20 percent of neighborhoods with the highest fraction of dropouts and college graduates. I found no effect of increased educational dispersion from any of the fiscal reforms.

Table 6 lays out some evidence on this issue. This table reports 1970 neighborhood income dispersion statistics. Rows (1) and (2) show the fraction of poor residents that live in poor communities and wealthy residents that live in wealthy communities. Rows (3) and (4) display the average household income of these high and low income communities. As in the previous tables, the poorest households and communities are defined as those with average income among their state's lowest 20 percent and the wealthiest households' communities are the analogous 20 percent at the top end of their state's income distribution. These sorting measures are stratified by reform status, and an asterisk represents a significantly different value from the no reform states. Clearly, only the legislature-enacted school finance reform states begin with higher levels of low income homogeneity in 1970 (43.2% versus 41.3%). This contributes to a lower average household income in these states' low income neighborhoods relative to no reform states ($24,089 versus $25,286). TEL states also show lower average income in low income neighborhoods. However, no reform grouping is different in terms of high income homogeneity, whether measured by the fraction of high income households in high income neighborhoods or the average income in high income neighborhoods.

Row (5) stratifies standard deviation measures of log household income by reform status. Lower levels of within-neighborhood income variance might suggest higher levels of sorting pre-reform. In fact, lower variance is found in states with legislated school finance and property tax reform. Thus, with three of the five measures, the biggest endogeneity concern is potentially in states that enacted school finance through the legislature. The other three fiscal reform groups do not display significantly different sorting patterns prior to reform relative to states that never passed reform initiatives.

Solving the endogeneity problem is difficult. Past attempts have adopted an instrumental variables approach.[24] Instead, I devise comparison groups that sidestep the potentially endogenous decision. In particular, I study the importance of endogeneity in school finance states by using states that upheld school financing schemes as a control group for states that overturned school finance reform. Twenty-six states brought school finance reform suits to the courts in the 1970s and 1980s but only half upheld the constitutionality of state education clauses. Using only these states has the advantage of exploiting an arguably more exogenous event -- the ruling of the courts -- instead of the decision by advocacy groups or others to bring a case to the judiciary or legislature. If the courts base opinions solely on their interpretation of the state's education clauses, their decisions should be free of endogeneity related to the composition or tastes of the population. Furthermore, to isolate the effect of the reform, this alternative control group comprises only the subset of four states (Michigan, New York, Oregon, and Pennsylvania) that did not subsequently undergo legislative action.[25]

The results of this exercise are very similar to those in table 4 and 5 and are thus not reported. Because of the smaller sample sizes, the precision of the estimates decline substantially, but the inferences are essentially the same. There is little evidence of dispersion effects after enactment of school finance equalization reform.

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[24] For example, Figlio (1996) and Reuben (1995) employ a two-stage estimator that uses the presence of voter initiatives to predict fiscal reform implementation. The idea is that many tax and expenditure reforms result from ballot initiatives. Therefore, the existence of such laws should be correlated with the likelihood of finance reform laws but should have no independent effect on population patterns. Matsusaka (1995) notes that, in all but four states, ballot laws were enacted prior to 1915, well before the start of the sample period. However, the correlation between state ballot initiatives and school finance, TEL, or property tax relief is zero. As a result, this approach did not produce robust results.

[25] Seven other states upheld the constitutionality of state education clause but enacted reform later through legislative activity. In four states, court decisions were never reached. In all four of these cases, reform occurred later through the legislature.

