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State Fiscal Policy and Local Residential Sorting: Are Tiebout Voters Hobbled?

Martin Farnham[1]
University of Michigan

Purvi Sevak
Hunter College

March 2004

Abstract

While the Tiebout hypothesis has come under increasing empirical fire, studies have not convincingly ascertained whether weak Tiebout sorting is truly evidence against the hypothesis or simply evidence that the prevalence of centralized state policies removes the conditions necessary for fiscal sorting. In this paper, we explore the extent to which state fiscal policy pertaining to the school finance system, "hobbles" Tiebout voters—that is, reduces their incentive or ability to residentially sort based on fiscal characteristics. Using panel data on older households from the Health and Retirement Study, we examine changes in property tax liabilities and exposure to school spending experienced by households that move under different school finance regimes. We also use micro data on all households from the 2000 Census, to analyze differences in mobility rates, private school enrollment, and commuting times under different school finance regimes. We find some, albeit mixed evidence that school finance regimes that reduce local control and variation inhibit household sorting.

[1] Corresponding author (mfarnham@umich.edu), Ph.D. candidate, University of Michigan.

I. Introduction

Charles Tiebout's famous (1956) hypothesis on residential sorting and local public goods provision suggests that households, given a choice of competing communities, will vote with their feet to match themselves to the best possible bundle of local taxes and services. Through this decentralized process (and under strict assumptions) an economy will achieve efficient provision of local public goods.

A voluminous and growing empirical literature attempts to establish the extent to which Tiebout's theory describes reality. For example, a number of state-level studies of fiscal determinants of migration find evidence suggesting that fiscal characteristics such as welfare generosity, income taxes, and certain sales tax exemptions play a role in determining location choice (for example, Cebula, 1974; Conway and Houtenville, 2001). While these studies find evidence in favor of Tiebout sorting, the evidence across the literature is generally fairly weak.

A more recent study by Rhode and Strumpf (2003) provides striking evidence against the Tiebout hypothesis. The authors propose a model of fiscal sorting that allows for costly mobility. They argue that decreases in mobility costs should lead to more complete sorting across communities. Documenting the dramatic decline in mobility costs in the United States between 1850 and 1990, they proceed to show that little if any increase in sorting has occurred over the 140 year period that they examine. They argue that either the Tiebout model does a very poor job of explaining behavior, or that other factors have offset the effect of decreasing mobility costs. In another recent study, Farnham and Sevak (2004) use the Health and Retirement Study and a national panel of school-district-level fiscal data to show that empty-nest households, on average, engage in little Tiebout-consistent fiscal adjustment when they move.

A number of factors may explain the relative lack of Tiebout sorting found in these studies. Costly mobility is clearly one of these, though Rhode and Strumpf provide evidence that this is not the dominant factor. Farnham and Sevak provide evidence that job location constraints limit Tiebout sorting. In addition, various state policies may play a role in impeding residential sorting over fiscal variables.

In particular, state-level school finance systems are likely to have important effects on residential choice. Under a system of pure local financing of public schools, one would expect households to engage in more complete residential sorting by demand for education services than under an equalized system of state-financed public schools. To the extent that equalization of spending leads to equalization of quality, a topic of empirical controversy, the state-financed

education system should reduce the importance of education consumption in the residential location decision. In the extreme case of perfect equalization of school quality within a state, households with children are free to make location decisions based on other concerns, such as job location, local non-fiscal amenities, and housing quality.

Residential sorting by the elderly may be similarly affected by state-level school finance policy. To the extent that state policy equalizes *tax burdens* across locations, it will reduce the incentive for such households to sort away (at least within the state) from high spending school districts upon becoming an empty nest. State policy may also weaken the link between tax burden and location by replacing a system of local property-tax finance with one of state-income-tax finance. This also reduces incentives to engage in the form of lifecycle Tiebout sorting described in Farnham and Sevak (2004) and discussed below.

So while the Tiebout hypothesis has come under increasing empirical fire, studies have not convincingly ascertained whether weak Tiebout sorting is truly evidence against the hypothesis or simply evidence that the prevalence of centralized state policies removes the conditions necessary for fiscal sorting. In this paper, we explore the extent to which state fiscal policy, specifically pertaining to the school finance system, "hobbles" Tiebout voters—that is, reduces their incentive or ability to residentially sort based on fiscal characteristics. We focus on school finance systems for three reasons: (1) school spending is the single greatest component of local expenditures in the United States, (2) there is substantial state-level variation in the form of such systems, and (3) the effect of these state policies on community composition is interesting in and of itself.

The major motivation for trying to understand the effect of state policy on Tiebout sorting is to better understand the role of policy in efficient provision of local public goods. If policy is causing weak Tiebout sorting, this may imply a strong efficiency/equity tradeoff and might suggest reexamination of policies that tend to centralize the provision of local public goods. If on the other hand, other factors are to blame for weak Tiebout sorting (e.g. job location constraints, limited Tiebout choice, returns to local diversity, or costly mobility), equity gains from centralization may not be so costly.

In order to assess the effect of school finance equalization on residential sorting, we perform the following analyses. First, we link a panel of homeowners from the Health and Retirement Study to local fiscal data and test for the types of fiscal adjustment achieved by empty-nest movers. In particular, we focus on how this fiscal adjustment varies in the presence of school finance equalization. Second, using Census micro data we test for the effect of school

finance equalization on household mobility rates, with a particular emphasis on households at critical points in the Tiebout lifecycle—namely parents and recent retirees. Third, we test for the effect of equalization on commute lengths of parents. Fourth, and finally, we analyze the effect of equalization on private school attendance rates.

The evidence we present in this paper, while far from conclusive, suggests that school finance equalization *does* inhibit Tiebout sorting. We can not yet conclude, however, that it inhibits sorting to a substantial degree. We present evidence from the Health and Retirement Study that suggests that moves of empty-nesters are associated with weaker reductions in exposure to school spending and property taxes in the presence of school finance equalization. Evidence from micro data in the 2000 Census suggests that equalization may slightly diminish the mobility of parents, although it appears to increase the mobility of empty nesters. Evidence from the 2000 Census is mixed on the question of whether school finance equalization is associated with reduced travel time to work. Finally, evidence from the Census suggests that school finance equalization lowers private school attendance overall, but increases it among the rich.

The paper is organized as follows. In section II we review some of the theoretical and empirical literature on the Tiebout hypothesis and on school finance systems. Section III discusses some theoretical issues. In section IV we describe our data and empirical strategy. Section V presents empirical findings and section VI concludes.

II. Existing Literature

A number of papers have addressed the extent to which the Tiebout hypothesis holds true empirically. A substantial review of this literature can be found in Dowding, *et al.* (1994). Farnham and Sevak (2004) review some more recent papers, including a growing literature on fiscally motivated migration by the elderly. Rhode and Strumpf (2003) make a major contribution to the empirical literature on the Tiebout hypothesis with a paper demonstrating that completeness of residential segregation (by proxies for fiscal demands) does not appear to be linked to mobility costs. Farnham and Sevak (2004) make a useful contribution to this literature by matching a panel of households to local school finance data, and by observing the fiscal adjustments completed by empty-nest movers.[2] Theirs is the first national study of Tiebout

[2] The authors focus on the first move a household makes after becoming an empty-nest. They argue that demand for local school services should have fallen substantially since the household bought their home. Therefore the next move, in a Tiebout world, should be associated with significant downward adjustment in exposure to school expenditures and taxes.

sorting that employs fiscal data at the school district (school expenditures and revenues) and household (property tax liability) level. While they find significant fiscal adjustment consistent with lifecycle changes in local public good demand among *cross-state* movers, they find virtually no evidence of such fiscal adjustment among *within-state* movers.

A growing literature on the effect of school finance equalization on spending and educational outcomes has developed in recent years. Important contributions have been made by Nechyba (2003a, 2003b), using general equilibrium simulation models. These results have been particularly useful in addressing complicated issues involving the interaction of public and private school markets, the role of real estate markets in transmitting effects of policy, and the potential distributional effects of different policies. Nechyba (2003b) presents a review of theoretical and empirical contributions of general equilibrium simulation models, and compares the findings from this methodology with more analytical approaches.

A number of empirical studies have addressed the effect of school finance equalization. Murray, Evans and Schwab provide significant discussion of the history of court-ordered school finance equalization in the U.S. and provide evidence of reduced spending inequality as a result of such policies. Card and Payne (2002) provide evidence that equalization has reduced the dispersion of education spending. Hoxby (2001) controls more directly for the exact form of equalization policy by calculating tax prices for each school district. She provides evidence of the effect of school finance equalizations in different states on the mean and distribution of spending outcomes. Her findings contradict those of Murray, Evans and Schwab by suggesting that school finance equalizations tend to "level down" spending (i.e. reduce spending in wealthy districts more than they increase spending in the poor). She makes the important point that school finance equalizations differ dramatically across states and that reduction of these heterogeneous policies to a simple binary variable is generally inappropriate.

