MOBILITY, HOUSING MARKETS, AND SCHOOLS:
ESTIMATING GENERAL EQUILIBRIUM EFFECTS OF INTERDISTRICT CHOICE

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Abstract: In theoretical models of residential sorting, a household's location decision is closely tied to its demand for consuming local public services. School choice programs typically weaken the link between residential location and schooling options. Computable general equilibrium models suggest large general equilibrium effects from expanded school choice, but there is limited empirical evidence concerning whether these effects occur. This paper develops and empirically tests predictions concerning the general equilibrium effects of inter-district choice programs. Consistent with theory, districts with popular nearby, out-of-district schooling options experience relatively large increases in housing values and in the number of households with children.

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I. Introduction

In theoretical models of residential sorting, a household's location decision is closely tied to its demand for consuming local public services. In Tiebout's (1956) widely-cited model of residential location decisions, households sort across communities to find an optimal match given their demand for excludable public goods provided by local governments. Other models of residential sorting have also recognized the importance of a home's proximity to non-excluded public goods (or bads) that are geographically concentrated and not easily consumed or avoided without non-trivial transaction costs (e.g., oceans, national parks, pollution). Some studies have used structural models and computable general equilibrium models to examine the potential importance of Tiebout sorting, and others provide indirect evidence on sorting by examining how excludable public goods are capitalized into housing values.[1] A few empirical studies have directly investigated residential sorting responses to policy changes which affect the geographic distribution of non-excludable public goods, (e.g., Banzhaf & Walsh 2008; Cameron and McConnaha 2006; Kahn 2000), or responses to the relative amount of taxes and services provided by local governments (e.g., Stanford and Hoyt 2009; Epple and Ferreyra 2008; Coons and Hoyt 2008; Nechyba and Strauss 1998). This paper expands on this literature by conducting the first direct empirical tests of the predicted general equilibrium effects for residential sorting when policies reduce the link between housing and an excludable local public good. We examine how inter-district public school choice programs affect residential sorting and house prices across school districts.

Inter-district choice programs, which allow parents to enroll their child in schools located outside of their assigned school district, are one of the oldest and most prominent forms of school choice available to parents. Thirty-one states currently have official statewide policies allowing for inter-district transferring and, as of the 1999-2000 school year, more students took advantage of inter-district choice options than charter schools and vouchers programs combined

[1] Empirical investigations of house price capitalization include studies using classic hedonic models (e.g., Oates 1969), hedonic models with panel data (e.g., Hoyt and Rosenthal 1997), boundary discontinuity approaches (e.g., Black 1999), and boundary discontinuity approaches combined with instrumental variables techniques (e.g., Bayer, Ferreira, McMillan 2007). Locational equilibrium models include the work of Epple and Sieg (1999), Epple, Romer, and Sieg (2001), Sieg, Smith, Banzhaf, and Walsh (2004), Calabrese, Epple, Romer and Sieg (2006), and Ferreyra (2007). Relevant computable general equilibrium studies include simulations of the effects of school choice programs (e.g., Nechyba, 2000, 2003a, 2003b; Epple and Romano, 2003; Ferrerya, 2007). Finally, Rhode and Strumpf (2003) provide indirect evidence on the importance of Tiebout sorting by examining changes in the degree of income heterogeneity across communities over long periods of time. Please see Oates (2006) and Epple (2008) for recent reviews of the Tiebout sorting literature.

(Holme and Wells, 2008). Similar to other forms of school choice, inter-district choice programs weaken the link between school quality and residential location. These choice programs may increase families' interest in living in districts which offer relatively cheap housing near popular alternative schooling options. A number of recent theoretical papers have revealed the potentially large general equilibrium effects of school choice programs (e.g., Nechyba 2000, 2003a, 2003b; Epple and Romano 1998, 2003; and Ferreyra 2007). Nechyba (2000, 2003a, 2003b) uses a computable general equilibrium (CGE) model to examine the impact of private school vouchers on housing values and residential income stratification. His results suggest that vouchers have the potential to significantly reduce income and housing value disparities across school districts. Ferreyra (2007) estimates a structural model of residential location decisions and housing values, and then uses the resulting parameters to simulate the effect of two hypothetical state-funded voucher programs in Chicago. Similar to Nechyba (2000, 2003a, 2003b), she finds that vouchers attenuate residential income and housing value disparities across school districts. Epple and Romano (2003) use a CGE model to examine the impact of frictionless inter- and intra-district open enrollment policies on residential segregation and housing values and reach conclusions similar to Nechyba (2000, 2003a, 2003b) and Ferreyra (2007).

While the general equilibrium effects found in the theoretical literature seem plausible and important, there have been surprisingly few direct tests of whether they actually occur.[2] Two prior studies provide indirect evidence concerning how school choice programs affect housing values. Using aggregate vote returns from California's 1993 universal voucher initiative, Brunner, Sonstelie and Thayer (2001) find that homeowners are significantly less likely to support school vouchers if they live in a good school district; a finding that suggests homeowners are aware of the property value implications of school choice programs. Brunner

[2] While few studies have examined the general equilibrium effect of school choice programs, a growing number of studies have begun to explore the impact of various types of school choice programs on student outcomes and on student sorting across schools. Recent studies of student achievement include: Peterson, Howell, Wolf and Campbell (2003) and Hoxby (2003) in the context of targeted vouchers, Hoxby (2003), Hoxby and Rockoff (2005), Bettinger (2005), Sass (2006), Bifulco and Ladd (2006, 2007), and Booker et al. (2007) in the context of charter schools, Engberg et al. (2009) in the context of magnet schools, and Cullen, Jacob and Levitt (2005, 2006) in the context of intra-district open enrollment. Recent studies of student sorting and segregation include: Epple and Romano (1998), Epple, Figlio and Romano (2004), Epple and Romano (2008), Brunner and Imazeki (2008) and Brunner, Imazeki and Ross (forthcoming) in the context of private school vouchers, Saporito (2003), Bifulco and Ladd (2007), Weiher and Tedin (2002), and Hanushek, Kain, Rivkin and Branch (2007) in the context of charter or magnet school choice and Hastings, Kane and Staiger (2009), and Bifulco, Ladd and Ross (2009) in the context of expanded intra-district choice.

and Sonstelie (2003) reach the same conclusion using individual-level survey data on voter support for California's 2000 voucher initiative. In the study closest to ours, Reback (2005) finds that Minnesota's adoption of an inter-district choice program increased housing values in districts with valuable outgoing transfer opportunities and decreased housing values in districts offering valuable incoming transfer spaces. These prior studies focus solely on the housing value implications of school choice programs and use data from individual states, where unobserved variables may have influenced within-state differences.

This paper provides the first direct empirical evidence concerning the impact of expanded school choice opportunities on the residential location decisions of families. We use national data and a robust empirical strategy to examine how inter-district choice programs affect both residential sorting patterns and housing values. To motivate our empirical work, we use a multi-community model to develop predictions about the effects of introducing inter-district choice into a previously residentially zoned school system. Our model borrows heavily from previous work by Epple and Romano (2003), Calabrese et al. (2006), and Banzhaf and Walsh (2008). With either frictionless school choice or choice subject to capacity constraints,[3] the model predicts that housing values will fall in initially high-quality districts and rise in initially low-quality districts. With choice subject to capacity constraints, the model also predicts increased relative population density in initially low-quality districts and decreased relative population density in initially high-quality districts. Finally, with frictionless school choice, the model predicts a decline (almost always) in residential income stratification across districts.

We empirically test these predictions by examining changes in school district-level demographics and housing values between the 1990 and 2000 Censuses, when twenty-six states adopted statewide inter-district programs. To address the non-random nature of states' adoption of choice programs, our empirical models use a triple-differences style approach—i.e., we test whether within-state differences in changes in school districts' outcomes over time correspond with the expected effects of states' adoption of choice policies. Unlike a basic triple-differences model, we allow the intensity of treatment to vary continuously across districts.

Our measure of districts' intensity of treatment is the predicted district-level student participation rate in inter-district choice during the 1999-2000 school year. We use predicted

[3] Frictionless school choice is defined as the case where families are free to select the school their child attends without constraint. Choice subject to capacity constraints is defined as the case where schools have limited capacity and thus only a fraction of families can take advantage of inter-district choice options.

rates because actual rates are endogenous—i.e., they are influenced by recent changes in school quality that also affect residential location decisions and property values. To predict district-level participation rates, we compare districts' 1990 Census demographic characteristics with the 1990 demographic characteristics of their surrounding Census block-groups. These initial demographic differences are strong predictors of choice participation. While initial demographic differences may be related to future changes in property values and residential sorting, we can examine how the effects of these demographic differences vary based on states' adoption of specific types of inter-district choice policies. Our empirical models thus identify the causal effects of school choice opportunities by isolating the policy-specific effects of initial demographic differences between districts and their immediate surrounding neighborhoods.

Our empirical results strongly confirm our theoretical predictions and the findings of the computable general equilibrium literature. States' adoption of inter-district choice programs in the 1990's increased population density, residential income, and housing values in previously low-quality districts. These findings collectively reveal that even moderate reductions in the link between residential location and excludable public services can lead to economically meaningful general equilibrium effects.

II. Conceptual Framework

To motivate the empirical work that follows, we begin by exploring the impact of introducing an inter-district choice policy on housing values and community composition within the context of a multi-community model. We synthesize the work of Epple and Romano (2003), Calabrese et al. (2006), and Banzhaf and Walsh (2008) to illustrate the general equilibrium effects that are likely to arise after the adoption of inter-district choice.