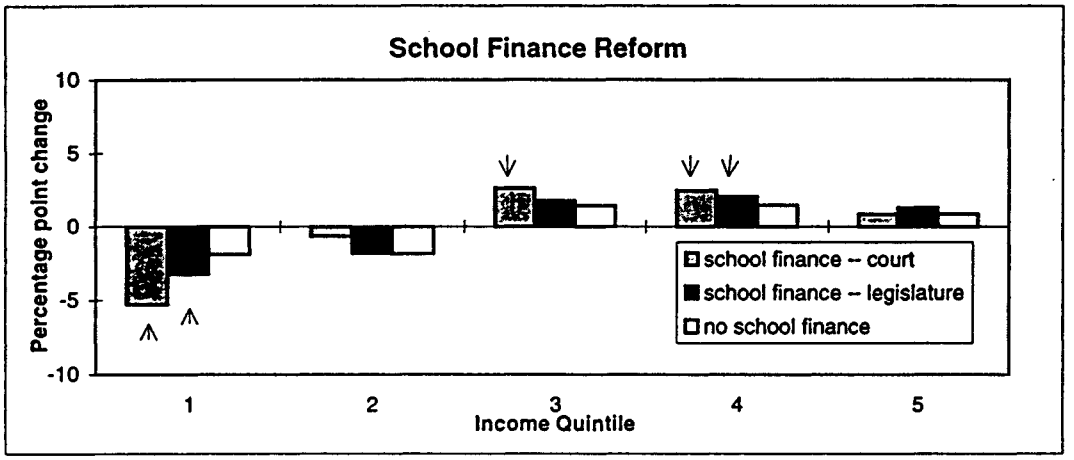
VI. Conclusions

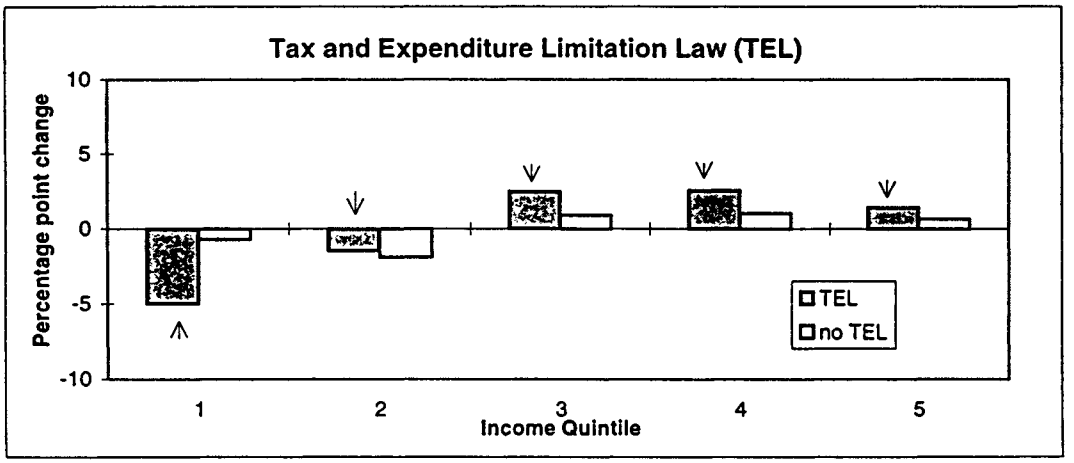
This paper tests whether tax and expenditure limitation laws, school funding reform, and property tax relief had an impact on community income homogeneity. It is hypothesized that restricting the latitude that local communities have in making funding decisions will diminish the level of household sorting behavior because equalizing public service funding is tantamount to integrating the tax bases of all communities. Furthermore, if funding inequities lead to an improvement in low tax base neighborhood services, the package of amenities and housing goods may change enough to encourage higher income families to reside in these communities. This effect is stronger if the reform leads to a simultaneous decline in the level of funding in higher income neighborhoods. However, intervening supply and demand factors could diminish the size or even reverse this prediction. For example, households with high tastes for educational services might find alternative ways to fund schools. Furthermore, there is evidence to suggest that school funding does not matter enough to instigate household movement.

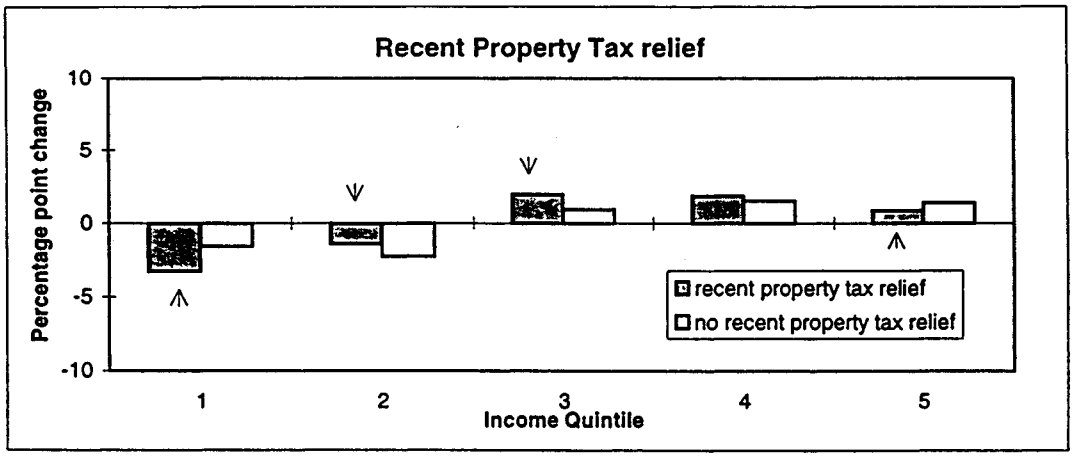
Using a national dataset of census tracts, reform is shown to have a limited impact on the stability of communities. While the raw data suggest a consistently significant impact of fiscal reform on population dispersion, especially among low income groups, the parametric results that control for neighborhood characteristics show less consistent results. There is some evidence of significant dispersion effects on low income populations in states with tax and expenditure limitation laws and property tax reform. Property tax reform appears to decrease overall neighborhood income variance. None of the fiscal reforms have an impact on high income populations.