The intersection of the empirical literature on Tiebout *and* school finance equalization is small. To our knowledge, the only paper to substantially address the effect of school finance equalization on Tiebout sorting is that by Hilber and Mayer (2004). In this paper, the authors examine the concentration of poor households, households with children, and the elderly within and across states, using decennial Census data from 1970, 1980, 1990, and 2000. They calculate Herfindahl indexes of concentration for each state, and test for the effect of school finance equalization on Tiebout separation of the rich from the poor and the young from the old. They provide evidence of decreasing concentration of the poor and elderly within states with school finance equalization. They also present evidence that equalization leads to decreased enrollment

in private schools, and that households with children prefer to move to states with greater local control.

We take a different approach to the general question of the effect of school finance equalization on residential sorting by focusing on fiscal adjustment and mobility at the household level. The first part of our analysis focuses on the actual fiscal adjustment (changes in fiscal characteristics associated with moves) of households in the Health and Retirement Study. By matching older households with a strong incentive for fiscal adjustment to data on local fiscal characteristics we are able to observe whether such adjustments are hindered by the presence of school finance equalization. The second part of our analysis, also done at the household level, uses public use micro data from the 2000 Census to examine household mobility, job location constraints on sorting, and private school enrollment decisions.

While our approach requires significant refinement, we feel it offers certain advantages. Most importantly, we are able to control for substantial household-level heterogeneity. In addition, we attempt to use some different measures of school finance equalization that we feel may be more appropriately characterize equalization. Finally, by examining mobility decisions at critical points in the lifecycle, such as when a household has children or becomes an empty-nest, we feel we can more precisely estimate effects of school finance equalization on Tiebout sorting.

III. Theoretical Considerations

Household demand for local public services is likely to vary over the lifecycle. For example, when a family is young and has children, its demand for K-12 services is typically high relative to its demand for those same services upon becoming an empty nest. It is natural, therefore, to consider an extension of the Tiebout model that allows for lifecycle variation in demand, and therefore lifecycle residential sorting. In such a model with decentralized provision of local education services, households sort into school districts providing high levels of education services when they are young (unless they utilize private schools), and sort out of those districts to lower spending, lower tax districts when their children finish high school. While mobility costs will reduce the frequency of adjustment so that households are not always consuming their instantaneously optimal local fiscal bundle, we would still expect to see a relatively high level of Tiebout-related residential mobility concentrated among households with children entering and exiting school age. For a more detailed discussion of lifecycle Tiebout sorting, see Farnham and Sevak (2004).

If households engage in lifecycle Tiebout sorting under decentralized provision of education services, then measures to centralize education should diminish such sorting. For example, if a state were to shift its system of education finance from one of locally-determined property taxes to one of state-determined income taxes distributed equally across districts on a per pupil basis, then a household's tax burden would no longer be tied to its residential location. And the link between local service levels and location would be diminished.[3] Young households would be less likely to relocate based on differential school quality, because such differentials would be reduced. Older households would be less likely to relocate based on differential tax burdens, because such differentials (in this example) would be eliminated.

To the extent that households do engage in moves under school finance equalization, we should expect to find their fiscal adjustments, in dollar terms, to be smaller. This is for two reasons. First, the opportunity to adjust along fiscal dimensions should be diminished under a more centralized system of finance. And second, as cross-district differentials are reduced, the incentive to move for fiscal reasons is diminished. Therefore observed moves are more likely to be for non-fiscal reasons, and therefore less likely to be associated with systematic changes in the fiscal bundle.

Intimately related to a household's location decision is the issue of job location for working members of the household and private school options for the school-age members of the household. Farnham and Sevak (2004) discuss job location constraints on Tiebout adjustment. They find evidence that these constraints do limit fiscal adjustment of empty-nest movers. Hilber and Mayer (2004) also note the tradeoffs between proximity to work and school district quality. The idea that a household may need to locate further from work in order to obtain a more desirable fiscal bundle is a simple one. If school finance equalization reduces the differential between fiscal bundles across communities, it lowers the relative cost of locating nearer to work. Therefore we should expect to see equalization associated with shorter commute times.

Much of the literature on school finance policy addresses the interaction between public and private school markets. Theoretical discussions include Nechyba (2003a) and Hoxby (2001). Nechyba's structural general equilibrium simulations suggest that for most of the relevant parameter space, centralization leads to diminished private school attendance. This occurs as

[3] If educational output is a function of only spending, then the link between location and consumption of education services would be completely severed. Peer effects, parental inputs, and other factors almost certainly enter into the educational production function. Therefore, even under complete equalization of spending, quality of service is likely to vary across jurisdictions. However, as long as spending matters, the link between location and services should be weakened by equalization.

poor families with a strong taste for education respond to increased spending in poor schools by switching out of private schools, and as wealthy households experience a higher opportunity cost of private school attendance, due to capitalization of those spending increases into property values in poor districts and the reverse effect in wealthy districts. His model does not take into account the fact that centralization may reduce publicly available choices for households, providing an opening for private schools to more easily differentiate themselves and attract the children of parents unhappy with the changes brought about by equalization. Hoxby, citing this point, argues that private school attendance should rise as a result of equalization.

Empirical evidence points both ways. Downes and Schoeman (1998) find evidence that equalization in California led to increased private school enrollments, thereby supporting Hoxby's claim. Hilber and Mayer (2004) find evidence using national data that school finance equalization tends to be associated with declines in private school enrollment—consistent with the findings of Nechyba's simulations. Therefore the question remains an empirically open one.

In our empirical analysis below we consider school finance equalization as a possible explanation for the weak Tiebout sorting results among empty-nest movers found in Farnham and Sevak (2004). We test for a role of equalization in reducing fiscal sorting among a sample of households in the HRS, matched to local fiscal data. We also use Census micro data to test for direct effects of equalization on mobility. In particular we focus on the effect of equalization on the mobility of parents and recent retirees, both groups that we would expect to be engaging in lifecycle Tiebout moves under a decentralized system of finance. We also use Census data to explore the effect of equalization on commuting times and on private school enrollment.

IV. Data and Empirical Strategy

Health and Retirement Study panel

The first part of our analysis uses household-level data from the Health and Retirement Study (HRS). The HRS began in 1992 as a panel of households with a household member born between 1931 or 1941. For this analysis, we use interviews of these households taken every two years since 1992 until 2000. The HRS contains extensive data on income, wealth, work and health status, and other individual and household characteristics of both movers and non-movers of retirement age. Along with this rich set of individual and household characteristics, the survey codes geographic identifiers—ZIP-code, census tract, county and state codes—in each wave that

allow identification of state and local amenities, and fiscal and demographic characteristics over time. Unlike many household panel surveys, the HRS does not drop households that move.[4]

Census household data

We use data on households from the 5% Public Use Micro Sample (PUMS) of the 2000 Census of Population and Housing.[5] The file contains data on five percent of the U.S. population in 2000 or about 14 million individuals across 5,663,214 households. The data available in the PUMS was provided by households through the long form of the 2000 Census. The long form asks questions about the household's residence, tenure at the residence, family structure, and income. In addition, it asks about current school enrollment, work status, and commuting times for appropriate members of the household. Households also report whether they have moved since 1995 and, if so, they report their prior state of residence.

We limit the sample to households that lived in the U.S. in both 1995 and 2000 and we exclude households that live in Alaska, Hawaii, and Washington D.C. in either 1995 or 2000. We make these exclusions because we examine household behavior as a function of state policies in either their 1995 or 2000 state of residence. At this point we do not have consistent school finance data for Alaska, Hawaii and D.C

Table 1 contains summary statistics of the sample of Census households we use. Using these data, we examine the effect of school finance equalization on three household outcomes: within-state mobility between 1999 and 2000, private school enrollment of children in 2000, and commuting time in 2000. About 15 percent of households moved in the year prior to the Census, and 11 percent moved within the same state. Approximately seven percent of households are consumers of private school services. A household is denoted a consumer of private school services if any of its school-age children are enrolled in private school. The third outcome variable, commuting time, is the number of minutes spent commuting to work. If the household is a two-earner household, we consider the maximum commuting time across the two partners. Across all households, the average commute time is about 17.6 minutes. This number is low

[4] A great deal of effort is placed on retaining households that change residence, move into nursing homes, or dissolve. The reinterview rate is over 90 percent in each wave. For a full description of the creation of this dataset and summary statistics, see Farnham and Sevak (2004).