Consider an educational market with J school districts and a continuum of households. Each household has one child that attends the local public schools and there is one school per district. Households differ only in their income, y, which is continuously distributed with density, $f(y)$, and support $[y_L, y_H]$. Households derive utility from the expected quality of schooling available to their child, q, housing consumption, h, and a composite private good, x. For simplicity, we assume that school quality in district j depends solely on the mean household income of the students attending district *j's* schools.[4] Define $\overline{\theta}_j$ as the average income of households whose children attend school in district *j*. Without any inter-district choice policy, $\overline{\theta}_j \equiv \overline{y}_j \equiv q_j$, where $\overline{y}_j$ is the mean household income of *residents* of district j . Following Calabrese et al. (2006), we assume preferences are separable in q and h such that housing demand is independent of school quality. The housing demand function is then $h(p, y)$ and the housing supply function is $S(p)$, where p is the price of a unit of housing .[5] A household's indirect utility function is given by:

$$V = V(q, h(p, y), y - ph(p, y)) = V(y, q, p), \qquad (1)$$

where $y - ph(p, y)$ is consumption of the composite private good, and the price of the composite private good is normalized to one. We further assume that $V_y > 0$, $V_q > 0$, and $V_p < 0$.

To characterize equilibrium, we assume household preferences satisfy the standard single crossing property.[6] Equilibrium is defined as a set of housing prices and an allocation of households to school districts such that all housing markets clear. In the absence of an inter-district school choice program, the resulting equilibrium is characterized by perfect stratification across school districts; the highest-income families live in the highest-quality (and highest-housing-price) district and the lowest-income families live in the lowest-quality (and lowest-housing-price) district.

To illustrate the predictions of the model, consider the case where the educational market consists of just two school districts. In an equilibrium with no inter-district choice, the single crossing property implies that if $q_2 > q_1$, then $\overline{y}_2 > \overline{y}_1$ and $p_2 > p_1$. The single crossing

[4] We assume that school spending is financed through a state-wide lump sum tax on income and that all schools receive the same amount of funding per pupil. This assumption allows us to abstract from the political economy aspects of schooling. See Epple and Romano (2003) and Nechyba (2000, 2003a, 2003b) for cases where school quality depends on spending per-pupil and schools are locally financed.

[5] Following Calabrese et al. (2006), we assume housing is produced from land and a mobile non-land factor of production. Specifically, for district j, housing per household is $H_j = h(L_j, Z_j)$, where L_j is the fixed amount of land area in district j and Z_j is the mobile factor of production with constant returns to scale. The housing supply function arises by assuming the price of Z is constant across districts.

[6] The single-crossing property requires the slope of indifference curves in school quality/house price space to be strictly increasing in income. As noted by Fernandez (2003), this ensures that if one family prefers the school quality/house price bundle offered by community *j* over some other bundle offered by community *k*, and $p_j > p_k$, then all families with higher income also prefer community *j*.

property also guarantees that there exists a “boundary” household, (characterized by that household’s unique income), that is indifferent between living in district 1 or district 2. All households with incomes below the boundary household’s income prefer to live in district 1, while all households with incomes above the boundary household’s income will prefer to live in district 2. The boundary income $y_{1,2}$ and the unique equilibrium price of housing in each community are implicitly defined by the equilibrium conditions:

$$V^1(q_1, y_{1,2}, p_1) = V^2(q_2, y_{1,2}, p_2) \tag{2}$$

$$\int_{y_L}^{y_{1,2}} h(p_1, y) f(y) dy = S(p_1) \tag{3}$$

$$\int_{y_{1,2}}^{y_H} h(p_2, y) f(y) dy = S(p_2) \tag{4}$$

Now consider how introducing frictionless inter-district choice affects the stratified equilibrium discussed above. Frictionless choice is defined as the case where households are free to select the school their child attends without constraint. Households face no transportation costs, schools face no capacity constraints, and any student that wishes to attend a particular school is guaranteed admittance. As noted by Epple and Romano (2003), the immediate implication of frictionless choice is that in equilibrium, school quality must be the same in the two districts, implying $q_1 = q_2$. With $q_1 = q_2$, the districts are identical and thus it follows that $p_1 = p_2$. Thus, introducing frictionless school choice leads to a decline in housing values in district 2, and a rise in housing values in district 1. Further, with $q_1 = q_2$ and $p_1 = p_2$, households are indifferent between districts. As a result, equilibrium is characterized by a random allocation of households between districts and thus a continuum of equilibrium household sorting patterns.[7] Aside from the extreme cases in which all of the highest income households live in one of the two districts, the resulting equilibrium with frictionless district choice decreases residential income stratification via a decline in the average income of households living in district 2 and a rise in the average income of households living in district 1.

[7] For a proof of this proposition see Epple and Romano (2003) and Banzhaf and Walsh (2008).

Allowing for Capacity Constraints

While the model outlined above leads to strong predictions about how inter-district choice is likely to affect housing values and residential income stratification, those predictions are based on the assumption that schools do not face capacity constraints, an assumption that is unlikely to hold. In this section we therefore consider the implications of relaxing that assumption. Specifically, suppose households are guaranteed admission to their district school as long as they live within the boundary of the district. Further, suppose district capacity is large enough such that all students living within the boundaries of a district can attend their local school and some fraction α of the students living in the other district could also be admitted. Districts admit students from other districts randomly using a lottery and no student can be denied admission conditional on capacity. Under these assumptions the expected quality of schooling available to a household residing in district 1 is: $E[q_1] = (1-\alpha)\cdot\bar{\theta}_1 + \alpha\cdot\bar{\theta}_2$. It follows that if $\alpha = 0$, the resulting equilibrium is identical to the case with no inter-district choice. Similarly, if $\alpha = 1$, the resulting equilibrium is identical to the case with frictionless choice.

To derive the comparative static implications of a change in α, we assume that households are myopic in the sense that they ignore the migration effects that are likely to accompany the introduction of an inter-district choice plan. Thus, households assume the distribution of income in their community will not change in response to the adoption of inter-district choice. Furthermore, we assume that households hold the following beliefs about school quality in district 1 and 2 (i.e., $\bar{\theta}_1$ and $\bar{\theta}_2$) following the introduction of inter-district choice:

1) Since admission into district 2 is based on a random lottery, $\bar{\theta}_1 = \bar{y}_1$. Myopic households expect the average household income of students that attend school in district 1 to remain the same, so they also expect school quality in district 1 to remain the same.
2) Since a random fraction, α, of the residents of district 1 gain admission to district 2 schools, $\bar{\theta}_2 = \dfrac{N_1\alpha\bar{y}_1 + N_2\bar{y}_2}{N_1\alpha + N_2}$, where N_1 and N_2 are the number of households residing in district 1 and 2 respectively. In other words, expected school quality in district 2 equals the weighted average of the average incomes in districts 1 and 2.

Based on these assumptions, the expected school quality available to a household residing in district 1 is:

$$q_1 = (1-\alpha)\cdot \bar{y}_1 + \alpha \cdot \left[\frac{N_1 \alpha \bar{y}_1 + N_2 \bar{y}_2}{N_1 \alpha + N_2}\right], \tag{5}$$

and the derivatives of q_1 and q_2 with respect to α are:

$$\frac{dq_1}{d\alpha} = \frac{N_2^2(\bar{y}_2 - \bar{y}_1)}{(N_1\alpha + N_2)^2} > 0 \text{ and } \frac{dq_2}{d\alpha} = \frac{N_1 N_2(\bar{y}_1 - \bar{y}_2)}{(N_1\alpha + N_2)^2} < 0.$$

Now consider how an increase in α affects the boundary income y_{12}. Applying the implicit function theorem to the equilibrium conditions given by equations (2), (3), and (4) and the school quality equation given by equation (5) yields:

$$\frac{dy_{1,2}}{d\alpha} = \frac{-V_{q_1}^1 \frac{dq_1}{d\alpha} + V_{q_2}^2 \frac{dq_2}{d\alpha}}{(V_y^1 - V_y^2) - f(y_{12})\left\{\frac{V_{p_1}^1 h(y,p_1)}{\int_{y_L}^{y_{1,2}} h_{p_1}(p_1,y) f(y)dy - S_{p_1}(p_1)} + \frac{V_{p_2}^2 h(y,p_2)}{\int_{y_{1,2}}^{y_H} h_{p_2}(p_2,y) f(y)dy - S_{p_s}(p_2)}\right\}} \tag{6}$$

To sign the derivative in equation (6) note that since $V_{q_1}^1 > 0$, $\frac{dq_1}{d\alpha} > 0$, $V_{q_2}^2 > 0$, and $\frac{dq_2}{d\alpha} < 0$ the numerator of the expression above is negative. Further, since $V_y^1 - V_y^2 < 0$ it follows that $\frac{dy_{1,2}}{d\alpha} > 0$. Due to income stratification, the immediate implication of this result is that an increase in α leads to an increase in the population of district 1 and thus a decrease in the population of district 2. Further, the increased demand for housing in district 1 and the decreased demand for housing in district 2 imply housing prices in district 1 must rise while housing prices in district 2 must fall. Finally, note that since the income of the boundary household residing in district 1 is an increasing function of α, $\frac{d\bar{y}_1}{d\alpha}$ and $\frac{d\bar{y}_2}{d\alpha}$ are both positive. That is, average household income in both districts rises as α increases. With school choice subject to capacity constraints, if $q_2 > q_1$ prior to the introduction of inter-district choice, we thus predict:

$$\frac{dN_1}{d\alpha} > 0, \quad \frac{dN_2}{d\alpha} < 0, \quad \frac{dp_1}{d\alpha} > 0, \quad \frac{dp_2}{d\alpha} < 0, \quad \frac{d\overline{y}_1}{d\alpha} > 0, \quad \frac{d\overline{y}_2}{d\alpha} > 0 .$$ [8]

In summary, the adoption of an inter-district choice plan creates an incentive for the boundary household in district 2 to move to district 1 to take advantage of lower housing prices. This in turn causes housing values in district 1 to rise and housing values in district 2 to fall. Since the households that choose to move to district 1 all have higher incomes than the set of households that currently reside in district 1, average income in district 1 must rise. Similarly, since the households that leave district 2 are the poorest residents of that district, average income in district 2 must also rise. The fact that average income rises in both districts implies that the change in district 1's income *relative* to district 2's income is ambiguous. This is in contrast to the case of frictionless school choice where there is (almost always) an unambiguous increase in district 1's income relative to district 2.