A potential explanation for the lack of a consistent dispersion effect is that the unit of observation may be incorrect. Since funding bases are usually at other governmental jurisdictions, such as school districts, it is possible that larger reform effects will be found in more appropriate interdistrict comparisons. Therefore, one future research project will be to replicate this study at the school district level. A second project could involve analysis of housing price distributions. Using micro data from the National Association of Realtors or state listing services, I could test whether these reforms influence premiums that are paid for high (or low) quality public services. Third, it would be useful to concentrate on the experience of a few cities so that more detailed information, especially information on the bindiness of financing reform, state and local policy, and neighborhood and school district characteristics can be isolated. Furthermore, exploiting unusual natural experiments, like in Dye and McGuire (1996), where adjacent counties experience different fiscal reform initiatives would help control unobserved state-wide heterogeneity.

Figure 1
1970-1990 Growth in Fraction of Households by Income Quintile
States with Reform Versus States without Reform
Sample: Neighborhoods with Average Income in Lowest 20th Percentile in 1970

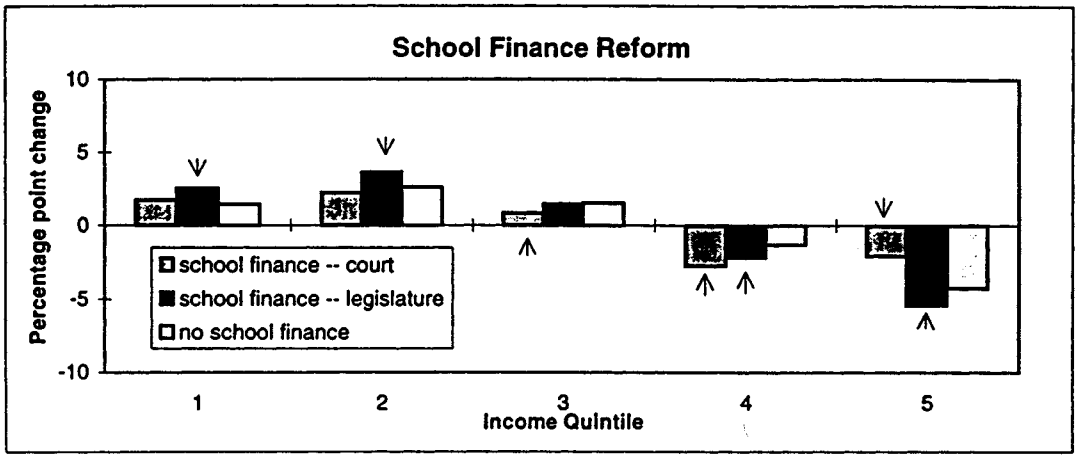


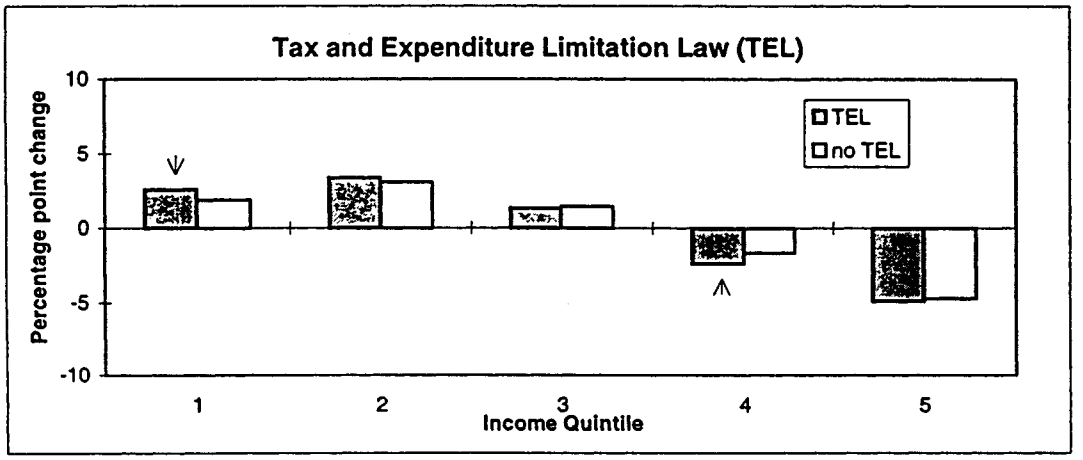


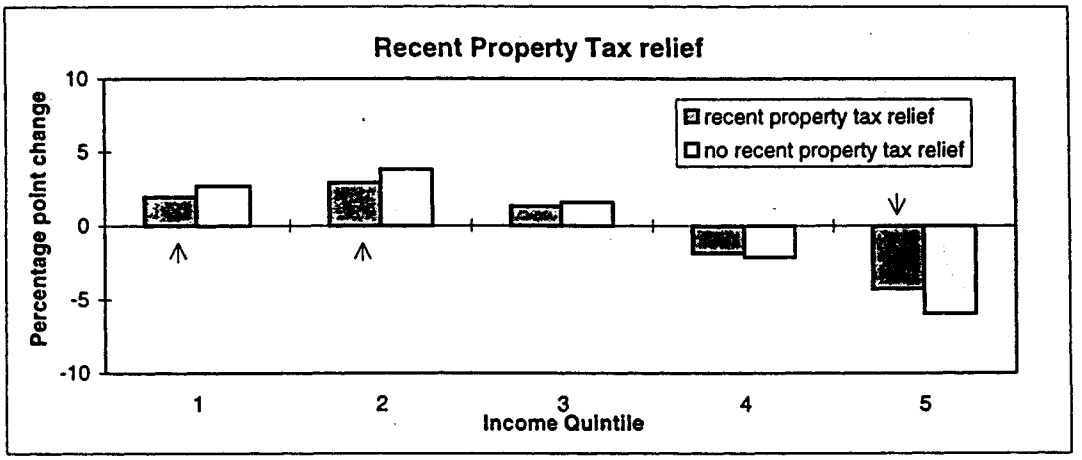


Note: ⩚⩛ = reform state change is lower (higher) than no reform state at the 5% sig. level.

Figure 2
1970-1990 Growth in Fraction of Households by Income Quintile
States with Reform Versus States without Reform
Sample: Neighborhoods with Average Income in Highest 20th Percentile in 1970







↑↓ = reform state change is lower (higher) than no reform state at the 5% sig. level.

Table 1
Descriptive Statistics on Neighborhood Variables (1
