**MOBILITY, HOUSING MARKETS, AND SCHOOLS:**
**ESTIMATING THE EFFECTS OF INTER-DISTRICT CHOICE PROGRAMS**

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*Abstract:* In theoretical models of residential sorting, a household's location decision is closely linked to its demand for local public services, such as schooling. Since school choice programs weaken the link between residential location and schooling options, they have the potential to affect both property values and residential location choices. Results derived from computable general equilibrium models suggest these effects could be large, but there is limited empirical evidence concerning whether they actually occur. This paper develops and tests predictions concerning the impact of inter-district choice programs on housing values and residential location decisions. Our empirical results strongly confirm our theoretical predictions and the findings of the computable general equilibrium literature: after their states adopt inter-district choice programs, districts with desirable nearby, out-of-district schooling options experience relatively large increases in housing values, residential income, and population density.

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## 1. Introduction

In theoretical models of residential sorting, a household's location decision is closely tied to its demand for local public services, such as schooling. School choice programs reduce the link between residential location and schooling services. As a result, these 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 find large potential effects of school choice programs on housing markets and residential sorting.[1] Nechyba (2000, 2003a, 2003b) and Ferreyra (2007) use structural and computable general equilibrium (CGE) models to examine the impact of private school voucher programs on housing values and residential income stratification. Their results suggest that voucher programs would reduce income and housing value disparities across school districts. Epple and Romano (2003) use a CGE model to examine the impact of 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).

In this paper, we provide the first direct empirical evidence of whether these predicted effects occur.[2] 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 (Holme and Wells, 2008).

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[1] A few studies have also examined the effect of school finance reform on housing values and community composition. Dee (2000) examines whether additional school resources generated by school finance reforms were capitalized into housing values. Epple and Ferreyra (2008) and Roy (2008) examine the impact of school finance reform in Michigan on housing values and community composition.

[2] Two prior studies provide indirect evidence concerning how school choice programs affect housing values. Using vote returns from voucher initiatives in California, Brunner, Sonstelie, and Thayer (2001) and Brunner and Sonstelie (2003) 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. 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.

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. The model synthesizes previous work by Epple and Romano (2003), Calabrese et al. (2006), and Banzhaf and Walsh (2008). The model predicts that inter-district transfer opportunities will cause housing values and population density to fall in initially high-quality districts and rise in initially low-quality districts.

To test those predictions, we exploit the fact that between 1989 and 1998, twelve states adopted inter-district choice programs that required school districts to receive incoming transfer students. We use data concerning student transfer flows during the year 2000 for more than 1,700 districts in these twelve states that adopted "mandatory" inter-district choice. We assess the impact of inter-district transfers on school district demographics and house prices by regressing district-level changes in housing values, income, and population density between 1989 and 1999 on inter-district transfer flows in 1999, while controlling for state and/or labor market-area fixed effects, baseline values of the dependent variables, changes from 1979 to 1989 in the dependent variables, and an assortment of baseline district characteristics from 1989.

Because students might transfer into districts where house prices are already increasing, least squares estimates of transfer effects on housing prices and other outcomes may be biased towards zero. We therefore instrument for transfers using baseline characteristics of neighboring school districts. In the absence of a major event such as the adoption of an inter-district choice program, neighboring districts' characteristics may be unrelated to breaks in trends in districts' own house prices and demographics. To confirm this, we test the validity of our instrumental variables in several ways. First, we conduct a series of falsification tests by predicting counterfactual transfer flows for similar districts located in states that did not adopt inter-district choice programs during the 1990's. Second, we conduct additional falsification tests by examining how predicted flows are related to trends in the dependent variables preceding the policy change (i.e., during the 1980's) for districts in the adopting states. Finally, we confirm that our results are robust to the inclusion of additional control variables and are robust to changes in the construction of the instrumental variables.

Our empirical results strongly support our theoretical predictions and the findings of the computable general equilibrium literature. Housing values and residential income increased in districts with desirable nearby transfer opportunities. The effects on house values are particularly large for school districts located in metropolitan areas. Districts with desirable nearby transfer opportunities also experienced relatively large growth in the number of households in residence after the adoption of inter-district choice. These findings reveal that even school choice programs with moderate participation rates can have economically meaningful effects on house values and residential sorting.