III. Data

We test the model's predictions by combining district-level data on inter-district choice transfer rates with demographic data from the 1980, 1990, and 2000 U.S. Censuses. Our data concerning inter-district transfer rates come from the restricted-use version of the NCES' 1999-2000 Schools and Staffing Survey (SASS). The SASS data include a nationally representative but incomplete sample of school districts, with data for approximately 35% of U.S. districts. We use the 1999-2000 SASS because this particular sample wave: (1) includes survey responses concerning inter-district transfers, (2) is sufficiently late given the numerous states adopting extensive inter-district choice programs in the 1990's, and (3) gathered information during the same year as the 2000 Census. We use two variables from the district-level SASS: the number of students transferring out to another district and the number of students transferring in from another district. The restricted-use version provides district identification numbers allowing us to merge these data with state open enrollment policy information and with Census data.

To obtain open enrollment policy information, we first examined state legislation, (using *LexisNexis* and state archives), that described each state's inter-district open enrollment policies.[9]

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[8] These predictions are similar to those derived by Banzhaf and Walsh (2008) who examine how exogenous improvements in local environmental quality impact population density, housing values and mean income, across communities.

[9] We also consulted Appendix B in Bierlein et al. (1993) for policy information for early-adopting states.

If necessary, we then contacted administrators working in their respective state departments of education for further information. We categorized statewide inter-district open enrollment policies based on the year of adoption and based on whether district participation was mandatory or voluntary. We classified programs as voluntary if school districts could freely choose to abstain from receiving incoming transfer students for reasons other than capacity concerns or concerns about racial imbalance of students across schools. Table 1 displays the year of adoption for the 13 states adopting mandatory programs and 13 states adopting voluntary programs between the spring of 1989 and the spring of 1998. We focus on this time frame because the 1990 and 2000 Census demographic data are based on 1989 and 1999 respectively, and general equilibrium effects might take at least one year to occur.

Based on the incoming transfer student counts in the SASS, one would estimate that about 1.3% of students in these 26 states transferred districts during the 1999-2000 school year, though this appears to slightly understate actual participation rates.[10] The underreporting of transfer rates might bias OLS estimates of general equilibrium effects if this underreporting is somehow systematically related to unexplained changes in district housing values or in district demographic composition over the 1990's. This underreporting is far less likely to bias our main estimates below, however, because we use predicted rather than actual district-level participation rates in inter-district choice.[11]

We combine these data with demographic and geographic data from the U.S. Censuses. To facilitate our empirical approach, our Census data are aggregated at two different geographic levels. We use district-level data from 1980, 1990, and 2000 concerning mean owner-occupied house values, mean household income, the percent of residents who are nonwhite, and the

[10] For example, the SASS sample suggests that 2.9% of all public school students in Minnesota were inter-district transfer students during 1999-2000, but Reback (2008) reports a 3.8% transfer rate based on state administrative data for that year. The mean net transfer rate is positive because the schools in the SASS sample collectively report greater numbers of incoming transfer students than outgoing transfer students, which may partly be due to chance and partly due to districts systematically underreporting the number of outgoing transfer students. We set transfer rates to missing for the 31 SASS districts, (less than 1.5% of the SASS sample), that reported suspiciously high positive or negative net inflows equivalent to more than 40% of their residential public school student population.

[11] The predicted district-level participation rates below are based on initial demographic differences between districts and their surrounding neighborhoods. For underreporting of 1999-2000 participation rates to bias these models' estimates, this underreporting would thus have to be: (1) related to unobserved changes in demographics or housing values during the 1990's, and (2) related to 1990 demographic differences between districts and their surrounding areas, controlling for a wide variety of other district-level variables and for state/metropolitan area fixed effects.

number of households.[12] We also use Census block-group-level data from 1990 concerning households' income and race. We have geographic data for all block-groups and for all school districts, and we use these data and ArcGIS software to determine which 1990 Census block-groups surrounded particular year 2000 school districts. We drop school districts from our sample if they changed their borders between 1990 and 2000 due to re-organizations such as mergers. Most block-groups are entirely contained within the boundaries of a single school district. In rare cases where block-groups cross district boundaries, we assume that the block-group's population is evenly distributed across its area and assign demographic characteristics to the within-block-group sub-regions located in each district based on their relative geographic size.[13]

These block-group-level data for neighboring community characteristics provide an enormous advantage over the district-level data typically used in other studies. We can examine how districts' own demographic characteristics compare with those in the surrounding neighborhoods that are very close to school districts' borders, regardless of the geographic structure of the surrounding school districts. In our analyses below, we define nearby surrounding neighborhoods as those located within ten miles of a district's borders.[14] This distance should be short enough so that the out-of-district neighborhoods offer reasonable commuting times to in-district schools, but wide enough that the out-of-district neighborhoods accurately characterize the demographics of families with access to the out-of-district schools located near the district's border. Table 2 lists the summary statistics for our Census-based variables.

IV. Empirical Methods

Models

Our main empirical analyses use a triple-differences style approach that accounts for the non-random nature of states' adoption of school choice programs. Before implementing this

[12] All of these variables are constructed using data from the special school district tabulations of the 1980, 1990 and 2000 Census made available by the National Center for Education Statistics (NCES).
[13] Districts' boundaries and block-groups' boundaries sometimes appear to cross each other simply due to imprecision in the geographic data. To avoid falsely classifying block-groups as crossing district lines, we only assign a portion of a block-group's demographics to a school district if our data suggest that more than 5% of the total geographic area of the block-group lies within that district's boundaries.
[14] Nearby, out-of-district neighborhoods include all Census block groups that have center coordinates that are located no more than ten miles away from the center coordinate of the closest in-district Census block group.

approach, we must address the endogeneity of district-level participation in these programs. School districts experiencing rising popularity during the 1990's might have relatively high inflows of transfer students during 2000 and, for reasons unrelated to the choice program, rising housing values, household incomes, and population density. The endogenous nature of district-level transfer flows could thus bias estimated general equilibrium effects of school choice opportunities towards zero. The OLS estimates reported below confirm that this is the case. To identify the effects of exogenous variation in valuable choice opportunities, our main analyses use 1990 demographic characteristics of districts and their surrounding neighborhoods to predict district-level student transfer flows during the 1999-2000 school year.

Given that there is not a price mechanism to allow the market for inter-district transfer spaces to clear, the actual number of incoming transfer students will equal the minimum of the supply of and demand for transfer spaces in a district. General equilibrium effects should depend on students' actual amount of access to valuable inter-district transfers, regardless of whether there is excess supply or demand for transfer spaces. To test for general equilibrium effects, we can thus predict transfer flows without estimating structural equations for the supply and demand for transfer spaces. We predict net transfer flows (i.e., the number of incoming transfer students minus the number of outgoing transfer students, divided by the total number of residential public school students), rather than separately predicting inflows and outflows, because there are theoretically compelling reasons why any of our independent variables affecting a flow in one direction could also affect the flow in the other direction. Given the lack of a unique exclusion restriction, including both predicted inflows and predicted outflows in our analysis would result in perfect collinearity among the independent variables. Fortunately, demographic differences across district borders are powerful predictors of net transfer flows—these variables typically affect inflows and outflows in opposite directions, resulting in strong effects on net flows.

Define $\overline{X}_{1is,1990}^{neighbor\_diff\_10miles}$ as a vector of variables which measure differences between a district's own demographic characteristics and the demographics of the pooled group of households living within a ten mile radius of the district's border. Previous studies find that cross-border differences in residential income predict variation in demand for inter-district transfer spaces (Reback 2008) and that parents shopping for schools are concerned with racial composition (Schneider and Buckley 2002; Koedel et al. 2009). Our $\overline{X}_{1is,1990}^{neighbor\_diff\_10miles}$ vector thus consists of cubic terms for the percent difference between the mean household income in district *i* and the mean household income in surrounding neighborhoods, as a well as a single term for the difference between the proportion of district *i*'s population that is white (and non-Hispanic) and the proportion of the surrounding neighborhoods' population that is white (and non-Hispanic). We use cubic terms for income differences because districts might restrict the supply of transfer spaces if they are much wealthier than their surrounding communities.[15]

Define $Policy_{s,1998}$ as a vector of two dummy variables, the first equal to one if state *s* had any inter-district choice program by 1998 and the second equal to one if district participation in the program was voluntary. We predict district-level net incoming transfer rates using the following regression equation:

$$T_{is,2000} = X_{1is,1990} Policy_{s,1998}\phi_1 + (X_{1is,1990} - X_{1is,1980}) Policy_{s,1998}\phi_2 + \overline{X}_{1is,1990}^{neighbor\_diff\_10miles} Policy_{s,1998}\phi_3 + \delta_s + \eta_{is} \qquad (7)$$

where $T_{is,2000}$ is the net incoming transfer rate for district i in state s during year 2000, $X_{1is,t}$ is a vector of demographic characteristics for district *i* during year *t* (e.g., mean household income and the percent of residents who are nonwhite), δ_s is a vector of state fixed effects, and η_{is} is a random disturbance term.