Weighed by Census Tract Population

	1970		1990	
	Mean (1)	Std Dev (2)	Mean (3)	Std Dev (4)
Fraction of households with income between: (2				
0-10th percentile	9.7	7.2	10.3	8.0
0-20th percentile	19.6	12.2	20.4	13.3
80-100th percentile	20.2	13.4	19.7	13.6
90-100th percentile	10.1	9.3	9.8	9.7
Average household income	40,209	14,279	46,302	22,654
Fraction White	90.0	21.2	76.7	27.2
Fraction Black	9.0	21.1	13.9	24.4
Fraction Hispanic	5.8	12.5	11.6	19.3
Fraction female household heads	10.4	7.2	18.3	12.6
Fraction female household heads with kids	10.8	9.0	10.2	8.8
Fraction families with kids	56.8	12.7	35.8	10.5
Fraction heads with education less than 12	43.7	17.8	24.3	15.3
Fraction heads with education equal to 12	32.3	8.3	28.3	8.8
Fraction heads with educ. between 13 -15	11.8	5.9	25.5	7.5
Fraction heads with education at least 16	12.2	10.5	21.9	15.3

Notes:
1) Sample size is 29,120 census tracts. This includes all tracts that are linked between 1970 and 1990. Tracts that split in 1980 or 1990 are summed and compared to the original 1970 tract. See text for further details.
2) Decile and quintile breakpoints are determined by a full sample of census tracts in 1970 and 1990. Therefore, the fractions in each decile do not necessarily add up to 10.

Table 2
Correlation Between Education Finance and Tax Reform Laws (1
Unweighted

	State enacted school finance reform:			Change in local revenue share, 1970-90	Number of states=1
	Court-mandated	Legislated	No reform		
	(1)	(2)	(3)	(4)	(5)
Change in local revenue share, 1970-90	-0.340 **	-0.017	0.240 *	1.000	
State passed a property tax limit law since 1970	-0.026	-0.127	0.129	-0.210	27
State passed a tax and expenditure limitation law	-0.026	0.191	-0.189	-0.091	18
Number of states = 1	9	27	14		

Notes:
** (*) = significantly different from 0 at the 5 (10) percent level.
1) All variables except the change in local revenue shares are discrete. The correlations do not adjust for this.