[5] We access the PUMS through www.ipums.org. Steven Ruggles and Matthew Sobek et al. *Integrated Public Use Microdata Series: Version 3.0* Minneapolis: Historical Census Projects, University of Minnesota, 2003.

because it is the average across all households, including those that are retired or out of the labor force for other reasons.[6]

We examine the effects of state policy variables on different demographic groups, some of which are particularly likely to be engaging in Tiebout-related fiscal adjustments. New parents, that haven't yet adjusted residential location to take into account their increased (or about-to-increase) demand for local education services are one example of a demographic group that is ripe for lifecycle Tiebout adjustment. In addition, parents whose children are entering primary or secondary school may wish to adjust location to improve their fiscal bundle as it relates to education services. Seven percent of the households in our sample are classified as new parents because their eldest child is age six or younger in 2000. The eldest child in six percent of households is age seven to ten in 2000 and in 16 percent of households the eldest child is age 11 to 18.

In contrast to parents, new retirees may be ripe for lifecycle Tiebout adjustment in the opposite direction. Empty-nest households likely prefer an area that spends less on public schools and those that have recently retired should have fewer constraints on their mobility because they are not in the labor force. Therefore the years immediately following retirement are likely prime years for fiscal adjustment. Seven percent of households are considered recently retired because the household head has retired in the past five years.[7]

We limit the sample to households whose head is age 25 or older. Because we place no upper limit on age, it is not surprising that the average age of household heads is 51.8 years. Just over 80 percent of household heads are white, 11 percent are black and eight percent identify themselves as having Hispanic ethnicity. Just over half of households in our sample are married and 18 percent are divorced. In our current analysis, we focus on homeowners, who make up about 70 percent of households in the sample.

State Equalization Variables

One of the challenges of studying the effects of school finance systems is that such systems are difficult to characterize with a small set of variables. Hoxby (2001) argues that states have a relatively small number of policy levers, but notes that fully characterizing a single state's finance policy may require several pages of computer code. While Hoxby provides various state-

[6] Our analysis of commuting times is limited to households with children, which should minimize the number of households who are not in the labor force.

[7] If the household is a married household, we consider the husband the household head The Census does not specifically identify retirement. We consider an individual retired if they are over age 50 and have withdrawn from the labor force.

level summary statistics based on this coding, these are not sufficient statistics for the school finance system in a given state. Rather than attempting to replicate Hoxby's work by coding school finance equalization formulas, we adopt a set of simpler measures to indicate whether—and in some cases, to what degree—a state has adopted a policy of school finance equalization. These measures include a dummy variable denoting the imposition of equalization, dummy variables denoting court-ordered versus legislatively-ordered equalization, a measure of the percent of school revenues in a state that are locally derived, and an index of school finance equalization that we construct, based on one developed by Card and Payne (2002).

Card and Payne derive their index of school finance equalization using the following procedure. They estimate the following equation for each state,

$$(1) \qquad S = \alpha_1 + \beta_1 I + \gamma_1 X + u ,$$

where S is state funding per pupil, I is household income, and X is a set of school district characteristics. Since equalization programs generally aim to give financial support to districts in inverse relation to district income or wealth levels, the estimate of β_1 should be negative. States with stronger equalization should have estimated values of β_1 that are more negative than those of states with weaker equalization. Our estimates of β_1 comprise a state-level index of school finance equalization.[8] We employ this index as a predictor of Tiebout mobility in our later analysis.[9]

We merge fiscal policy variables to households based on the state in which they lived in 1995 and the state in which they lived when the Census was conducted in 2000. Table 1 provides population-weighted means for these variables based on the 1995 state of residence. The Card-Payne equalization index has a mean value of -36. Seventy percent of residents live in states that have some form of school finance equalization. About one-third of those residents are in states with court-ordered equalization and two-thirds are in states with legislatively-imposed equalization. On average, 45 percent of educational spending in school districts is collected locally.

[8] School district controls in our calculation of the Card-Payne index include log of district enrollment, population density, race distribution, and the share of students in secondary school versus primary school.
[9] We are currently missing certain data for Nevada, Utah, and New Mexico that are required to construct the index. Therefore it is currently not available for those states.

State-by-state values for each of these indicators of school finance equalization are given in Table 2. We base the binary indicator variables for school finance equalization, court ordered equalization, and legislatively ordered equalization on Hoxby (2001), who in turn collects her data from a variety of sources. The measure of percent local revenues comes from the 1995 Common Core of Data.[10]

Empirical Strategy

We use several different approaches to examine the effect of school finance equalization on Tiebout sorting. We begin by examining actual fiscal adjustments experienced by movers in the HRS, and test for whether movers under equalization experience less fiscal adjustment than movers in states without equalization. The methodology here involves a simple extension of that employed in Farnham and Sevak (2004). In that paper, the authors test for evidence of lifecycle Tiebout sorting by observing the changes in fiscal bundle experienced by older movers. They hypothesize that households engaged in their first move since their youngest child reached age 18 should reduce their exposure to school spending and property taxes compared with non-empty-nest movers. To test this, the authors estimate a regression of the form given by

(2)

$$\Delta G_{ijt,t-1} = \Delta\alpha_{ijt,t-1} + \beta_1 M_{ijt,t-1} + \beta_2 (M * EN)_{ijt,t-1} + \beta_3 \Delta X_{ijt,t-1} + \beta_4 \Delta A_{ijt,t-1} + \sum_{t=1994}^{2000} \gamma_t \Delta Y_{t,t-1} + \sum_s \delta_s S_j + \Delta v_{ijt,t-1}$$

where i indexes household, j indexes locality, and t denotes time. They follow households from 1992 to 2000 and pool observations across years.

v is a standard disturbance term. ΔY is a series of dummy variables equal to one if the household observation refers to a given survey period. It is intended to capture time fixed effects. S is a series of dummy variables that absorb state-of-origin fixed effects. X is a vector of observable household characteristics that may be correlated with a household's propensity to move and taste for fiscal policy, including income level. A is the vector of local amenities and demographic characteristics intended to proxy for cost-of-living differences across locations.

G is the local fiscal variable of interest (e.g. property tax liability or school spending). M equals 1 if the household moves between t-1 and t. EN equals 1 if the moving household was an unadjusted empty nest just prior to its move. So the interaction term captures the additional adjustment associated with empty-nest moves. The omitted category is non-movers.

[10] Source: National Center for Education Statistics.

It should be emphasized that estimation of (2) is not meant to imply a causal relationship between moving and changes in fiscal characteristics. Rather the regression context is employed to control for cost-of-living differences across places. Coefficients are only meant to be interpreted as changes in fiscal variables that are associated with moves.

An additional interaction term, (*M*EN*SFE*) can be added to document the difference in fiscal adjustment between *EN* movers in states with and without a School Finance Equalization program. *SFE* can take the form of either a dummy variable or a continuous variable that proxies for equalization, such as the percent of school spending that is locally raised or an index of equalization. *SFE* denotes policy at the destination. *If* school finance equalization programs limit the ability of empty-nest movers to engage in lifecycle Tiebout adjustment away from high levels of school spending and associated taxes, then we should observe positive coefficients associated with greater equalization on this interaction term.

In addition to testing whether fiscal adjustment among movers is lower in the presence of equalization, we also directly test for the effect of equalization on mobility. We do this using 2000 Census PUMS data. If school finance equalization weakens the link between location and the consumption of education services, then under equalization households should be less inclined to time their moves to conform with critical points in the Tiebout lifecycle (i.e. around the time when the first child reaches school age and upon achieving empty-nest status). We are able to observe the age of children present in the PUMS households, so testing for the effect of equalization on mobility among families with young children is relatively straightforward.

Since we do not observe the age of children *absent* from the PUMS households, we can not cleanly designate households as unadjusted empty-nests, as we do in the HRS. Instead, we focus on mobility among the recently retired. Since many older households out of Tiebout equilibrium are likely to wait until retirement to engage in fiscally-adjusting moves (when residential choice will be less constrained by job location considerations) we should expect that many moves shortly after retirement will be Tiebout motivated. In the case of older households, to the extent that the link between *tax burden* and location is weakened by school finance equalization, such policies should reduce the incentive for older households to move out of the district where they sent their children to school. We therefore expect to find that equalization reduces the mobility of recent retirees.