We estimate equation (7) for the more than 2,300 districts in the SASS sample that were located in states which adopted inter-district open enrollment programs during the relevant years (1989 to 1998). We then use the estimated coefficients from this model to predict net transfer rates for *all* districts (i.e., both districts included and not included in the SASS sample) in the 26 states adopting inter-district open enrollment programs during those years. This amounts to nearly 7,000 school districts. Note that because none of the 26 states in our sample had a statewide inter-district choice program in place prior to 1989 (implying formal net transfer flows were essentially zero in 1989), the dependent variable in equation (7) is actually equivalent to the *change* in formal net transfer flows between 1989 and 1999.

Using the full sample of all U.S. school districts, including districts in non-adopting states, we then estimate the following regression equation:

[15] Substantial racial differences across district borders are far more prevalent in some states than in others. The addition of higher order terms for cross-border racial differences would not significantly increase the power of the demographic difference variables in equation 7 nor would it lead to substantively different estimates of β_1 in equation 8.

$$(X^k_{is,2000} - X^k_{is,1990}) = \beta_1 \hat{T}_{is,2000} + X_{1is,1990} Policy_{s,1998} \beta_2 + (X_{1is,1990} - X_{1is,1980}) Policy_{s,1998} \beta_3 + \overline{X}^{neighbor\_diff\_10miles}_{1is,1990} \beta_4 + X_{1is,1990} \beta_5 + (X_{1is,1990} - X_{1is,1980}) \beta_6 + \delta_s + \varepsilon_{is}, \quad (8)$$

where $\hat{T}_{is,2000}$ is the predicted net transfer inflow rate for district *i*, obtained from equation (7) for districts in adopting states or equal to zero for districts in non-adopting states. The dependent variable measures the change in a demographic variable for district *i* between 1990 and 2000, ($X^k_{is,t}$ is the k^{th} demographic variable in the $X_{is,t}$ vector), and ε_{is} is a random disturbance term.

Note that the exclusion restriction allowing us to estimate β_1 in equation (8) is that the elements of the $\overline{X}^{neighbor\_diff\_10miles}_{1is,1990}$ vector—comparing district *i*'s demographics with those in the surrounding neighborhoods—have uniform coefficients in equation (8) regardless of whether state *s* has a voluntary inter-district program, a mandatory program, or no program. This restriction would be invalid if states are more or less likely to adopt inter-district choice policies when school quality across neighboring districts is becoming more homogenous. For example, states might adopt mandatory inter-district choice policies only when initially low-quality districts are suddenly catching up to their higher quality neighbors in the surrounding area.

We address the potential bias due to the non-random adoption of state policies in several ways. First, some models restrict the sample to districts located in metropolitan areas and control for metropolitan area fixed effects. This ensures that the estimates are not biased due to states adopting more extensive school choice policies when their more diverse metropolitan areas are becoming more homogenous in quality relative to their less diverse metropolitan areas. Second, we estimate "quadruple-differences" models that add control variables capturing the state-policy-specific effects of demographic disparities between districts and a wider set of surrounding neighborhoods. The inclusion of these additional controls implies we are now identifying the general equilibrium effects of choice opportunities based solely on the demographic differences between a district's own characteristics and the demographic characteristics of households living within a small (10 mile) radius of the district's border. Consequently, this specification should remove potential biases due to states adopting inter-district choice policies when relatively low-quality districts are suddenly catching up to other districts in the surrounding area. This specification should also lead to conservative estimates of the general equilibrium effects of expanded school choice because attractive transfer opportunities (and resulting changes in housing demand) likely extend further than ten miles beyond a district's borders. Finally, we test for expected variation in causal effects based on variation in the timing of states' adoption of inter-district policies, including a falsification test using states that adopted new polices after the 2000 Census.

Our baseline analyses below include all types of school districts, but our preferred specifications limit the sample to districts in metropolitan areas. One might expect larger effects of inter-district transfer opportunities in metropolitan areas for several reasons. First, there may be greater capitalization of inter-district transfer opportunities into housing values in metropolitan areas where the supply of land is relatively inelastic.[16] Second, the theoretical model developed in section II, assumes that inter-district transportation costs are negligible and that households can easily sort among districts. Those assumptions are more likely to hold in metropolitan areas, which typically contain more districts located in close proximity and as noted by Figlio et al. (2004) "have much better potential for Tiebout sorting."

V. Results

OLS Estimates of the Effects of Inter-District Choice Opportunities

For the sake of comparison with our main results reported in subsequent tables, Table 3 reports OLS estimates of the impact of net transfer inflows on changes in districts' population density, housing values, and mean resident income. We restrict our sample to districts in the Schools and Staffing Survey (SASS) and estimate equation (8) using actual net transfer inflow rates from formal choice programs. This net transfer inflow measure has a standard deviation of 5.0%, and, as expected, districts in states with formal choice programs have much higher reported unidirectional transfer flows than districts in other states.[17] While Table 3 only reports the key coefficients of interest, all specifications include the full set of control variables listed in Table 2, (except for the district–neighbor demographic comparisons). Column 1 of Table 3 reports parameter estimates when the dependent variable is the percent change in the number of households, column 2 reports estimates when the dependent variable is the percent change in the number of households with children, and columns 3 and 4 report estimates when the dependent

[16] See Hilber and Mayer (2009) for recent empirical evidence on land supply elasticity and capitalization.

[17] The average percent inflow in states with formal choice programs is 2.6% while it is only 0.6% in other states. In rare cases, students may cross districts lines in other states based on decentralized agreements between school districts. In all models, we set districts' transfer rates to zero in states without formal programs, because we only anticipate general equilibrium effects due to the adoption of formal choice programs.

variable is the change in the average value of owner-occupied homes and the change in the average income of households, respectively.

Panel A of Table 3 reports results based on the full sample of school districts contained in the SASS. Recall that our theoretical model predicts increased relative population density in initially low-quality districts and decreased relative population density in initially high-quality districts. The estimated coefficient on the predicted net inflow variable reported in columns 1 and 2 should thus be negative: districts with positive net inflows should experience a decline in the number of households while districts with negative net inflows (i.e., net outflows) should experience an increase in the number of households. Consistent with that prediction, the estimated coefficients on the net inflow variable reported in columns 1 and 2 are negative and statistically significant, though they are small—a one percentage point increase in net transfer inflows is associated with only a 0.19% decrease in the number of households residing in the district and with only a 0.21% decrease in the number of households with children. Our theoretical model also predicts an increase in housing values in initially low-quality districts and a decrease in initially low-quality districts. Consistent with that prediction, the estimated coefficient on the net inflow transfer variable reported in column 3 is negative, though it is small in magnitude and statistically insignificant. The estimated coefficient reported in column 4 (average household income) is also small in magnitude and statistically insignificant.

Panels B and C of Table 3 report OLS estimates when we restrict the sample to school districts located in metropolitan areas. Panel C adds controls for metropolitan area fixed effects. [18] The estimated coefficients are all much larger in magnitude and of greater statistical significance than the corresponding estimates for the pooled sample models reported in panel A. Consistent with greater capitalization effects in metropolitan areas, the estimated coefficient on the net transfer inflow variable reported in column 3 of panel B, (-$408), is more than 10 times larger than the corresponding estimate reported in panel A.

Overall, the signs of the estimates in Table 3 are consistent with our theoretical predictions, but the estimated effects on districts' housing values and mean income are small and not very statistically significant. The main analyses below confirm that these OLS estimates are biased toward zero due to the endogeneity of district-level transfer rates.

[18] Several MSA's cross state boundaries. Our empirical specification includes state fixed effects for those MSA's.

Predicted Within-state Variation in Net Student Transfer Flows

Table 4 displays our estimation results for equation (7). The first eight rows display the estimated coefficients composing the ϕ_3 vector, which help identify the intensity of treatment in our main analyses. These estimates are jointly significant at the .0004 level, with an F-statistic greater than 3.5. We thus have sufficient power to identify meaningful general equilibrium effects from equation (8) after first assigning districts to various levels of predicted intensity of treatment. While our predictions may be somewhat imprecise, this simply means that our difference-in-differences estimates in equation (8) will have larger standard errors and may be slightly biased toward the smaller OLS estimates, i.e., biased toward zero.

The estimates reported in Table 4 correspond with the version of equation (7) used to examine the percent change in the number of households in districts as the dependent variable in equation (8); in other versions, we replace the number of households control variables with control variables for lagged levels and lagged changes of the appropriate outcome of interest (e.g., lagged levels and changes in mean house values for the housing value models). In each version, the eight coefficients on the policy interaction terms remain similar and are jointly significant at the .004 level or better; the F-statistic is 2.9 for the housing values model and greater than 3 for all other models. Even for the extended versions of these models that control for the state-policy-specific effects of demographic disparities between districts and a wider set of surrounding neighborhoods—i.e., our quadruple-differences specification—the F-statistics range from 2.8 to 3.3.