Table 3
Aggregate Neighborhood Income Sorting, 1970 and 1990, by State Fiscal Policy
Coefficient of Variation of Neighborhood Income Characteristics (1

State reform experience	Coefficient of variation: Fraction of households in state income decile/quintile 0-10 (1)	0-20 (2)	80-100 (3)	90-100 (4)	Average income (5)
(1) All census tracts					
1970	76.4	63.1	72.4	101.1	38.3
1990	77.7	64.5	77.8	112.3	51.8
Percentage Change	1.8%	2.3%	7.6%	11.1%	35.2%
(2) School finance through courts					
1970	73.5	62.0	71.1	102.4	37.6 **
1990	78.0	61.6 **	77.2	113.0	51.1
Percentage Change	6.1%	-0.5% **	8.6%	10.3%	35.9%
(3) School finance through legislature					
1970	77.7	63.8	73.6	100.4	38.2 *
1990	77.4	63.9 *	79.4	114.5	53.0
Percentage Change	-0.5% *	0.2% **	7.9%	14.0%	38.9%
(4) No school finance reform					
1970	74.0	62.2	71.6	103.3	40.0
1990	76.7	66.3	75.4	109.0	51.3
Percentage Change	3.6%	6.6%	5.4%	5.5%	28.3%
(5) State tax and expenditure limit					
1970	74.1 **	62.2	73.6	100.8	38.0
1990	73.5 **	61.6 **	77.9	111.7	51.7
Percentage Change	-0.8% **	-0.9% **	5.8% *	10.8%	36.0%
(6) No state tax and expenditure limit					
1970	77.9	63.7	71.5	101.3	38.5
1990	80.6	66.5	77.8	112.7	51.9
Percentage Change	3.5%	4.5%	8.9%	11.3%	34.6%
(7) Recent property tax limit					
1970	75.7 *	62.8	72.4	101.4	38.8
1990	76.1 **	64.3	77.5	112.3	51.9
Percentage Change	0.6% *	2.5%	7.1%	10.8%	33.7% *
(8) No recent property tax limit					
1970	78.2	63.8	72.4	100.3	37.0
1990	81.7	64.9	78.7	112.3	51.4
Percentage Change	4.5%	1.7%	8.8%	11.9%	38.9%

Notes:

1) The astericks represent a test that the variance of the no reform states exceeds the variance of the reform states (ie. row 1 v. 4, row 2 v. 4, row 5 v. 6, and row 7 v. 8) for a given year at the 5 percent (*) or 1 percent (**) level.

Table 4

Effect of State Reforms on Neighborhood Income Distributions (1

Fraction Poor and Average Income in Poor Neighborhoods and Fraction Wealthy and Average Income in Wealthy Neighborhoods

	Dependent variable: 1970-90 growth in							
	the fraction of households with income in:							
	0-20 quintile		80-100 quintile		average household income			
Neighborhood group:	Low inc nghbrhds (2		High inc nghbrhds (2		Low inc nghbrhds (2		High inc nghbrhds (2	
Estimation method: (1	OLS	GLS	OLS	GLS	OLS	GLS	OLS	GLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A (3								
School finance	0.019	-0.024	0.021	0.039	0.035	0.039	0.070	0.069
-- courts	(0.059)	(0.045)	(0.062)	(0.049)	(0.057)	(0.054)	(0.046)	(0.043)
School finance	0.024	0.049	-0.051	-0.068 *	0.035	0.016	0.009	-0.005
-- legislature	(0.044)	(0.035)	(0.047)	(0.038)	(0.043)	(0.041)	(0.035)	(0.033)
PANEL B (3								
TEL state	-0.087 **	-0.074 **	-0.030	-0.024	0.013	0.016	-0.067 **	-0.058 *
	(0.041)	(0.030)	(0.046)	(0.037)	(0.042)	(0.040)	(0.034)	(0.031)
PANEL C (3								
Property tax reform	-0.069 *	-0.053 *	0.023	0.035	-0.033	-0.030	-0.043	-0.031
	(0.041)	(0.031)	(0.045)	(0.037)	(0.041)	(0.039)	(0.033)	(0.031)
expected sign	-	-	-	-	+	+	-	-

Notes:

** (*) = significant at the 5 (10) percent level.

1) Parameters are derived from the two-stage estimator described in the text. The results reported are from a regression of the state dummy coefficients from a first stage regression on the reform indicators. See appendix 1 for first stage results. The GLS estimates follow the method outlined in Borjas (1987).
2) Low (high) income neighborhoods have average household income among their state's bottom (top) quintile in 1970. Sample sizes are 5,818 for the low income neighborhood sample (cols. 1,2,5,6) and 5,784 for the high income neighborhood sample (cols 3,4,7,8).
3) Panels A,B, and C are from separate regressions.