In addition to providing empirical evidence in support of the previous theoretical literature concerning the effects of expanded school choice programs, our results provide evidence consistent with Tiebout's (1956) 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.

## 2. 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 Nechyba (2000, 2003), 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, $f(y)$. Households derive utility from the perceived 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 such that $q_j = f(\bar{\theta}_j)$, where $\bar{\theta}_j$ is the average income of households whose children attend school in district *j*.[3] Housing demand is assumed to be independent of school quality and is given by $h(p, y)$, where $p$ is the price of a unit of housing. A household's indirect utility function is then:

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

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 $V_y > 0$, $V_q > 0$, and $V_p < 0$.

To characterize equilibrium, we assume household preferences satisfy the standard single crossing property.[4] 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 $\bar{y}_2 > \bar{y}_1$ and $p_2 > p_1$, where $\bar{y}_1$ and $\bar{y}_2$ are the average incomes of residents of districts 1 and 2 respectively. The single crossing 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.

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[3] 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.

[4] 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*.

Now consider how the introduction of an inter-district choice program affects the stratified equilibrium discussed above. We focus on the case where schools within districts face capacity constraints, implying that inter-district choice options are constrained by the availability of transfer space in nearby out-of-district schools.[5] Specifically, we assume districts guarantee admission to all students living within the boundaries of the district. District capacity is such that districts may serve these residential students as well as some fraction, $\alpha$, of the students living in the other district, where $\alpha \in (0,1)$. Districts cannot deny admission to any particular student and must select students randomly by lottery if there are more applicants than available spaces. Under these assumptions, the perceived school quality available to households residing in district 1 is now[6]: $E[q_1] = (1-\alpha) \cdot q_1 + \alpha \cdot q_2$.

Note that the introduction of inter-district choice increases the perceived quality of schooling available in district 1 (the low-quality district). Consequently, inter-district choice creates an incentive for the boundary household living in district 2 to move to district 1 to take advantage of lower housing prices. This in turn leads to an increase in the population density of district 1 and a decline in the population density 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. Since the households that choose to move to district 1 all have higher incomes than the set of households that currently reside in that district, 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. We thus have the following predictions concerning changes in population density, income and housing values following the adoption of an inter-district choice program: if $q_2 > q_1$ prior to the introduction of inter-district choice, then

[5] Epple and Romano (2003) examine the impact of frictionless inter- and intra-district open enrollment policies on residential segregation and housing values. Frictionless school choice is defined as the case where households face no transportation costs, schools face no capacity constraints, and any student that wishes to attend a particular school is guaranteed admittance. They demonstrate that introducing frictionless school choice leads to an equalization of housing values and in most cases a reduction in residential income inequality across neighborhoods.

[6] Recall that $q_1$ and $q_2$ are functions of the average income of households whose children attend school in districts 1 and 2, respectively. After the implementation of school choice, $q_2$ is now a weighted average of the average incomes in districts 1 and 2 since some fraction, $\alpha$, of students in district 1 now attend school in district 2. Since the households in district 1 who transfer their children to district 2 all have lower incomes than the set of households residing in district 2, school quality in district 2 declines after the implementation of school choice.

$$\Delta N_1 > 0, \quad \Delta N_2 < 0, \quad \Delta p_1 > 0, \quad \Delta p_2 < 0, \quad \Delta \bar{y}_1 > 0, \quad \Delta \bar{y}_2 > 0$$ [7],

where $N_1$ and $N_2$ are the population densities of districts 1 and 2 respectively.