It is important to note a theoretical ambiguity in the prediction regarding effects of school finance equalization on mobility of older households. One possible explanation for the lack of Tiebout-consistent fiscal adjustment by empty-nest movers, is that older households, while

preferring low levels of education spending, may prefer certain local amenities that are correlated with high levels of school spending. Examples include a low incidence of crime, a prevalence of good restaurants, etc. Many elderly migrants may be willing to bear high levels of local school taxes, in exchange for access to such amenities.[11] To the extent that equalization allows the elderly access to communities with desirable amenities without having to pay high levels of school taxes, it may greatly expand the set of desirable communities from which recent retirees may choose. This expansion of the choice set could actually *increase* the mobility of recent retirees. Also, to the extent that older households live in wealthier communities, equalization that *raises* the property tax burden on the wealthy may induce households to move that might have stayed put in the absence of equalization. So the effect of school finance equalization on the mobility of recent retirees remains an empirical question.

Farnham and Sevak (2004) find evidence that job location constraints limit fiscal adjustment among empty-nest movers.[12] To provide further insight into this question, we test for cross-sectional differences in commute times among households in states with school finance equalization versus states without. If households' location choices involve tradeoffs between distance-to-work and school district quality, *and* if equalization weakens the link between school quality and residential location, the presence of equalization in a state should reduce the commuting time of parents.

Finally, we test for cross-sectional differences in private school attendance by the presence or absence of school finance equalization. We do this by estimating a logit regression of the probability of attending private school on household characteristics and indicators for the presence of equalization. As noted above, the effect of equalization on private school enrollment is theoretical ambiguous and thus remains an empirical question.

V. Findings

Fiscal Adjustment Among HRS Movers

Table 3 replicates findings from Farnham and Sevak (2004).[13] Coefficients indicate the estimated change in four different fiscal variables associated with moves by households in the HRS. The authors hypothesize that if households engage in Tiebout sorting, there should be a

[11] In a perfect Tiebout world, a sufficient number of communities would form to allow each household the perfect residential bundle. Clearly, space constraints and the multidimensionality of the residential bundle preclude this possibility, in general.
[12] This is Table 8 from the paper. Table 9 in Farnham and Sevak (2004) provides evidence that job location constraints may inhibit Tiebout sorting.
[13] See Farnham and Sevak (2004) for a description of their dataset and summary statistics.

lifecycle component to it. Households, upon achieving empty-nest status, are assumed to experience a drop in demand for local educational services. Therefore, in a Tiebout world, movers who are adjusting their residential choice for the first time after becoming empty-nests should experience downward adjustment of their exposure to school spending and associated property taxes. Findings in Table 3 suggest that while cross-state empty-nest movers achieve significantly greater fiscal adjustment than cross-state non-empty-nest movers, the same does not hold for within-state moves. Approximately 75 percent of moves in the Farnham-Sevak sample are within-state moves. The failure of Tiebout sorting to apply at the level of within-state moves therefore presents a significant strike against Tiebout's hypothesis. It also raises a number of questions about what might be impeding fiscal adjustment among movers.

In order to test the hypothesis that state policy inhibits the ability of households to engage in within-state Tiebout sorting, we apply a simple extension of the analysis presented in Table 3. Tables 4-5 presents results of regressions of the form in equation (2) with an additional interaction term representing whether the state of destination has been subject to state or legislatively ordered school finance equalization. We perform this analysis for two key fiscal variables, the constant-house change in property tax liability and the change in local education revenues per capita.

Table 4 shows results of including this additional interaction term in regressions with constant-house change in property tax as the left-hand-side variable. Constant-house change in property tax liability, measured at the household level, is simply the change in property tax rates from origin to destination multiplied by the self-reported house value at the origin. For non-movers this change is typically close to zero. This measure of change in property tax liability allows us to control for changes in tax liability associated with trading up (trading down) into a home of greater (lesser) value. This allows us to isolate changes in tax regime associate with moves from changes in house value associated with moves.

Results in Table 4 suggest that school finance equalization plays a role in inhibiting Tiebout adjustment. Column 1 gives results from interacting a dummy variable indicating the imposition of school finance equalization with empty-nest moves at the state level, the cross-Metropolitan Statistical Area (but within state) level, and at the within-MSA level. Cross-state empty-nest moves into a state where SFE has been imposed are associated with much bigger declines in property tax liability than other cross-state empty-nest moves. This is likely due to the fact that school finance equalization is generally associated with a decline in the fraction of total school revenues derived from property taxes. As expected, however, the opposite is true for

empty-nest moves that occur within state. Estimates of the coefficients on the interaction between *SFE* and *EN*M (*empty-nest move) are positive and quite large for both local and cross-MSA movers. However, the coefficient estimates are statistically insignificant. The same basic pattern holds, but with greater strength, when we indicate school finance equalization by use of a dummy for court-imposed equalization (Column 2). Here, the coefficient estimates are very large, and all of the expected sign. By this test, court-ordered equalization appears to be a major inhibitor of Tiebout-consistent fiscal adjustment among empty-nest movers. It is worth noting, however, that even in the absence of equalization, reductions in property tax liability associated with within-state empty nest moves are modest and statistically insignificant.

Table 4, Column 3 gives results using a continuous policy measure. Here we interact the percent of school revenues in a state that are locally derived with *EN*M*. Greater values of *PCTLOC* indicate greater local control, and we expect, greater opportunities for within-state Tiebout sorting. Results are qualitatively similar to those using binary indicators of school finance equalization. Households moving *into* states with higher *PCTLOC* tend to experience slightly smaller declines in property tax liability. However, coefficient estimates for the interaction of *PCTLOC* with within-state empty nest moves suggest that greater local control is associated with greater downward adjustments in property tax liability by empty-nest movers. In the case of local moves, the coefficient is large and statistically significant. The coefficient on empty nest moves within MSA indicates that households that move locally generally experience large increases in taxes, a finding inconsistent with our hypothesis bases on Tiebout. However, the coefficient on the interaction of these moves with *PCTLOC* suggests that this could be because households are "hobbled" in their ability to reduce property taxes by state equalization.

The results in Column 4 are less supportive of our hypothesis that school finance equalization inhibits Tiebout adjustment. Here we interact our calculation of the Card-Payne index with *EN*M.* More negative values of the Card-Payne index indicate greater equalization. Thus, like *PCTLOC,* larger values of this measure indicate less equalization. Therefore, if our hypothesis that equalization inhibits Tiebout adjustment holds, we should observe negative coefficients on the interaction with empty-nest moves within state. Our estimates are small, and, in the case of cross-MSA moves, not of the expected sign. Overall, however, the findings in Table 4 provide some support for the hypothesis that equalization inhibits fiscal adjustment by empty nesters engaging in residential readjustment.

Table 5 presents the parallel analysis for a different fiscal variable—the change in local education revenues per capita. This variable is measured at the school district level. Here results

are somewhat less supportive of our hypothesis. Interactions of the equalization measures with *EN*M* generally do not yield the expected signs or they are not significant. Results are slightly more promising in Column 3, where empty-nest cross-MSA movers experience greater reductions in their exposure to local school revenue collection in states with greater local control. The estimate on the interaction term is of the "wrong" sign, however, for within-MSA moves. So while evidence in Table 4 is fairly supportive of the argument that equalization limits the ability of older households to engage in lifecycle Tiebout fiscal adjustments, results in Table 5 do little to further this support.

This line of analysis is limited in three ways. First, the HRS sample is not very large, and cell sizes quickly become small if we try to cut the data too finely. Second, the HRS, while nationally representative, does not have a representative sample of every state. Therefore an analysis of state policy using the HRS can be difficult. Finally, the HRS samples only older households. So it makes it impossible to do a full analysis of lifecycle Tiebout sorting.

In order to increase our sample size, explore state policy in more detail, and expand our sample to include households at both ends of the Tiebout lifecycle (at least, with respect to local education services) we pursue the rest of our analysis using 2000 Census micro data, as described above. Two major elements of the previous analysis must be given up to use the Census micro data. First, the household panel is lost. We have household-level data in the Census, but only a cross-section. Second, Census privacy measures prevent us from matching households to local fiscal characteristics to the degree that we are able to with the HRS. So while we can know each household's tax liability, we cannot know which school district it is located in or other such fiscal details. As a result, rather than focusing on fiscal changes associated with moves, as we do with the HRS sample, with the Census data we focus on mobility, commute time, and private school attendance. These are all issues closely related to lifecycle Tiebout sorting.

Census Mobility and School Finance Equalization

To analyze the effect of school finance equalization on within-state mobility, we estimate a series of logistic regressions. The dependent variable, *MOVE,* equals 1 if a household makes a within-state move "within the past year." If a household makes a cross-state move or doesn't move at all, we set *MOVE*=0.