The direction of the estimated coefficients in the first eight rows of Table 4 are revealing—they suggest that net incoming transfers are greatest when a district has a greater share of white residents and moderately greater mean household income than in surrounding neighborhoods. Racial differences are very strong predictors of incoming transfer students for districts in states requiring mandatory participation. The coefficient of the racial difference variable is statistically significant at the .001 level and suggests that a ten percentage point increase in a district's own share of residents who are white, holding surrounding neighborhoods' racial composition constant, leads to about a 0.7 percentage point increase in the predicted net incoming transfer rate. In contrast, this change only leads to slightly more than a 0.1 percentage point increase in the predicted net incoming transfer rate for districts in states with voluntary participation; a difference in estimated slopes which is statistically significant at the .02 level. These patterns are consistent with parents preferring to send their children to non-residential districts with fewer minority students and districts wanting to restrain the supply of incoming transfer spaces that may potentially be used by nonwhite students.

In mandatory participation states, districts with relatively high incomes also tend to attract a greater net inflow of transfer students, though this effect diminishes in size as the income gaps become very large. Incoming transfers continue to increase until a district is more than 72% wealthier than surrounding neighborhoods. Even in mandatory participation states where districts are compelled to admit at least some incoming transfer students, districts may restrict the supply of incoming transfer spaces by reporting relatively low capacity levels. Reback (2008) finds evidence that districts in a mandatory participation state are more likely to limit the supply of incoming transfer spaces when their median residential income exceeds those of neighboring districts. As expected, the cubic terms' coefficients suggest that districts in voluntary participation states have a quicker rate of decline in the positive slope of the income gap—incoming transfers stop increasing once a district is more than 34% wealthier than surrounding neighborhoods. These reduced-form models thus have sensible coefficients and provide sufficient power for us to reliably estimate general equilibrium effects based on predicted net transfer flows.

Estimates of the Effects of Inter-District Choice Opportunities Using Predicted Net Transfers

Table 5 reports our estimates of equation (8). Table 5 and all subsequent tables display two-sample bootstrapped standard errors based on 1,000 Monte Carlo simulations, a method used previously by Bjorklund and Jantti (1997). For each simulation, we randomly draw with replacement from the entire national sample of school districts used to estimate equation (8), with stratification at the state-level and additional stratification to ensure that we maintain the same sample size for our estimation of equation (7). In the interest of brevity, Table 5 reports only the estimated coefficients on the net transfer flow variable, but the Appendix displays the full regression results associated with the first column of Table 5.

Results based on the full sample of school districts located in the continental United States are reported in panel A of Table 5. The estimated coefficients in Table 5 reflect the impact of a one percentage point change in predicted net inflows, which is a fairly large change—a one standard deviation change in predicted net inflows for districts in states with an inter-district policy is only 1.2 percentage points. The estimated coefficients on the predicted net inflow variable reported in panel A are all negative but small in magnitude and statistically insignificant. A one percentage point increase in predicted net inflows leads to only a 0.23% decrease in the number of households residing in the district and a $122 decrease in housing values. For the national sample of all districts, we find no evidence of large average general equilibrium effects associated with inter-district transfer opportunities.

Panel B of Table 5 reports results restricting the sample to school districts located in metropolitan areas. These results strongly support our theoretical predictions. The estimated coefficients are much larger in magnitude and far more statistically significant than the corresponding estimates for the pooled sample models of panel A, [19] and they are also much larger than the corresponding OLS estimates displayed in Table 3. Expanded transfer opportunities affect population density and house prices in metropolitan-area districts—a one percentage point increase in predicted net transfer inflows decreases the number of households living in a district by 1.4 percent and decreases mean house values by $3,997. Consistent with choice with only partial capacity constraints, transfer opportunities also moderately affect the average household income among school districts' residents. A one percentage point increase in net transfer inflows decreases a district's residents' mean household income by $1,043. These estimates imply that a one standard deviation increase in net transfer inflows decreases year 2000 mean house values and mean household income by 0.04 standard deviations each.

Panel C of Table 5 reports estimates from specifications that continue to limit the sample to metropolitan-area districts and also control for metropolitan area fixed effects. By identifying the impact of transfer opportunities based solely on within-metro-area variation, this specification substantially reduces our concerns that the estimates may be biased by the non-random nature of states' adoption of choice programs. The point estimates reported in columns 1, 2, and 4 (population density and mean income) are similar in magnitude to the estimates reported in the corresponding columns of panel B and all three estimates remain statistically significant. In contrast, the point estimate reported in column 3 (housing values) is about two-

[19] In all four specifications reported in Table 5, we can reject the null hypothesis that the estimated coefficient on the predicted net inflow variable for the sample of metropolitan districts (panel B) equals the estimated coefficient on the predicted net inflow variable for the sample of non-metropolitan districts (not reported here). For the population density results reported in columns 1 and 2, differences in slopes across metropolitan and non-metropolitan districts are statistically significant at the .10 level, the housing value differences are statistically significant at the .01 level and the income differences are statistically significant at the .02 level.

thirds as large as the corresponding estimate in Panel B and is no longer statistically significant. This difference may be due to unobserved differences in trends in metropolitan areas or due to legitimate variation across metropolitan areas in how choice opportunities are capitalized into housing values. Even if the estimates in Panel C are conservative, they suggest economically important effects for metropolitan-area districts—for a district with mean characteristics, a one percentage point change in net transfer inflows results in a 2.4% change in housing values and 2.3% change in income.

Robustness Check: Quadruple-differences Models

Table 6 reports results based on our quadruple-differences specifications that add controls for state-policy-specific effects of demographic disparities between districts and a wider set of surrounding neighborhoods. In both equation (7) and equation (8), we control for the state-policy-specific effects of demographic differences between a district and the surrounding neighborhoods within fifty miles. The results continue to suggest that expanded inter-district transfer opportunities are associated with theoretically predicted changes in population flows and housing values. All of the estimated coefficients on the predicted net inflow variable reported in Table 6 remain negative; the estimated effects on households with children (column 2) and on mean income (column 4) remain statistically significant. As we previously discussed in Section IV, Table 6 provides conservative estimates because the additional control variables in these models absorb some of the intra-metropolitan area variation in school choice opportunities. This might explain why the housing value estimates in particular are smaller in Table 6 than in Table 5, though these differences are not statistically significant. We continue to find statistically significant effects on changes in the number of households with children even if we change the extra set of controls to be based on thirty mile surrounding areas rather than fifty mile surrounding areas.[20]

Along with the MSA-fixed effect models in panel C of Table 5, these quadruple-differences results are very reassuring. Recall that the policy-specific slopes on the initial own-versus-surrounding-neighborhood demographic variables are the identifying source of exogenous within-state variation in how school choice policies should affect districts. These results show

[20] We prefer the fifty mile results because these thirty mile results have fairly large standard errors given that there are rarely large demographic differences between 10 mile and 30 mile surrounding areas. The F-statistic for the joint significance of the 10 mile comparison variables in equation 7 equals 2.2 in the population density models that control for 30 mile comparisons.

that this variation is an important predictor of general equilibrium effects even when controlling for heterogeneous effects of how districts' demographics initially compare with those in other neighborhoods in their local region. While states' adoption of choice programs may be non-random, adoption should not be systematically related to a pattern of within-state (or within-MSA) variation in breaks from prior trends that would bias estimates in these models. To illustrate that point, consider two hypothetical districts, Districts A and B, that are located in the same metropolitan area in the same state and have similar surrounding districts within a 50-mile radius. Suppose that these districts are initially in the same place of the quality distribution among districts in their metropolitan area. Compared to District B, however, District A has higher quality surrounding neighborhoods within a ten-mile radius. Are states more likely to adopt inter-district choice policies when districts like District A are suddenly experiencing increases in population density and income compared to districts like District B? While this scenario may be possible, it does not seem particularly likely and the converse relationship seems just as plausible—states might adopt choice programs to avoid having to provide greater state education aid to help districts compete with their superior-quality surrounding districts.[21] The next subsection investigates this issue more directly and the findings further strengthen our claim that the results in Tables 5 and 6 should be viewed as conservative estimates of the general equilibrium effects of choice programs.

Falsification and Robustness Tests: The Timing of States' Policy Adoption

While the results reported in Table 5 and 6 provide compelling evidence that inter-district transfer opportunities affect housing values and residential location choices, one might still be concerned that a state's decision to adopt an inter-district choice policy is correlated with recent within-state variation in demographic trends. We further address this concern by conducting a series of falsification and robustness tests. The idea behind our falsification tests is simple: if the results reported in Tables 5 and 6 are truly causal, then they should only hold for districts in states that adopted inter-district choice plans between 1989 and 2000 and *not* for districts in states that adopted inter-district choice plans after 2000. That is, future (post-2000) adoption of inter-district choice plans should not influence changes in district demographics and housing values between 1990 and 2000. To implement our falsification tests we exploit the fact that five

[21] For example, the NAACP's lawsuit in Minneapolis over the inadequacy of public schooling was settled when the surrounding suburban districts agreed to guarantee a minimum number of inter-district transfer spaces.

states adopted inter-district choice plans after the 2000 Census.[22] For districts located in these states we first find counterfactual year 2000 predicted net inflows based on our original estimated coefficients from equation (7). We then re-estimate equation (8) and include an independent variable measuring these counterfactual predicted net inflows for states that adopted an inter-district choice plan after 2000. To be consistent with our main specification, we also expand the policy-specific control variables to include interaction terms with an indicator for post-2000 adoption.