Several considerations are worth noting before applying these theoretical predictions to our empirical tests. First, we have assumed that households differ only along a single dimension, namely income. While that assumption allowed us to plainly illustrate the likely effects of expanded school choice on housing values and community composition, it also led us to restrict our attention to a case where willingness to pay for school quality and perceived school quality depended solely on household income. Of course, willingness to pay and perceived school quality most likely depend on several factors other than income. For example, Bayer, Ferreira, and McMillan (2004, 2007) develop and estimate an equilibrium model of residential sorting in which households have preferences defined over school quality and the socio-demographic characteristics of their neighbors. Their results suggest that heterogeneous preferences for school quality and neighbors leads to substantial stratification along racial and socioeconomic lines, with white, highly-educated, and high-income households clustering in neighborhoods that contain the highest-quality schools. Similarly, Clapp, Nanda, and Ross (2008), Downes and Zabel (2002) and Kain, Staiger and Riegg (2006) find that housing prices are lower near schools with high concentrations of minority students. The results of these studies suggest that households use easily observable characteristics, such as race, when comparing public school quality and that perceived school quality is lower in districts with high concentrations of minority students. Schneider and Buckley (2002) reach a similar conclusion based on the internet search patterns of parents participating in the Washington DC school choice program—parents were more likely to browse schools' racial composition than any other school characteristic, including test scores. Their results also suggest that most parents seek out schools with lower percentages of black students.

Second, our conceptual framework also assumes that households do not face significant transportation costs associated with sending a child to a nonresidential school. In reality, some

[7] 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. They are also similar to the predictions derived by Nechyba (2000, 2003) who examines how the introduction of private school vouchers into a previously residentially zoned school system affects housing values and community composition. Nechyba (2000, 2003), however, assumes a fixed housing stock and thus does not consider the effect of expanded school choice on population density.

households are likely to face nontrivial transportation costs if they choose to participate in an inter-district choice program. None of the 12 states that implemented mandatory inter-district school choice programs fully funded transportation for students exercising choice. Parents are typically responsible for transporting their children into the receiving school district, though some states allow district buses to cross boundary lines at the discretion of the school district. Using survey data from Washington DC and Denver, Teske et. al. (2009) find that, when faced with greater transportation costs, lower income families were significantly less likely to take advantage of choice opportunities than higher income families. Epple and Romano (2003) address the issue of transportation costs within the context of a theoretical model of intra-district choice. Their results suggest that transportation costs may lead to heterogeneous changes in the quality of schooling available to households residing in low quality districts—particularly if relatively high income households in these districts are the ones most likely to take advantage of choice opportunities. Due to the exodus of students from relatively wealthy families, the quality of schooling declines for students from the lowest income households because of negative peer effects. As long as transportation costs are not excessively high, the theoretical model of Epple and Romano (2003) still predicts that the introduction of school choice leads to a reduction in *residential* disparities in income and housing values across communities. However, these effects will be attenuated as transportation costs rise. Given that different assumptions lead to different predictions, this paper makes an important contribution by taking these predictions to the data to be tested.

Third, we have assumed that inter-district choice programs affect school quality solely through peer effects associated with changes in the composition of enrolled students.[8] It is possible that inter-district choice programs could also alter districts' productivity by inducing competitive responses, changing school resources, or changing parental perceptions of school quality. For example, Bayer, Ferreira and McMillan (2004) find evidence of strong social multiplier effects whereby initial changes in school quality lead to re-sorting on the basis of income, race and educational attainment that reinforce the initial effect that changes in school

[8] A growing number of studies explore the impact of various types of school choice programs on student sorting across schools. 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 (2010) 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.

quality have on housing values and community composition. Our empirical estimates will pick up effects such as these and any other type of effect from the adoption of inter-district choice. These estimates are thus ideal for predicting how expanding school choice affects various types of school districts, but less useful for precisely identifying household demand for school quality given that changes in access across school districts may not be the only parameter in motion.

Finally, the example above considered two nearby districts, whereas districts are typically surrounded by several districts of varying quality. Our empirical framework accounts for a continuum of district quality by utilizing a continuous measure of the net transferring that might occur into and out of the same district. We do not expect strong effects for districts that ultimately export roughly the same number of students as they import.

## 3. Data and Background of Inter-District Choice Policies

We test the model's predictions by examining the impact of student participation in mandatory statewide inter-district choice programs.[9] By the spring of 1998, twelve states had adopted inter-district choice programs that mandated district participation, forcing districts to admit incoming transfer students residing in other districts. The first of these mandatory inter-district choice programs began in 1989. We focus on these twelve states so that we can examine how the adoption of inter-district choice policies affected housing values and district composition between the baseline year of 1989 (based on the 1990 U.S. Census) and the year 1999 (based on the 2000 U.S. Census).