Results of mobility logits that include 9 region fixed effects are given in Table 6. Estimates suggest that married couples are less mobile than other households. Divorced households are more mobile. Mobility appears to vary nonmonitonically in educational

attainment, with college educated households and households without a high school education having mobility rates higher than those of high-school educated households. Households with two earners tend to have relatively lower mobility rates, consistent with job location constraints on mobility.

Some information about mobility over the lifecycle can also be gleaned from the results in Table 6. Estimates suggest that mobility is declining in age, but at a decreasing rate. Young childless couples have relatively high mobility rates as might be expected. Parents tend to have lower mobility rates especially once their children reach school age. This also seems natural, given that switching schools may be traumatic for many children. In addition, it appears that households that have experienced a retirement within the past five years have elevated rates of mobility, relative to otherwise similar households. This is also quite natural, given that work often places significant relocation constraints on households. Given these constraints and the significant costs associated with even short distance moves, it is likely that many households wait until retirement to engage in late-in-lifecycle residential adjustments.

In each column of Table 6, we use a different proxy for state equalization policy, as we did with Tables 4-5. In Table 6, Column 1 we use a simple dummy variable for whether SFE has been imposed, either by court or legislature. In Column 2 we use the state average percent of school expenditures funded out of local revenues as an indication of the degree of local control. In Column 3 we use the Card-Payne index described above. Table 7 extends the analysis to give a comparison between the effects of court-ordered versus legislatively-imposed equalization.

Estimates in Column 1 suggest that the imposition of school finance equalization is associated with reduced mobility among parents of school-age children. This can be seen in the negative coefficient estimates on the interaction between *SFE* and various cohorts of parents. While consistent with our hypothesis that equalization should reduce the Tiebout motivation of households at key points in the lifecycle, the marginal effects are small and uniformly statistically insignificant.[14] Results in Column 2 are qualitatively similar. Recall that higher values of *PCTLOC* imply greater local control, so a positive coefficient on the parent interaction terms suggests that equalization reduces mobility. While most of the coefficient on parent interaction terms are statistically insignificant, young "pre-parents"—married couples under age 35 with no children—are more mobile in states with greater local control. The coefficient estimate is statistically significant at the 10 percent level. The marginal effect suggests that a 10 percentage

[14] We report the average marginal effect across households, rather than the marginal effect faced by the average household.

point increase in the percent of school revenues derived locally is associated with a 0.3 percentage point increase in mobility of pre-parents, relative to the residual population of non-parents and non-recent retirees. Given a mobility rate of 15 percent, this translates into a 2 percent increase in mobility. Findings in Column 3 exhibit the same basic pattern, with less equalization (a less negative Card-Payne index, or a larger Card-Payne index) associated with higher mobility among parents. In this case, however, the strongest effects of equalization are found among parents of children already of school age. The marginal effects for parents of school-age children suggest that a 1 standard deviation increase in equalization from the mean of the Card-Payne index, leads to a 3 tenths of a percentage point decrease in mobility, relative to the control group.

Table 7 gives estimates from a similar logit where we include dummies for court-imposed versus legislatively-imposed school finance equalization. This is intended to provide a test of the relative strength of these basic forms of equalization. Card and Payne (2002) find evidence that court-ordered equalizations have stronger equalizing effects. In terms of effects on parent mobility relative to the control group of non-parents and non-recent retirees, we generally find stronger effects of legislatively-imposed equalization. However, our findings on the effect of both types of equalization on parent mobility are statistically insignificant. As a caveat, one should note that policy endogeneity is likely to be stronger in the case of legislatively imposed equalizations. Policy endogeneity may be present for both types of school finance equalization.

Turning to the question of effects of equalization on mobility of empty-nest households, we next consider the impact on mobility of recent retirees. Census PUMS data allow us to determine whether a household has children who no longer live at home. It also allows us to determine which households have recently withdrawn from the labor force. We argue that the years immediately following retirement should be a focal point for lifecycle Tiebout mobility. This is because moving is costly, especially when a household is currently working, and some empty-nest households that desire to relocate for fiscal reasons are likely to wait until retirement in order to more cheaply adjust their location.[15] If Tiebout-adjusting moves are concentrated in the years following retirement, then to the extent that equalization reduces the incentive to engage in such moves, we should observe relative mobility declines among recent retirees in the presence of equalization.

[15] One can think of this as waiting for an expanded choice set (i.e. one that includes communities out of current commuting range) or for lower moving costs (in terms of required commuting time).

Our estimates in Tables 6-7 suggest the opposite. In most specifications, estimates suggest that greater equalization, as proxied for by each of the policy variables, increases mobility of recent retirees. The finding is statistically significant in the case of *PCTLOC* where greater local control appears to lower mobility among recent retirees. A ten percentage point increase in the local share of educational revenues reduces mobility by 2 tenths of a percentage point relative to non-parents and non-recent retirees. In Table 7, however, only both court-ordered equalizations appear to increase the mobility of recent retirees. This finding is statistically significant at the 10 percent level, and implies that court-ordered equalization raises mobility of recent retirees by 5 tenths of a percentage point relative to non-parents and non-recent retirees.

There appear to be two possible explanations for the puzzling findings regarding the effect of school finance equalization on mobility of recent retirees. One applies to the case where equalization weakens the link between location and tax burden. The other applies to the case where it strengthens the link between location and tax burden.

In the case where school finance equalization weakens the link between location and tax burden, for example by shifting from a system of local property tax finance to one of state income tax finance, we can think (roughly) of equalization as inducing a simultaneous shift in supply and demand. While equalization may reduce the incentive of households to escape high spending school districts upon retirement (or achievement of empty-nest status) the delinking of tax burden from other location characteristics simultaneously raises the relative appeal of other communities in the state with characteristics correlated with high levels of school spending. So empty-nest households that find non-fiscal characteristics of good school districts appealing may have more opportunities to move to other good school districts in states with school finance equalization

Another possibility is that equalization actually increases cross-district differentials in tax burden, even while reducing differentials in school service levels. This possibility should not be immediately ruled out. Many equalization programs include components that tax local revenue collections in high spending communities while subsidizing local revenue collections in low spending communities. If this has the effect of raising overall property tax liabilities in communities where parents tend to congregate, while lower overall tax liabilities in communities where the elderly tend to congregate, then mobility of empty-nest households could actually be increased.

Our HRS findings suggest that opportunities to fiscally adjust may be less in states under school finance equalization. This suggests that the former story may be more applicable. The theoretical ambiguities associated with the effect of school finance equalization on empty-nest

migration must be clarified and addressed in empirical work before strong conclusions can be drawn. We plan to elucidate these issues in further work.

Commute Time and School Finance Equalization

Farnham and Sevak (2004) provide evidence that retired empty-nest movers achieve stronger fiscal adjustment than non-retired empty-nest movers. This suggests that job location constraints play a role in limiting Tiebout sorting. Multidimensionality of the local fiscal and amenities bundle and limited space—which prevents an infinite number of differentiated communities from forming—suggests that Tiebout sorting requires tradeoffs beyond those of simple moving costs. Choice of location involves tradeoffs between such characteristics as school quality, tax burden, and convenience with respect to job location. Therefore, equalization, by weakening the link between the consumption of school services and location, should allow households, on average, to locate closer to work.

Table 8 displays results from OLS regressions of travel time to work, expressed in minutes, on household and state-level characteristics, including one of the four variables denoting equalization policy. We also include 9 region fixed effects to control for regional differences in travel times. The sample for these regressions includes households with children under the age of 18. In the case of two earner households, we use the maximum commute time. We would expect such households to face tradeoffs between commuting time and school consumption, in a Tiebout world. Note that more educated households, households with greater income, married couples, and younger households tend to have longer commutes. Not surprisingly, the presence of two earners tends to make for a longer maximum commute in the household. According to estimates in Column 1, equalization imposed in a state by either court or legislature is associated with a 2 minute *longer* commute for parents, contrary to our hypothesis. Results in Column 2 suggest that the increase in commuting time accrues roughly equally regardless of whether courts or the legislature has imposed equalization.

We obtain the opposite result when using *PCTLOC* as our policy variable, as can be seen in Table 8, Column 3. The coefficient estimate on *PCTLOC* suggests that a 10% decrease in *PCTLOC* shortens commute times by 4.7% from the mean. Results using the Card-Payne index (Column 4) also give an estimate on the policy variable consistent with our hypothesis, though it is small and statistically insignificant. Overall, the findings on commuting time are mixed.