The top panel of Table 7 reports results based on these falsification tests for the metropolitan area fixed effect model (equivalent to Panel C of Table 5). We report estimated coefficients of the predicted net inflow variable for states that adopted inter-district choice plans during the 1990's directly above the counterfactual estimates for states that adopted inter-district choice plans after 2000. Reassuringly, unlike our actual estimates of general equilibrium effects, none of the coefficients for the post-2000 adopting states are negative. In columns 1 and 2, the estimated coefficients for post-2000 adopting states are small in magnitude and statistically insignificant. In columns 3 and 4, the estimated coefficients for post-2000 adopting states are positive, relatively large in magnitude and statistically significant. The final row of the top panel of Table 7 presents p-values for the one-tailed test that the counterfactual slope is more positive than the corresponding actual slope. For the housing value and mean income results (columns 3 and 4) we can confidently ($p<.10$) reject the null hypothesis that the two coefficients are equal in favor of the hypothesis that the counterfactual slope is more positive. These results suggest that, if anything, the estimated effects on housing values and income in Panel C of Table 5 may be biased downward—states may be more likely to adopt choice programs when relatively unpopular districts are suddenly falling behind their more popular neighbors in terms of housing values and residential income. Fortunately, the quadruple-differences estimates of Table 6 should not be influenced by this issue, though those estimates will also be conservative (as discussed above).

The bottom panel of Table 7 presents the results of an additional robustness check of our core results. We separated states that adopted inter-district choice plans during the 1990's into two groups: those that adopted a plan in 1996 or earlier and those that adopted a plan in 1997 or

[22] These late-adopting states were Florida, Georgia, Louisiana, Maine, and Mississippi. Among those states, Mississippi adopted a mandatory policy while the other states adopted voluntary policies. We adjust our counterfactual predicted net transfer flows for these states accordingly.

1998. We then re-estimated our models and included unique independent variables measuring the predicted net inflows for states in each of these groups. One would expect the general equilibrium effects associated with expanded school choice to manifest themselves gradually. We thus expect inter-district transfer opportunities to have larger effects on the residential location decisions of families and on housing values in states that adopted plans earlier in the relevant time period. This is precisely what we find. With the exception of the mean income results (column 4) all of the estimated coefficients on the predicted net inflow variable for states that adopted plans in 1996 or earlier are much larger in magnitude than the corresponding estimates for states that adopted plans in 1997 or 1998. While all of the estimated coefficients for states that adopted plans by 1996 are statistically significant, none of the coefficients for states that adopted plans in the late 1990's are statistically significant. Given the relatively large standard errors, the p-values for rejecting the null hypothesis of equal slopes for early versus late 1990's adopters are somewhat large—ranging from .11 to .19 for the population density and housing value models and equal to .37 for the income model. [23]

Separating the Effects of Net Inflows from Net Outflows

All the results reported thus far are based on a specification that assumes the effect of predicted net inflows on changes in district demographics and housing values is linear. By modeling changes in district demographics as a linear function of predicted net inflows, we are restricting the effect of increases in net outflows and increases in net inflows to be equal in magnitude but opposite in sign. While this assumption allowed us to preserve the same functional form for predicted inflows across equations (7) and (8), this assumption is inconsistent with the results of several prior studies using computable general equilibrium models to examine the impact of expanded school choice. For example, Nechyba (2003a, 2003b) finds that introducing a universal voucher program into a previously residentially zoned school system increases average income and property values in initially low-quality districts by larger amounts than it decreases average income and property values in initially high-quality districts.[24] In this section, we therefore relax the assumption that the effect of predicted net inflows on changes in

[23] We also tested whether the estimated coefficients for the early 1990's adopting states in the bottom panel of Table 7 were statistically different from the estimated coefficients for the counterfactual adopting states in the top panel of Table 7. For the housing value and mean income results, we can reject the null hypothesis that the two coefficients are equal at the .05 level or better.

[24] See the top panel of Table 5.4 in Nechyba (2003a) and Table 5 of Nechyba (2003b).

district demographics and housing values is linear by allowing for unique effects of positive net transfer inflow and negative net transfer inflow (i.e., positive net transfer outflow).

Define $D_{is,t}$ as a dummy variable equal to one if and only if district *i* has a positive predicted net inflow in year *t*, so that:

$$D_{ist} = 1 \quad \text{if } \hat{T}_{is,t} \geq 0$$
$$= 0 \quad \text{if } \hat{T}_{is,t} < 0$$

Using this dummy variable, we expand Equation (8) to allow for unique effects of predicted net inflows and predicted net outflows as follows:

$$\begin{aligned}(X^k_{is,2000} - X^k_{is,1990}) &= \gamma_1 \hat{T}_{is,2000}(D_{is,2000} - 1) + \gamma_2 \hat{T}_{is,2000} D_{is,2000} + \gamma_3 D_{is,2000} + X_{1is,1990} Policy_{s,1998}\gamma_4 \\ &+ (X_{1is,1990} - X_{1is,1980}) Policy_{s,1998}\gamma_5 + \overline{X}^{neighbor\_diff}_{1is,1990}\gamma_6 + X_{1is,1990}\gamma_7 + (X_{1is,1990} - X_{1is,1980})\gamma_8 + \delta_s + \varepsilon_{is},\end{aligned} \tag{9}$$

In Equation (9), γ_1 represents the effect of increases in net outflow for districts with predicted net outflows, γ_2 represents the effect of increases in net inflow for districts with predicted net inflows, and γ_3 allows for a unique intercept for districts that experienced net inflows versus net outflows.[25]

Results based on Equation (9) are reported in Table 8. In the interest of brevity, we report results only for the sample of districts located in metropolitan areas. In addition, we report only the estimated coefficients on the outflow and inflow variables but note that all specifications include the full set of control variables.[26] The top panel reports estimates for specifications that include state fixed effects while the bottom panel reports estimates for specifications that include metropolitan area fixed effects.

The population density results (columns 1 and 2) confirm our theoretical prediction that the adoption of inter-district choice increases population density in initially low-quality districts—a one percentage point increase in predicted net outflows causes a more than 2% increase in population density. The estimates reported in columns 1 and 2 of the bottom panel imply that a one standard deviation increase in net outflows increases the year 2000 number of

[25] As noted in section III, mean net inflows are often positive due to the underreporting of outflows, we therefore first demean net inflows by state and use the demeaned net inflow variable to construct the net inflow and net outflow variables.

[26] We also do not report the estimated coefficients on the dummy variable for whether a district has positive predicted net inflow. The estimated coefficients on that variable were never statistically significant.

households by 0.0036 standard deviations and the number of households with children by 0.006 standard deviations. The negative estimated coefficients on the inflow variable reported in columns 1 and 2 also suggest that population density decreases in initially high-quality districts after the adoption of choice, but these inflow estimates are not statistically significant. Inter-district choice opportunities should be most relevant to households with children, so it is reassuring that the point estimates in column 2 are larger in magnitude than the point estimates in column 1. The estimate in column 2 may seem moderate in terms of standard deviation changes, but it is large considering that a one percentage point increase in predicted outflow leads to more than a 3.7 percent increase in the number of households with children. This large response suggests that families may appreciate the option value of outside transfer opportunities and that there may be large social multiplier effects, similar to those found by Bayer, Ferreira and McMillan (2005), whereby attractive schooling opportunities lead to an influx of relatively wealthy families.[27]

The estimates in column 3 reveal substantial and statistically significant effects of school choice transfer opportunities on housing values—a one percentage point increase in predicted outflow increases housing values by $6,342 in the state fixed effects model and by $3,631 in the MSA fixed effects model. The latter estimate is nearly identical to Reback's (2005) analogous estimate for capitalization effects in Minnesota, while the former estimate is much larger.[28] Districts in metropolitan areas should experience larger capitalization effects than the average Minnesota school district, so it is not surprising that limiting the sample to metropolitan-area districts produces relatively large capitalization effect estimates in models that do not control for metropolitan area fixed effects. As expected, the estimated coefficients on the net inflow variable reported in column 3 are negative, though they are statistically insignificant. Expanded inter-district transfer opportunities affect home values in initially low-quality districts more

[27] Bayer, Ferreira and McMillan (2005) find that the general equilibrium effects of changes in school quality on housing values are much larger than the direct (partial equilibrium) effect. They attribute the larger general equilibrium effects to the presence of a strong social multiplier, whereby initial changes in school quality lead to changes in the residential location decisions of families, with high-income and highly-educated families relocating to areas that experience increases in school quality.

[28] Reback (2005) finds that, for Minnesota districts with net outflows of transfer students, a one standard deviation increase in initial outgoing transfer flow is associated with approximately a 4.4% increase in housing values, (see Reback's Table 6). Multiplying the estimated coefficient of the predicted net outflow variable in column 3 of Table 8 by 1.26 (a one standard deviation change) and dividing by the mean 1990 housing values in MSAs ($107,864) implies that a one standard deviation increase in predicted net outflow is associated with approximately a 7.4% (top panel) or 4.2% (bottom panel) increase in housing values.

strongly than home values in initially high-quality districts, which is consistent with Nechyba's (2003a, 2003b) conclusion that a private school voucher program should cause the largest changes in property values for the initially low-quality public school districts. Finally, net outflows positively affect mean income (column 4) and net inflows negatively affect mean income, though neither estimate is statistically significant.