Table 5
Effect of State Reforms on Neighborhood Income Distributions (1
Household Income Variance

	Dependent variable: 1970-90 growth in household income variance					
Neighborhood group:	All nghbrhds		Low inc nghbrhds (2		High inc nghbrhds (2	
Estimation method: (1	OLS	GLS	OLS	GLS	OLS	GLS
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A (3						
School finance	0.001	0.002	-0.003	-0.004	0.010	0.018
-- courts	(0.015)	(0.014)	(0.024)	(0.021)	(0.019)	(0.014)
School finance	0.001	-0.001	0.005	-0.002	-0.004	-0.010
-- legislature	(0.011)	(0.011)	(0.018)	(0.016)	(0.014)	(0.011)
PANEL B (3						
TEL state	-0.006	-0.006	-0.025	-0.021	0.007	0.009
	(0.011)	(0.011)	(0.017)	(0.015)	(0.014)	(0.011)
PANEL C (3						
Property tax reform	0.020 *	0.020 **	0.019	0.018	0.012	0.020 *
	(0.010)	(0.010)	(0.017)	(0.015)	(0.014)	(0.011)
expected sign	+	+	+	+	+	+

Notes:
** (*) = significant at the 5 (10) percent level.

1) Parameters are derived from the two-stage estimator described in the text. The results reported are from a regression of the state dummy coefficients from a first stage regression on the reform indicators. See appendix 1 for first stage results. The GLS estimates follow the method outlined in Borjas (1987).
2) Low (high) income neighborhoods have average household income among their state's bottom (top) quintile in 1970. Sample sizes are 5,808 for the low income sample (cols. 3,4) and 5,771 for the high income sample (cols 5,6). These numbers are slightly lower than table 3.4 because some variance estimates did not converge. The total sample of all neighborhoods is 29,010.
3) Panels A,B, and C are from separate regressions.

Table 6
Are Initial Neighborhood Income Distributions Different in States with Fiscal Reform
1970 Neighborhood Income Characteristics, by State Reform Experience

1970 Neighborhood characteristics (1	School finance: court (1)	School finance: legislature (2)	School finance: none (3)	TEL (4)	No TEL (5)	Property tax reform (6)	No property tax reform (7)
Fraction poor in poor neighborhoods	41.3	43.2 *	41.3	42.5	42.4	42.3	42.8
Fraction wealthy in wealthy neighborhoods	40.6	40.0	40.4	39.9	40.1	40.0	40.1
Average household income in poor neighborhoods	26,405 *	24,089 *	25,286	24,455 *	25,038	24,952 *	24,391
Average household income in wealthy neighborhoods	62,971	58,855 *	61,625	58,803 *	60,814	60,271	59,286
Standard deviation of log household income in all neighborhoods (2	0.692	0.691 *	0.695	0.694 *	0.687	0.689 *	0.693

Notes:
*=significant relative to no reform states at 5% level.
1) Poor and wealthy neighborhoods refer to those census tracts with average income in 1970 among their state's lowest and highest quintile.
2) Assumes log normal income distribution. See text for an explanation.