Equalization and Private School Attendance

Another issue intertwined with equalization policy and Tiebout sorting is the question of private school attendance. Parents may experience reduced mobility under equalization because the return to moving, in terms of better public school quality, declines, or because they shift kids into private school. As noted above, economists have made conflicting theoretical predictions on the effect of equalization on private school attendance. We test for a relationship between equalization and private school attendance by estimating a logit where Y=1 if a household has at least one child in attendance at a private school. The sample is limited to households with children age 5-18. Again, we include region fixed effects to control for regional differences in private school enrollment.

Table 9, Column 1 gives findings from the first of these regressions. The policy variable used in this case is the dummy variable, *SFE*. According to these estimates, the marginal effect of equalization on private school attendance is to reduce the probability of enrollment in private school by one percentage point**.** We include income interaction terms in order to judge the distribution of effect across income classes. We expect equalization to improve schools in poor districts and lessen quality among schools in wealthy districts. Therefore we expect poor families with strong tastes for education will respond to equalization by switching their children from private to public schools. Wealthy families may respond to quality diminution by switching into private schools. The income interaction terms support this hypothesis to some extent. The omitted income category is households with income under $25,000 a year. Taken together, the findings suggest that under equalization, private school attendance falls for poor families and rises, on average, for families with incomes above$50,000. These results are basically consistent with school finance equalization that lessens quality in wealthy districts and increases it in poor districts.

Results in Column 2 are qualitatively similar, and are statistically significant. Here, equalization (as proxied for by lower *PCTLOC*) leads to declines in private school attendance, but primarily among poor households. Estimates using the Card-Payne index are given in Column 3. Again, our estimates suggest that equalization results in lower private school attendance. The differential distribution of impact between poor families and rich is less pronounced in this case, though.

The results on private school attendance provide some preliminary empirical support for the theoretical predictions of both Nechyba and Hoxby. Private school attendance among poor families does appear to be lower in the presence of SFE, while the opposite is true among wealthy families.

VI. Conclusions

Further work is required to clarify our findings thus far. To summarize, we find some evidence, using a panel dataset linking the Health and Retirement Study to local fiscal data, that fiscal adjustment is inhibited by school finance equalization. To investigate the effects of equalization on residential mobility, we look at Census PUMS data and estimate the impact of school finance equalization on mobility of households at critical points in the Tiebout lifecycle—namely young parents and recent retirees. Here we find limited and somewhat contradictory evidence that equalization reduces the mobility of parents but increases the mobility of recent retirees. The latter finding points to a theoretical ambiguity in the potential effect of equalization on empty-nest mobility.

We also test for the effect of equalization on travel time to work and on private school attendance. We find decided mixed evidence on travel time. The evidence on private school attendance is more consistent and suggests that while equalization is associated with lower rates of private school attendance, this appears to reflect declines in private school attendance among the poor and increases among the rich.

A number of empirical issues must be addressed in pursuing this analysis further. First, policy endogeneity is likely to play an important role in estimated "effects" of equalization on various outcomes. For example, states with particularly high mobility rates may have experienced more thorough Tiebout sorting and therefore greater school spending inequality. If equalization tends to be implemented in high mobility states, either by lawsuit or legislative action, its true effect on mobility is likely to be understated in this analysis. Second, unobserved heterogeneity is not well-controlled for in our cross-sectional analysis. Assembling a number of decennial Censuses and observing the response of mobility and other variables of interest to changes in policy over time would provide a significant advantage over the current analysis. Finally, more work should be done to differentiate between different types of equalization policy.[16] It is important to consider what aspect of the link between location and the fiscal bundle is affected by equalization, in order to determine the distribution of effects. We plan to explore these issues in further work.

[16] Hoxby (2001) emphasizes that a simple dummy variable treatment of equalization policy ignores significant policy details.

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Table 1: Summary Statistics

Households in the 5% PUMS of the 2000 U.S. Census

	Mean	SD
Household Characteristics		
Moved in Past Year	0.15	0.36
Moved within State in Past Year	0.11	0.31
Homeowner	0.69	0.46
Married	0.56	0.50
Divorced	0.18	0.39
Childless Couple < Age 35	0.03	0.16
Eldest Child < Age 7	0.07	0.26
Eldest Child Age 7-10	0.06	0.24
Eldest Child Age 11-18	0.16	0.36
Child in Private School	0.07	0.26
ED=College Degree	0.28	0.45
ED=HS Degree	0.56	0.50
ED=less than HS Degree	0.16	0.37
Log Household Income	10.71	1.15
Two Earner Household	0.25	0.43
Maximum Commuting Time of Household Members	17.66	21.39
Metro Area	0.42	0.49
Household Head's Characteristics		
Age	51.79	16.94
White Race	0.81	0.39
Black Race	0.11	0.31
Hispanic Ethnicity	0.08	0.27
Retired between 1995 and 2000	0.07	0.25
State Characteristics		
Average Size of School District	35.10	61.85
Card-Payne Equalization Index	-35.76	27.58
State Has SFE	0.70	0.46
State Had Court Ordered SFE	0.23	0.42
State Had Legislative SFE	0.48	0.50
Avg. % of Education Revenue Locally Collected	45.12	12.18
n	5,127,175	

Notes: State characteristics are based on state of residence in 1995.

Table 2:
Characteristics of School Financing Across the States

	SFE	Legislative SFE	Court Ordered SFE	% Ed from Local Revenue	Card-Payne Index	Minimum Foundation Grant Program	Foundation Grant Program	Variable Grant Program
AL	0	0	0	29.50	7.09	1	0	0
AZ	1	1	0	43.00	-28.57	1	0	0
AR	1	0	1	31.40	-9.23	0	0	0
CA	1	0	1	35.30	-22.07	1	1	0
CO	0	0	0	50.90	-50.45	1	0	0
CT	1	0	1	58.30	-76.12	0	0	1
DE	0	0	0	26.70	-39.24	0	1	1
FL	1	1	0	44.00	-117.13	1	0	0
GA	1	1	0	41.30	-14.09	1	0	1
ID	1	1	0	28.60	-43.86	1	0	0
IL	1	1	0	66.60	-31.79	1	1	1
IN	0	0	0	39.80	-10.04	1	1	0
IA	1	1	0	45.60	-8.77	1	1	0
KS	1	0	1	33.00	-26.26	0	0	0
KY	1	0	1	26.40	-28.25	1	1	0
LA	0	0	0	37.60	-10.42	1	0	0
ME	1	1	0	47.50	-51.38	1	0	0
MD	1	1	0	56.80	-19.74	1	1	0
MA	1	1	0	57.00	-29.74	0	0	1
MI	0	0	0	27.00	-18.52	0	1	1
MN	1	1	0	35.60	-36.44	1	0	0
MS	0	0	0	28.50	10.33	1	0	0
MO	1	1	0	53.40	-20.94	1	1	1
MT	0	0	0	32.10	0.80	1	0	1

Notes: These measures come from a variety of sources. Please see text for details and definitions.

Table 2 (continued):
Characteristics of School Financing Across the States

	SFE	Legislative SFE	Court Ordered SFE	% Ed from Local Revenue	Card-Payne Index	Minimum Foundation Grant Program	Foundation Grant Program	Variable Grant Program
NE	0	0	0	62.10	-11.39	1	0	0
NV	0	0	0	63.50	.	1	0	0
NH	1	1	0	89.70	-17.22	1	0	0
NJ	1	0	1	58.00	-34.88	1	0	0
NM	1	1	0	13.90	.	1	0	0
NY	0	0	0	54.10	-59.47	0	1	1
NC	0	0	0	28.30	-17.40	0	1	0
ND	0	0	0	45.30	-10.69	1	0	0
OH	1	1	0	53.00	-25.49	1	0	0
OK	1	1	0	29.40	-42.16	1	0	1
OR	0	0	0	37.80	-4.01	1	0	0
PA	0	0	0	54.20	-58.26	0	0	1
RI	1	1	0	53.40	-49.50	0	0	1
SC	1	1	0	38.80	-19.61	1	0	0
SD	1	1	0	59.30	-8.52	1	0	0
TN	1	1	0	43.40	-6.02	1	0	0
TX	1	1	0	49.60	-54.96	1	0	1
UT	1	0	1	34.80	.	1	0	0
VT	1	1	0	67.40	-53.60	1	1	0
VA	1	1	0	63.60	-27.32	1	0	0
WA	1	0	1	26.20	-18.55	0	0	1
WV	1	0	1	28.90	-57.65	1	0	0
WI	1	1	0	52.80	-29.42	0	0	1
WY	1	0	1	35.20	-146.08	1	0	0

Notes: These measures come from a variety of sources. Please see text for details and definitions.