VI. Conclusions

Theoretical models of residential sorting suggest that the adoption or expansion of school choice programs can have important general equilibrium effects in terms of housing markets and residential sorting. In this paper, we provide the first direct empirical test of whether those predicted general equilibrium effects occur. We use a multi-community model to derive predictions concerning the impact of expanded inter-district transfer opportunities on housing values and the residential location decisions of families. Our model predicts that the introduction of inter-district choice into a previously residentially zoned school system should increase population density in initially low-quality districts and decrease population density in initially high-quality districts. Our model also predicts that housing values should rise in initially low-quality districts and decline in initially high-quality districts. Our empirical analysis confirms that these general equilibrium effects are present even in a context in which rates of participation in a choice program are fairly low. A small expansion of inter-district transfer opportunities can lead to substantial changes in the distribution of families across school districts and in metropolitan-area housing values. Our results are consistent with the prediction that the adoption of an inter-district choice program creates an incentive for relatively high-income households with children to relocate to previously lower-quality districts to take advantage of lower housing prices.

These results are not only highly consistent with the qualitative findings of theoretical studies that examine the general equilibrium effects of expanded choice (e.g., Nechyba 2000, 2003a, 2003b; Epple and Romano 2003; Ferreyra 2007), they are also of a similar order of magnitude to the simulated general equilibrium effects found in those studies for cases where policies modestly expand school choice. Nechyba' (2003a) simulations suggest that a $1,000 private school voucher introduced into a region of three representative, locally financed public school districts in New Jersey would cause a 7.4% increase in mean household income and a

10.9% increase in housing values for the lowest-quality district. Ferrerya's (2007) simulations suggest that the adoption of a $1,000 non-sectarian private school voucher in Chicago would cause a 1.1% decline in the ratio of housing value between the highest- and lowest-wealth districts and a 1.1% decline in the ratio of household income in these districts. Our estimates in Panel C of Table 5 reveal that the magnitude of the general equilibrium effects of inter-district public school choice programs lies in between these authors' estimates of the general equilibrium effects associated with modest private school voucher programs. On average, inter-district choice would increase residential incomes by 3.0% ($1,041) and home values by 3.4% ($2,890) for metropolitan-area districts in the bottom third of the predicted net outflow distribution. Our population density estimate in Panel C of Table 5 also suggests that far more than 1% of households relocate due to expanded inter-district public school choice.[29] This mobility response is comparable to Ferreyra's (2007) estimate that 4% of households would relocate their residences in response to a modest voucher program in Chicago.

In addition to providing empirical evidence in support of the previous theoretical literature concerning the general equilibrium effects of school choice programs, our results provide unique evidence in favor of Tiebout's hypothesis that people "vote with their feet" in response to changes in public service provision. In that sense, our results complement the recent results of Banzhaf and Walsh (2008) who find that household migration patterns are highly correlated with changes in local environmental quality. Our finding that the adoption of an inter-district choice program causes income and housing values to rise in previously low-quality districts suggests that such programs may reduce residential income stratification and induce gentrification effects similar to those found by Banzhaf and Walsh (2008) in the context of improved environmental quality and Kahn (2007) in the context of improved access to rail transportation. Residential homogeneity increases across local districts when excludable local public services become less exclusive.

[29] This minimum 1% estimate assumes that all residential moves affect districts' population densities via property abandonment and new construction; in reality, the vast majority of residential moves should consist of households moving into previously-occupied homes.

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Table 1: States' Inter-District Open Enrollment Policies

State	(1) Year passed	(2) Could school districts abstain from receiving incoming transfer students?
Arizona	1994	no
Arkansas	1989	no
California	1994	yes
Colorado	1994	no
Connecticut	1997	yes
Delaware	1996	no
Idaho	1990	yes
Iowa	1989	no
Kentucky	1992	yes
Massachusetts	1991	yes
Michigan	1996	yes
Minnesota	1989*	no*
Montana	1993	yes
Nebraska	1989	no
New Hampshire	1998	yes
New Mexico	1998	no
North Dakota	1993	yes
Ohio	1993	yes
Oklahoma	1990	no
Oregon	1991	yes
South Dakota	1997	no
Tennessee	1992	yes
Texas	1995	yes
Utah	1993	no
Washington	1993	no
Wisconsin	1997	no

Notes: Policy information was based on state legislation that described each state's relevant policies (using LexisNexis and state archives), as well as Appendix B from Bierlein et al. (1993). We also contacted state department of education officials to resolve cases in which policy details were not obvious from the state legislative code. States not listed in this table did not adopt an inter-district open enrollment policy during our time period.

* Minnesota's program began in 1987 but district participation was not mandatory until 1989.

Table 2
Summary Statistics

	All Districts Continental U.S.		Districts Located in MSAs	
Variable	**Mean**	**St. Dev**	**Mean**	**St. Dev**
Dependent Variables:				
Change Between 1990 and 2000				
Households (Change as a Percentage)	12.29	13.80	14.33	14.37
Households with Children (Change as a Percentage)	10.57	16.79	14.33	16.65
Mean House Value	$39,917	$30,409	$46,325	$35,139
Mean Household Income	$17,021	$6,829	$19,185	$7,758
Independent Variables:				
Change Between 1980 and 1990				
Households (Change as a %)	10.68	18.28	14.86	18.47
Households with Children (Change as a %)	-12.92	22.61	-11.03	24.31
Mean House Value	$37,219	$45,552	$53,761	$53,891
Mean Household Income	$15,570	$8,680	$19,065	$9,937
Percent Non-White	1.68	3.79	2.29	4.18
1990 Level				
Households	7,496	36,807	11,776	49,960
Households with Children	2,770	12,508	4,331	16,948
Mean House Value	$83,606	$65,277	$109,845	$75,953
Mean Household Income	$34,751	$13,362	$40,761	$15,084
Percent Non-White	11.53	17.04	12.90	17.51
Surrounding Neighborhoods' Characteristics (Block Groups) Minus own District Characteristics				
Mean Household Income (Percent Difference)	0.13	20.44	1.63	24.47
Percent White (Percentage Point Difference)	-0.75	12.87	-0.04	14.55

Note: These summary statistics are based on the sample of districts used to examine the percent change in the number of households in districts between 1990 and 2000—i.e., column 1 of Table 5. The continental U.S. sample includes 11,769 districts, and the metropolitan-area sample includes 6,264 districts.

Table 3
OLS Results

		(1) Percent Change in # of Households	(2) Percent Change in # of Households with Children	(3) Change in Mean House Values	(4) Change in Mean Household Income
A.	*All Districts Continental U.S. with State F.E.*				
	Net Transfer Inflows	-0.190**	-0.214*	-34	9
		(0.068)	(0.084)	(102)	(23)
	Observations	3,836	3,836	3,880	3,831
	R-Square	0.43	0.30	0.64	0.55
B.	*Districts in Metropolitan Areas with State F.E.*				
	Net Transfer Inflows	-0.366**	-0.427**	-408*	-71
		(0.122)	(0.153)	(212)	(51)
	Observations	2,149	2,149	2,167	2,135
	R-Square	0.49	0.34	0.66	0.62
C.	*Districts in Metropolitan Areas with Metro-Area F.E.*				
	Net Transfer Inflows	-0.365**	-0.448**	-292	-67
		(0.101)	(0.165)	(185)	(50)
	Observations	2,150	2,150	2,170	2,140
	R-Square	0.59	0.47	0.79	0.68

Notes: Each column represents estimated coefficients from a separate regression, including control variables for lagged trends in demographic variables (changes between 1980 and 1990), levels of these demographic variables in 1990, differences between district *i*'s own demographic characteristics and the demographic characteristics of district *i*'s neighbors (10 mile radius) in 1990, and state or MSA fixed effects. Transfer rates are measured as 100 times the decimal form (1=1%). See Table 2 for summary statistics for these variables. Robust, clustered standard errors are in parentheses below each estimated coefficient. In order to limit the impact of outliers due to misreported values of the dependent variable, the sample excludes districts with values of each dependent variable that are in the top 0.5% or bottom 0.5% of the overall distribution. The reported sample sizes are rounded to the nearest 10 to comply with restricted-use data reporting requirements. * indicates significant at the 10% level and ** significant at the 5% level.

Table 4
Predicting Within-State Variation in Net Student Transfer Rates

	Coefficient		Standard Error
Surrounding Neighborhoods' Characteristics (Block Groups) Minus own District Characteristics			
Mean Household Income (Percent Difference)			
Alone	0.0019		0.0135
Alone Squared	0.1279	***	0.0401
Alone Cubed	-0.1194	**	0.0505
Interacted with Voluntary Receiving	0.0124		0.0166
Interacted with Voluntary Receiving Squared	-0.1519	***	0.0463
Interacted with Voluntary Receiving Cubed	0.1341	**	0.0524
Percent White (Percentage Point Difference)			
Alone	0.0691	***	0.0189
Interacted with Voluntary Receiving	-0.0569	**	0.0248
Other Independent Variables			
# of Households in 1990 (thousands)			
Alone	-0.0116		0.0074
Interacted with Voluntary Receiving	0.0087		0.0080
Mean Household Income in 1990 ($ thousands)			
Alone	-0.136	**	0.054
Interacted with Voluntary Receiving	0.010		0.075
Percent Non-White in 1990			
Alone	0.0189		0.0158
Interacted with Voluntary Receiving	-0.0214		0.0217
Percentage Change in the # of Households, 1980 to 1990			
Alone	0.0004		0.0084
Interacted with Voluntary Receiving	-0.0328	***	0.0115
Change in Mean Household Income ($ thousands), 1980 to 1990			
Alone	0.2504	***	0.0889
Interacted with Voluntary Receiving	-0.0829		0.1199
Change in the Percent Nonwhite, 1980 to 1990			
Alone	-0.0342		0.0505
Interacted with Voluntary Receiving	0.0333		0.0642