Appendix 1
First Stage Fiscal Reform Regressions

Dependent variable:	Growth in poor households in poor neighborhoods		Growth in wealthy households in wealthy neighborhoods		Growth in average household income of poor neighborhoods	
Independent variables (1	Coefficient (1)	Standard error (2)	Coefficient (3)	Standard error (4)	Coefficient (5)	Standard error (6)
Constant	0.169	(0.158)	-2.831	(0.140)	0.184	(0.094)
Fraction Black, 1970	0.000	(0.003)	-0.018	(0.004)	-0.006	(0.002)
Fraction Hispanic, 1970	0.013	(0.004)	-0.009	(0.004)	-0.008	(0.002)
Fraction female heads, 1970	0.102	(0.017)	0.113	(0.013)	-0.041	(0.010)
Fraction female head w/ kid, 1970	0.052	(0.012)	-0.022	(0.009)	-0.038	(0.007)
Fraction families with kids, 1970	-0.267	(0.025)	0.332	(0.021)	-0.033	(0.015)
Fraction adults HS dropouts, 1970	0.035	(0.024)	0.021	(0.017)	0.037	(0.014)
Fraction adults w/ educ=12, 1970	0.100	(0.019)	0.075	(0.021)	0.017	(0.011)
Fraction adults w/ educ 13-15, 1970	0.025	(0.012)	0.214	(0.021)	0.019	(0.007)
Alaska	0.324	(0.212)	0.078	(0.210)	-0.224	(0.126)
Arizona	-0.074	(0.087)	0.020	(0.079)	-0.068	(0.052)
Arkansas	-0.104	(0.111)	-0.050	(0.106)	0.181	(0.066)
California	-0.263	(0.060)	0.268	(0.053)	0.062	(0.036)
Colorado	-0.129	(0.083)	0.026	(0.074)	-0.075	(0.049)
Connecticut	-0.057	(0.072)	0.346	(0.065)	-0.077	(0.043)
Deleware	-0.266	(0.119)	0.237	(0.111)	0.124	(0.071)
DC	-0.353	(0.111)	0.437	(0.107)	0.224	(0.066)
Florida	-0.027	(0.065)	0.199	(0.060)	0.008	(0.038)
Georgia	0.093	(0.076)	-0.143	(0.070)	-0.113	(0.045)
Hawaii	-0.398	(0.112)	0.324	(0.103)	0.097	(0.067)
Idaho	-0.047	(0.210)	-0.093	(0.210)	-0.046	(0.125)
Illinois	-0.065	(0.061)	0.199	(0.056)	-0.180	(0.036)
Indiana	-0.045	(0.069)	0.175	(0.063)	-0.167	(0.041)
Iowa	-0.010	(0.093)	0.121	(0.085)	-0.223	(0.055)
Kansas	-0.053	(0.088)	-0.010	(0.080)	-0.073	(0.052)
Kentucky	-0.023	(0.092)	0.128	(0.085)	-0.091	(0.054)
Lousiana	-0.007	(0.075)	0.092	(0.069)	-0.132	(0.044)
Maine	-0.026	(0.130)	0.217	(0.122)	0.017	(0.077)
Maryland	-0.012	(0.068)	0.222	(0.062)	0.017	(0.040)
Massachusetts	-0.162	(0.065)	0.208	(0.060)	0.017	(0.039)
Michigan	-0.289	(0.074)	0.403	(0.067)	-0.257	(0.044)
Minnesota	-0.089	(0.074)	0.167	(0.067)	-0.120	(0.044)
Mississippi	-0.018	(0.126)	-0.255	(0.122)	0.082	(0.075)
Missouri	-0.058	(0.071)	0.202	(0.066)	-0.127	(0.042)
Montana	-0.087	(0.184)	0.038	(0.180)	-0.270	(0.109)
Nebraska	-0.063	(0.109)	0.090	(0.101)	-0.199	(0.065)
Nevada	-0.133	(0.127)	0.039	(0.119)	-0.182	(0.075)
New Hampshire	0.150	(0.210)	0.094	(0.193)	0.112	(0.125)
New Jersey	-0.204	(0.063)	0.343	(0.058)	0.097	(0.038)
New Mexico	-0.193	(0.140)	0.024	(0.133)	0.144	(0.083)
New York	-0.166	(0.059)	0.313	(0.053)	-0.013	(0.035)

Appendix 1
First Stage Fiscal Reform Regressions

Dependent variable:	Growth in poor households in poor neighborhoods		Growth in wealthy households in wealthy neighborhoods		Growth in average household income of poor neighborhoods	
Independent variables (1	Coefficient	Standard error	Coefficient	Standard error	Coefficient	Standard error
	(1)	(2)	(3)	(4)	(5)	(6)
North Carolina	-0.036	(0.077)	-0.127	(0.071)	0.092	(0.045)
North Dakota	0.081	(0.293)	0.082	(0.327)	-0.318	(0.174)
Ohio	-0.022	(0.061)	0.239	(0.056)	-0.250	(0.036)
Oklahoma	-0.239	(0.083)	-0.092	(0.076)	-0.028	(0.049)
Oregon	-0.136	(0.084)	0.126	(0.076)	-0.071	(0.050)
Pennsylvania	-0.079	(0.061)	0.337	(0.055)	-0.077	(0.036)
Rhode Island	-0.196	(0.095)	0.251	(0.088)	0.028	(0.056)
South Carolina	-0.031	(0.087)	0.128	(0.081)	0.227	(0.052)
South Dakota	-0.142	(0.184)	0.067	(0.180)	0.032	(0.109)
Tennessee	-0.084	(0.077)	0.039	(0.071)	-0.022	(0.046)
Texas	-0.029	(0.063)	-0.025	(0.057)	-0.013	(0.038)
Utah	0.418	(0.186)	-0.058	(0.180)	-0.339	(0.111)
Virginia	0.005	(0.074)	-0.075	(0.068)	0.022	(0.044)
Washington	-0.177	(0.074)	0.183	(0.067)	-0.070	(0.044)
West Virginia	-0.097	(0.106)	0.248	(0.097)	-0.061	(0.063)
Wisconsin	-0.015	(0.070)	0.277	(0.064)	-0.267	(0.042)
Adjusted R-squared	0.097		0.139		0.157	
Sample size	5,819		5,785		5,819	