Table 3:
Regression Results of 1-Period Change in
Local Finance Variables Associated With Moves in HRS 1992-2000
(controlling for community characteristics)

	(1) *?PPE*		(2) *?LOCEDPC*		(3) *?PROPTAX_CH*		(4) *?PROPTAX*	
Just Moved								
Across State	-64.15		-17.76		124.41		159.97	
	(139.08)		(40.95)		(244.81)		(203.23)	
(Across State)*(Unadjusted EN)	-740.60	**	-126.75	**	-588.07	**	-829.74	**
	(181.08)		(64.25)		(304.10)		(250.05)	
Across MSA	287.45	**	-28.53		113.92		96.34	
	(129.76)		(38.63)		(175.70)		(110.86)	
(Across MSA)*(Unadjusted EN)	-488.62	**	-1.93		-5.63		63.34	
	(163.59)		(37.33)		(206.33)		(152.91)	
Within MSA	-40.93		-6.67		-93.68		-86.67	
	(77.58)		(11.70)		(121.58)		(144.42)	
(Within MSA)*(Unadjusted EN)	-36.48		18.17		34.41		291.73	*
	(90.98)		(17.22)		(209.70)		(159.61)	
R-Sq	0.27		0.16		0.02		0.02	
Mean	5,619		529		1,584		1,584	
n	14,487		14,514		14,474		14,477	

Notes: This is Table 8 from Farham & Sevak (2004)
Standard errors are clustered at the town level.
** denotes statistical significance at the 5% level, * at 10%.

Table 4:
Regression of 1-Period Change in Constant-House Property Tax
Associated with Moves and School Finance Variables in HRS 1992-2000
(controlling for community characteristics)

	Policy Variable:			
	SFE in Place	C.O. SFE	% Local	Card-Payne
Just Moved				
Across State	121.41	135.58	104.39	99.81
	(239.14)	(244.37)	(246.39)	(237.89)
(Across State)*(Unadjusted EN)	-174.88	-566.21 *	-1960.52 **	-405.96
	(317.57)	(328.54)	(809.54)	(353.2)
Policy*(Across State)*(Unadjusted EN)	-569.55 *	-158.25	31.18 **	3.26
	(304.57)	(422.61)	(15.18)	(4.92)
Across MSA	114.70	115.35	111.03	112.51
	(178.41)	(176.98)	(173.88)	(176.72)
(Across MSA)*(Unadjusted EN)	-176.98	-149.38	269.68	158.88
	(322.51)	(215.12)	(581.1)	(214.4)
Policy*(Across MSA)*(Unadjusted EN)	267.25	951.64 **	-6.32	4.18 *
	(308.19)	(266.72)	(12.51)	(2.25)
Within MSA	-94.66	-96.02	-91.75	-92.59
	(120.76)	(120.89)	(121.65)	(121.59)
(Within MSA)*(Unadjusted EN)	-35.37	-88.25	1499.66 **	-109.52
	(259.28)	(196.4)	(580.61)	(235.66)
Policy*(Within MSA)*(Unadjusted EN)	101.80	624.99 *	-30.89 **	-3.95
	(255.63)	(357.91)	(10.66)	(4.55)
R-Sq	0.020	0.021	0.022	0.020
n	14,474	14,474	14,474	14,432

Notes: Standard errors are clustered at the town level.

** denotes statistical significance at the 5% level, * at 10%.

Table 5:
Regression of 1-Period Change in Per-Capita Educational Expenditure
Associated with Moves and School Finance Variables in HRS 1992-2000
(controlling for community characteristics)

	Policy Variable:			
	SFE in Place	C.O. SFE	% Local	Card-Payne
Just Moved				
Across State	-16.06	-15.44	-19.29	-22.42 **
	(41.28)	(41.6)	(40.32)	(41.89)
(Across State)*(Unadjusted EN)	-158.88 **	-121.82 *	-272.48 **	-98.42 **
	(54.62)	(62.19)	(86.53)	(72.83)
Policy*(Across State)*(Unadjusted EN)	45.62	-35.86	3.43 **	0.59
	(45.15)	(49.43)	(1.56)	(0.51)
Across MSA	-27.98	-27.58	-30.10	-27.71 **
	(38.08)	(38.77)	(39.47)	(38.51)
(Across MSA)*(Unadjusted EN)	-63.07	-18.10	193.45 **	15.65
	(60.67)	(34.21)	(83.95)	(41.27)
Policy*(Across MSA)*(Unadjusted EN)	82.38	93.82	-4.28 **	0.45
	(59.33)	(97.49)	(1.63)	(0.51)
Within MSA	-6.75	-6.75	-6.38	-6.61 **
	(11.64)	(11.74)	(11.66)	(11.73)
(Within MSA)*(Unadjusted EN)	6.23	21.30	-6.71	21.72
	(32.55)	(19.77)	(53.22)	(20.05)
Policy*(Within MSA)*(Unadjusted EN)	15.98	-15.70	0.52	0.15
	(32.38)	(28.3)	(1.11)	(0.21)
R-Sq	0.097	0.097	0.098	0.097
n	14,514	14,514	14,514	14,472

Notes: Standard errors are clustered at the town level.

** denotes statistical significance at the 5% level, * at 10%.

Table 6:
Logit Results of Whether the Household Moved Within State between 1999 and 2000

	Policy Measure: SFE in Place			Policy Measure: % of Ed Locally Financed			Policy Measure: Card-Payne Index		
	Coeff.	Marginal		Coeff.	Marginal		Coeff.	Marginal	
Age	-0.1185	-0.0071	**	-0.1187	-0.0071	**	-0.1188	-0.0071	**
	(0.00)			(0.00)			(0.00)		
Age Squared	0.0006	0.0000	**	0.0006	0.0000	**	0.0006	0.0000	**
	(0.00)			(0.00)			(0.00)		
Married	-0.1475	-0.0088	**	-0.1467	-0.0088	**	-0.1463	-0.0087	**
	(0.01)			(0.01)			(0.02)		
Divorced	0.2477	0.0148	**	0.2480	0.0149	**	0.2473	0.0148	**
	(0.01)			(0.01)			(0.01)		
White Race	-0.0614	-0.0038	**	-0.0633	-0.0038	**	-0.0746	-0.0045	**
	(0.03)			(0.03)			(0.03)		
Black Race	-0.1039	-0.0062	**	-0.1103	-0.0066	**	-0.1037	-0.0062	**
	(0.03)			(0.03)			(0.03)		
Hispanic	0.1291	0.0077	*	0.1312	0.0079	*	0.1195	0.0071	*
	(0.07)			(0.07)			(0.07)		
Ed=College	0.0970	0.0058	**	0.0976	0.0058	**	0.0918	0.0055	**
	(0.02)			(0.02)			(0.02)		
Ed=HS Grad	-0.0082	-0.0005		-0.0065	-0.0004		-0.0134	-0.0008	
	(0.02)			(0.02)			(0.01)		
Two Earners	-0.1074	-0.0064	**	-0.1080	-0.0065	**	-0.1104	-0.0066	**
	(0.01)			(0.01)			(0.01)		
Childless Couple < Age 35	0.3752	0.0225	**	0.1269	0.0076		0.3703	0.0221	**
	(0.08)			(0.12)			(0.04)		
Eldest Child < Age 7	0.0420	0.0025		-0.0537	-0.0032		0.0397	0.0024	
	(0.04)			(0.07)			(0.03)		
Eldest Child Age 7-10	-0.1138	-0.0068	**	-0.1658	-0.0099	**	-0.0795	-0.0048	**
	(0.02)			(0.05)			(0.03)		
Eldest Child Age 11-18	-0.1902	-0.0114	**	-0.1990	-0.0119	**	-0.1704	-0.0102	**
	(0.03)			(0.04)			(0.02)		
Retired 1995-2000	0.2297	0.0138	**	0.4328	0.0259	**	0.2563	0.0153	**
	(0.05)			(0.07)			(0.05)		
Log Income	0.0609	0.0037	**	(0.05)	0.0036	**	0.0614	0.0037	**
	(0.01)			(0.01)			(0.01)		
Average Size of School District	0.0003	0.0000		0.0002	0.00001		-0.0007	0.0000	
	(0.00)			(0.00)			(0.00)		
School Finance Policy Variable	**0.0895**	**0.0054**		**0.0024**	**0.0001**		**-0.0035**	**-0.0002**	**
	(0.06)			**(0.00)**			**(0.00)**		
Policy*Childless Couple < Age 35	**-0.0533**	**-0.0022**		**0.0046**	**0.0003**	*	**0.0009**	**0.000065**	
	(0.10)			**(0.00)**			**(0.00)**		
Policy*Eldest Child < Age 7	**-0.0568**	**-0.0032**		**0.0012**	**0.0001**		**0.0011**	**0.0001**	
	(0.05)			**(0.00)**			**(0.00)**		
Policy*Eldest Child Age 7-10	**-0.0424**	**-0.0034**		**0.0005**	**0.00003**		**0.0018**	**0.00008**	**
	(0.03)			**(0.00)**			**(0.00)**		
Policy*Eldest Child Age 11-18	**-0.0361**	**-0.0025**		**-0.0004**	**-0.00002**		**0.0013**	**0.00005**	**
	(0.03)			**(0.00)**			**(0.00)**		
Policy*Retired 1995-2000	**0.0726**	**0.0043**		**-0.0034**	**-0.0002**	**	**-0.0008**	**-0.00005**	
	(0.05)			**(0.00)**			**(0.00)**		
R-Sq	0.0875			0.0875			0.0883		

Notes: See text for variable definitions. ** denotes statistical significance at the 5% level and * at the 10% level.