Notes: "Interacted with Voluntary" refers to variables multiplied by an indicator equal to one if the district's state did not require all districts to admit transfer students. The R-squared for this regression equals 0.064 and the sample size equals 2,240, (rounded to the nearest 10 to comply with restricted-use data reporting requirements). * indicates significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table 5
Effects of Transfer Opportunities on Residential Sorting and Housing values

	(1) Percent Change in # of Households	(2) Percent Change in # of Households with Children	(3) Change in Mean House Values	(4) Change in Mean Household Income
A. *All Districts Continental U.S. with State F.E.*				
Predicted net inflows	-0.234	-0.911	-122	-224
	(0.602)	(0.732)	(1,007)	(364)
Observations	11,769	11,769	12,011	11,719
R-Square	0.337	0.213	0.622	0.511
B. *Districts in Metropolitan Areas with State F.E.*				
Predicted net inflows	-1.382	-1.922**	-3,997**	-1,043*
	(0.913)	(0.954)	(2,027)	(569)
Observations	6,264	6,264	6,343	6,202
R-Square	0.394	0.248	0.622	0.553
C. *Districts in Metropolitan Areas with Metro-Area F.E.*				
Predicted net inflows	-1.665*	-2.350**	-2,668*	-943*
	(0.967)	(1.128)	(1,632)	(567)
Observations	6,264	6,264	6,343	6,202
R-Square	0.449	0.323	0.742	0.597

Notes: Each column of each panel represents estimated coefficients from a separate regression. Regressions include control variables for lagged trends in demographic variables (changes between 1980 and 1990), levels of these demographic variables in 1990, demographic differences in 1990 between district *i* and surrounding neighborhoods (10 mile radius), and state or MSA fixed effects. Predicted transfer rates are measured as 100 times the decimal form (1=1%). See Table 2 for summary statistics for these variables. Bootstrapped standard errors are in parentheses below each estimated coefficient. In order to limit the impact of outliers due to misreported values of the dependent variable, the sample excludes districts with values of each dependent variable that are in the top 0.5% or bottom 0.5% of the overall distribution. * indicates significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table 6
Quadruple-difference Estimates

	(1) Percent Change in # of Households	(2) Percent Change in # of Households with Children	(3) Change in Mean House Values	(4) Change in Mean Household Income
Districts in Metropolitan Areas with State F.E.				
Predicted net inflows	-1.155	-2.358**	-1,993	-814**
	(0.786)	(0.916)	(1,911)	(399)
Observations	6,264	6,264	6,343	6,202
R-Square	0.400	0.253	0.626	0.561
Districts in Metropolitan Areas with Metro-Area F.E.				
Predicted net inflows	-1.471	-2.728**	-1,646	-870**
	(0.951)	(1.157)	(1,751)	(397)
Observations	6,264	6,264	6,343	6,202
R-Square	0.452	0.327	0.744	0.605

Notes: See Notes to Table 5. Additional control variables in these models capture the policy-specific effects of demographic differences in 1990 between district *i* and nearby neighborhoods within a 50 mile radius.

Table 7
Falsification and Robustness Tests

		(1) Percent Change in # of Households	(2) Percent Change in # of Households with Children	(3) Change in Mean House Values	(4) Change in Mean Household Income
Actual Versus Post 2000 Adopters					
(1)	Predicted Net Inflows for 1990's Adopters	-1.612*	-2.281**	-2,606	-924*
		(0.961)	(1.115)	(1,626)	(567)
(2)	Fake Predicted Net Inflows for 2000's Adopters	0.911	0.0317	5,888*	1,554**
		(2.072)	(2.737)	(3,276)	(718)
	p-value	0.18	0.30	0.06	0.02
Early Versus Late Adopters					
(1)	Predicted Net Inflows for early 1990's Adopters (pre-1997)	-1.708**	-2.444**	-3,585**	-920**
		(0.856)	(1.024)	(1,586)	(552)
(2)	Predicted Net Inflows for late 1990's Adopters (1997 or 1998)	0.157	-0.317	378	-1,211
		(1.317)	(1.309)	(2,215)	(772)
	p-value	0.19	0.15	0.11	0.37

Notes: See Notes to Table 5. Each regression includes separate terms for the predicted net transfer flows for districts in two groups of states. The top panel displays unique estimates for districts in states that actually adopted inter-district choice during the 1990's and in states that adopted inter-district choice after 2000. The bottom panel displays unique estimates for districts in states that adopted inter-district choice before 1997 and for districts in states that adopted inter-district choice in either 1997 or 1998. Reported *p*-values are for a one tailed test with the alternative hypothesis that the first group's coefficient is less than (i.e., more negative than) the second group's coefficient.

Table 8
Separate Estimates for Inflow and Outflow Districts

	(1) Percent Change in # of Households	(2) Percent Change in # of Households with Children	(3) Change in Mean House Values	(4) Change in Mean Household Income
Districts in Metropolitan Areas, State F.E.				
(1) Predicted Net Outflow	2.093*	3.742**	6,342**	908
	(1.232)	(1.542)	(2,657)	(731)
(2) Predicted Net Inflow	-0.559	-0.644	-2,611	-1,060
	(1.149)	(1.271)	(2,998)	(768)
Observations	6,264	6,264	6,343	6,202
R-Square	0.395	0.249	0.623	0.553
Districts in Metropolitan Areas, Metro F.E.				
(1) Predicted Net Outflow	2.565**	4.508**	3,631*	807
	(1.265)	(1.713)	(2,089)	(715)
(2) Predicted Net Inflow	-1.004	-1.285	-2,292	-956
	(1.193)	(1.325)	(2,401)	(774)
Observations	6,264	6,264	6,343	6,202
R-Square	0.449	0.325	0.742	0.597

Notes: See Notes to Table 5. The “Predicted Net Outflow” estimates describe the effect of a one percentage point increase in net outflows for districts that are predicted to have negative net inflows. The “Predicted Net Inflow” estimates describe the effect of a one percentage point increase in net inflows for districts that are predicted to have positive net inflows. These regression models also control for an indicator variable equal to one if the district has positive predicted net inflows, but the estimated coefficient on this control variable is never statistically significant at the .10 level.

Appendix: Full Regression Results for Regressions Presented in the First Column of Table 5

	A. All Districts Continental U.S. w/ State F.E.		*B. Districts in Metropolitan Areas w/ State F.E.*		*C. Districts in Metropolitan Areas w/ Metro-Area F.E.*	
	Coefficient	SE	Coefficient	SE	Coefficient	SE
Predicted net inflows	-0.234	0.602	-1.382	0.913	-1.665*	0.967
# of Households in 1990 (ten thousands)						
Alone	-0.0145	0.0429	-0.0139	0.0448	-0.00610	0.049
Interacted with Any Inter-district Policy	-0.945***	0.176	-1.26***	0.188	-1.30***	0.191
Interacted with Voluntary Receiving	0.813***	0.184	1.04***	0.194	1.11***	0.195
Mean Household Income in 1990 ($ thousands)						
Alone	-0.122**	0.056	-0.318***	0.071	-0.307***	0.074
Interacted with Any Inter-district Policy	0.348***	0.096	0.03	1.04	-0.17	1.05
Interacted with Voluntary Receiving	-0.236***	0.097	0.0944	0.143	0.213	0.159
Percent Non-White in 1990						
Alone	-9.83***	1.75	-8.64***	2.35	-14.3***	2.75
Interacted with Any Inter-district Policy	-5.15***	2.52	-20.5***	4.3	-19.1***	4.85
Interacted with Voluntary Receiving	9.46***	2.39	25.7***	4.21	15.5***	4.83
Change in Mean Household Income, 1980 to 1990 ($ thousands)						
Alone	.543***	0.087	.713	1.09	.604	1.17
Interacted with Any Inter-district Policy	-0.097	0.161	.513**	0.234	.789***	0.252
Interacted with Voluntary Receiving	-.145	0.156	-.707***	0.233	-.812***	0.254
Change in the Percent Nonwhite, 1980 to 1990						
Alone	-34.1***	5.74	-38.3***	6.74	-35.9***	7.0
Interacted with Any Inter-district Policy	18.6*	9.51	18.9	14.8	7.65	15.7
Interacted with Voluntary Receiving	-2.29	9.34	-2.51	14.9	20.4	15.9
Percentage Change in the # of Households, 1980 to 1990						
Alone	28.7***	1.12	32.6***	1.45	31.1***	1.51
Interacted with Any Inter-district Policy	-8.72***	1.68	-15.9***	2.42	-14.9***	2.5
Interacted with Voluntary Receiving	10.4***	1.91	9.90***	2.71	4.97*	2.81
District's Mean Household Income Minus Surrounding Neighborhoods' Mean Household Income (Percent Difference)						
Linear	-1.88***	0.803	3.38***	1.05	5.11***	1.17
Squared	-11.3***	2.1	-13.8***	2.44	-13.1***	2.44
Cubed	3.11**	1.48	3.61**	1.68	2.91*	1.68
District's Percent White Minus Surrounding Neighborhoods' Percent White (Percentage Point Difference)	-3.35***	1.42	-5.58***	1.86	-8.71***	2.07
Observations	11,769		6,264		6,264	
R-Square	0.337		0.394		0.449	

Note: See Notes to Table 5.