Appendix 1
First Stage Fiscal Reform Regressions

Dependent variable:	Growth in average household income of wealthy neighborhoods		Growth in variance of household income, all neighborhoods	
Independent variables (1	Coefficient (7)	Standard error (8)	Coefficient (9)	Standard error (10)
Constant	0.055	(0.071)	0.141	(0.008)
Fraction Black, 1970	-0.008	(0.002)	0.000	(0.000)
Fraction Hispanic, 1970	0.000	(0.002)	0.000	(0.000)
Fraction female heads, 1970	0.006	(0.007)	0.002	(0.001)
Fraction female head w/ kid, 1970	0.016	(0.005)	-0.003	(0.001)
Fraction families with kids, 1970	0.001	(0.011)	-0.004	(0.001)
Fraction adults HS dropouts, 1970	-0.040	(0.009)	-0.016	(0.001)
Fraction adults w/ educ=12, 1970	-0.101	(0.011)	-0.011	(0.001)
Fraction adults w/ educ 13-15, 1970	0.150	(0.011)	-0.001	(0.001)
Alaska	0.010	(0.108)	0.003	(0.011)
Arizona	-0.034	(0.041)	0.004	(0.004)
Arkansas	-0.025	(0.054)	0.005	(0.006)
California	0.163	(0.027)	0.005	(0.003)
Colorado	-0.089	(0.038)	0.014	(0.004)
Connecticut	0.254	(0.033)	0.029	(0.004)
Deleware	0.105	(0.057)	0.013	(0.006)
DC	0.206	(0.055)	0.005	(0.006)
Florida	0.071	(0.031)	-0.005	(0.003)
Georgia	0.016	(0.036)	0.017	(0.004)
Hawaii	0.168	(0.052)	-0.021	(0.006)
Idaho	-0.142	(0.108)	-0.017	(0.011)
Illinois	0.049	(0.029)	0.029	(0.003)
Indiana	-0.026	(0.032)	0.021	(0.004)
Iowa	0.019	(0.044)	0.012	(0.005)
Kansas	-0.063	(0.041)	0.006	(0.005)
Kentucky	0.012	(0.044)	0.024	(0.005)
Lousiana	0.012	(0.035)	0.020	(0.004)
Maine	0.214	(0.062)	0.029	(0.007)
Maryland	0.094	(0.032)	0.009	(0.003)
Massachusetts	0.217	(0.031)	0.046	(0.003)
Michigan	-0.039	(0.034)	0.024	(0.004)
Minnesota	0.053	(0.034)	0.020	(0.004)
Mississippi	-0.042	(0.062)	-0.006	(0.007)
Missouri	0.009	(0.034)	0.016	(0.004)
Montana	-0.177	(0.092)	0.004	(0.010)
Nebraska	-0.046	(0.052)	0.012	(0.006)
Nevada	-0.103	(0.061)	-0.001	(0.007)
New Hampshire	0.273	(0.099)	0.020	(0.011)
New Jersey	0.282	(0.030)	0.031	(0.003)
New Mexico	-0.149	(0.068)	-0.016	(0.007)
New York	0.169	(0.027)	0.032	(0.003)

Appendix 1
First Stage Fiscal Reform Regressions

Dependent variable:	Growth in average household income of wealthy neighborhoods		Growth in variance of household income, all neighborhoods	
Independent variables (1	Coefficient (7)	Standard error (8)	Coefficient (9)	Standard error (10)
North Carolina	0.004	(0.036)	0.008	(0.004)
North Dakota	-0.165	(0.167)	0.001	(0.016)
Ohio	-0.007	(0.029)	0.024	(0.003)
Oklahoma	-0.154	(0.039)	0.004	(0.004)
Oregon	0.005	(0.039)	0.001	(0.004)
Pennsylvania	0.165	(0.028)	0.023	(0.003)
Rhode Island	0.172	(0.045)	0.016	(0.005)
South Carolina	0.072	(0.041)	-0.011	(0.005)
South Dakota	0.013	(0.092)	-0.015	(0.010)
Tennessee	0.041	(0.036)	0.006	(0.004)
Texas	-0.026	(0.029)	0.013	(0.003)
Utah	0.030	(0.092)	-0.029	(0.010)
Virginia	0.081	(0.035)	-0.005	(0.004)
Washington	0.040	(0.034)	0.004	(0.004)
West Virginia	0.070	(0.050)	0.023	(0.005)
Wisconsin	0.023	(0.033)	0.010	(0.004)
Adjusted R-squared	0.218		0.062	
Sample size	5,785		29,010 (2	

Notes:
1) The missing state is Alabama. Vermont and Wyoming are not included because there were no metropolitan tracts in these two states in 1970.
2) Does not include approximately 100 tracts that did not converge in the MLE estimation of census tract variance. See text for explanation.

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