Contains 9 Region Fixed Effects.

Table 7:
Logit Results of Whether the Household Moved Within State between 1999 and 2000

	Policy Measure: Court Ordered vs. Legislative SFE		
	Coeff (SE)	Marginal Effect	
Court Ordered SFE	0.0514	0.003	
	(0.07)		
CO SFE*Eldest Child < Age 7	-0.0303	-0.002	
	(0.06)		
CO SFE*Eldest Child Age 7-10	-0.0093	-0.001	
	(0.03)		
CO SFE*Eldest Child Age 11-18	-0.0395	-0.002	
	(0.03)		
CO SFE*Retired 1995-2000	0.0915	0.005	*
	(0.05)		
CO SFE*Childless Couple < Age 35	-0.0181	-0.001	
	(0.12)		
Legislatively Ordered SFE	0.1062	0.006	
	(0.07)		
LO SFE*Eldest Child < Age 7	-0.0687	-0.004	
	(0.06)		
LO SFE*Eldest Child Age 7-10	-0.0577	-0.003	
	(0.04)		
LO SFE*Eldest Child Age 11-18	-0.0349	-0.002	
	(0.03)		
LO SFE*Retired 1995-2000	0.0641	-0.004	
	(0.06)		
LO SFE*Childless Couple < Age 35	-0.0688	-0.004	
	(0.11)		
R-Sq	0.0875		

Notes: See text for variable definitions. ** denotes statistical significance at the 5% level and * at the 10% level.

Contains all covariates listed in Table 6, and 9 Region Fixed Effects.

Table 8:
Regression Results of Commuting Time to Work
Sample includes Households with Children Under age 18

	Policy Measure: SFE in Place			Policy Measure: Court Ordered vs. Legislative SFE		Policy Measure: % of Ed Locally Financed		Policy Measure: Card-Payne Index	
Married	4.01	**		4.02	**	4.01	**	4.02	**
	(0.20)			(0.21)		(0.21)		(0.21)	
Divorced	1.66	**		1.66	**	1.64	**	1.64	**
	(0.13)			(0.14)		(0.13)		(0.14)	
Black Race	1.75	**		1.75	**	1.65	**	1.69	**
	(0.69)			(0.69)		(0.68)		(0.70)	
Hispanic	-0.71			-0.72		-0.67		-0.48	
	(0.71)			(0.71)		(0.73)		(0.78)	
Ed=College	2.91	**		2.91	**	2.94	**	3.00	**
	(0.53)			(0.53)		(0.53)		(0.55)	
Ed=HS Grad	3.24	**		3.24	**	3.29	**	3.29	**
	(0.32)			(0.32)		(0.32)		(0.33)	
Age	0.26	**		0.26	**	0.26	**	0.26	**
	(0.03)			(0.03)		(0.03)		(0.04)	
Age Square	-0.0050	**		-0.0050	**	-0.0049	**	-0.0049	**
	(0.00)			(0.00)		(0.00)		(0.00)	
Log Income	3.13	**		3.13	**	3.11	**	3.17	**
	(0.23)			(0.23)		(0.23)		(0.24)	
Metro Area	0.17			0.17		0.15		0.20	*
	(0.11)			(0.11)		(0.11)		(0.11)	
Average Size of School District	-0.0029			-0.0029		-0.0065	**	-0.0180	*
	(0.00)			(0.00)		(0.00)		(0.01)	
Two Earners	6.83	**		6.82	**	6.81	**	6.82	**
	(0.31)			(0.31)		(0.31)		(0.31)	
School Finance Policy Variable	**1.75**	****	**CO SFE**	**1.72**	**	**0.08**	****	**0.00**	
	(0.69)			**(0.94)**		**(0.03)**		**(0.01)**	
			LO SFE	**1.76**	****				
				(0.71)					
R-sq	0.07			0.07		0.07		0.07	
n	1,187,562			1,187,562		1,187,562		1,160,321	

Notes: Contains 9 Region Fixed Effects. See text for variable definitions. ** denotes statistical significance at the 5% level and * at the 10% level.

Table 9:
Logit of whether the household has one or more children enrolled in private school
Sample Includes Households with children ages 5 to 18

	Policy Measure: SFE in Place			Policy Measure: % of Ed Locally Financed			Policy Measure: Card-Payne Index		
	Coeff	Marginal Effect		Coeff	Marginal Effect		Coeff	Marginal Effect	
married	0.28	0.036	**	0.28	0.036	**	0.28	0.036	**
	(0.03)			(0.03)			(0.03)		
divorced	-0.07	-0.010	**	-0.07	-0.010	**	-0.07	-0.010	**
	(0.02)			(0.02)			(0.02)		
white	-0.12	-0.015		-0.12	-0.015		-0.12	-0.016	
	(0.09)			(0.09)			(0.09)		
hisp	-0.07	-0.009		-0.08	-0.010		-0.07	-0.009	
	(0.08)			(0.09)			(0.08)		
1+ Parent w/4-yr college	1.25	0.163	**	1.25	0.163	**	1.24	0.163	**
	(0.12)			(0.12)			(0.12)		
1+ Parent w/12-yr ed	0.49	0.064	**	0.49	0.065	**	0.48	0.064	**
	(0.11)			(0.11)			(0.12)		
Age of HH head	-0.03	-0.004	**	-0.03	-0.004	**	-0.03	-0.004	**
	(0.01)			(0.01)			(0.01)		
Age Squared	0.0002	0.00002	**	0.0002	0.000	**	0.0002	0.00002	**
	(0.00)			(0.00)			(0.00)		
Metro Area	0.10	0.013	**	0.10	0.000	**	0.10	0.013	**
	(0.01)			(0.01)			(0.01)		
Average Size of School District	0.0006	0.000		0.0004	0.013		0.0044	0.0005738	
	(0.00)			(0.00)			(0.00)		
Two Earner HH	-0.26	-0.034	**	-0.26	-0.034	**	-0.26	-0.035	**
	(0.02)			(0.02)			(0.03)		
Income 25K-50K	0.11	0.014	**	0.34	0.044	**	0.17	0.023	**
	(0.04)			(0.07)			(0.03)		
Income 50K-100K	0.29	0.038	**	0.58	0.076	**	0.39	0.052	**
	(0.04)			(0.12)			(0.05)		
Income > 100K	0.63	0.082	**	1.11	0.146	**	0.72	0.095	**
	(0.05)			(0.16)			(0.07)		
School Finance Policy Variable	**-0.079**	**-0.010**		**0.010**	**0.0013**	**	**0.002**	**0.00020**	
	(0.1)			**(0.00)**			**(0.00)**		
*** 25K = Inc < 50K**	**0.050**	**0.007**		**-0.005**	**-0.0006**	**	**0.001**	**0.00011**	
	(0.05)			**(0.00)**			**(0.00)**		
*** 50K = Inc < 100K**	**0.103**	**0.014**	*	**-0.005**	**-0.0007**	**	**0.000**	**0.00006**	
	(0.06)			**(0.00)**			**(0.00)**		
*** Inc > 100K**	**0.123**	**0.016**		**-0.009**	**-0.0012**	**	**0.000**	**-0.00002**	
	(0.08)			**(0.00)**			**(0.00)**		
R-sq	0.057			0.058			0.057		
n	1,031,952			1,031,952			1,007,923		

Notes: See text for variable definitions. ** denotes statistical significance at the 5% level and * at the 10% level.
Contains 9 Region Fixed Effects.