We combine district-level Census data with all available district-level information on inter-district choice participation. Our key independent variable of interest is a district's net outgoing transfer flow rate, measured as the number of residential students attending schools in other districts minus the number of students residing in other districts who transfer into the local public schools, divided by the total number of residential public school students. We examine net outgoing transfer flows for the fall of 1999.[10] Where available, we use administrative data on

[9] To determine which states adopted mandatory inter-district choice policies, we first examined state legislation concerning inter-district open enrollment policies using LexisNexis and state archives. We then contacted administrators working in their respective state departments of education for further information. We also consulted Appendix B in Bierlein et al. (1993) for policy information for early-adopting states.

[10] Our use of transfer flows in 1999, rather than the change in transfer flows between 1989 and 1999, is due to the unavailability of pre-program transfer flow data. Thus, we have implicitly assumed that transfer flows are zero prior to the implementation of a formal inter-district choice policy. To examine the validity of that assumption, we used the 1993-94 wave of the SASS to calculate transfer flows in the three states in our sample that adopted mandatory

transfer flows from states' departments of education—Colorado, Iowa, Minnesota, and Wisconsin provided statewide administrative data for a total of 1,312 school districts. For the other eight adopting states, we use transfer flow data from the restricted-use version of the NCES' 1999-2000 Schools and Staffing Survey (SASS). The restricted-use version provides district identification numbers allowing us to merge districts' net transfer flows with our other district-level data. The SASS contains a representative sample covering roughly 40 percent of the school districts in these eight states, so our combined sample includes transfer flow information for roughly 65 percent of all districts in the twelve adopting states.[11]

We combine these data with demographic and geographic data from the U.S. Censuses. We use district-level data from 1979, 1989, and 1999 concerning mean owner-occupied house values, mean household income, the fraction of the population who is non-white, the number of households, and the number of households with children.[12] We also use district-level data from the 1990 Census to create additional district-level control variables: (1) current expenditures per pupil in 1989; (2) the percent urban population; (3) the fraction of the population age 65 or older; and the fraction of heads of households who (4) did not possess a high school diploma; (5) possessed a Bachelor's degree; (6) owned their residence.

Table 1 lists the 12 states that adopted mandatory inter-district choice programs by 1998, along with their year of adoption and the percent of enrollments that were transfer students in the average district. The transfer student enrollment share ranges from 0.7% for the average district in Utah to 9.8% for the average district in Colorado. In the average district across the 12 states, almost 5 percent of students were transfer students.

While the details of the inter-district choice programs enacted in each of the 12 states listed in Table 1 vary to some degree, there are several common features. First, all 12 states

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inter-district choice policies after 1994 (i.e. Delaware, South Dakota and Wisconsin). In these three states only 8% of districts reported serving *any* incoming transfer students in 1994. In contrast, in the 1999-2000 wave of the SASS, 94% of districts in these three states reported serving inter-district transfer students. We also calculated transfer flows in 1999-2000 for the five states that adopted inter-district choice policies after 2000. In those five states, the average district's student body was composed of less than 0.3% inter-district transfer students in 1999-2000. Thus our assumption that inter-district transfer flows were negligible prior to the adoption of a formal inter-district choice policy seems reasonable.

[11] We set transfer rates to missing for eight SASS districts and twenty-four other districts that reported suspiciously high positive or negative net inflows equivalent to more than 40% of their residential public school student population. The mean net incoming transfer rate is positive in our sample because 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 underreporting the number of outgoing transfer students.

[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).

prohibit districts from selectively admitting students based on criteria such as gender or past student achievement. Second, state statutes provide a limited number of reasons for why a district may refuse to accept a transfer student. As noted by Carlson et. al. (2011), the most common reason is capacity constraints. For example, Minnesota allows district to reject transfer applications based on the capacity of a program, class, or school building. Most states also allow districts to deny transfer requests based on prior expulsions, suspensions, or disciplinary issues such as criminal activity or violent behavior. In the event that the number of eligible transfer applications exceeds the number of seats available in a district, states typically require districts to admit students using a random lottery.

In terms of transportation, as noted previously, none of the 12 states that implemented mandatory inter-district school choice programs fully fund transportation for students exercising choice. In three states, Delaware, Iowa and Minnesota, low income families (typically defined as eligible for free or reduced price lunch or income below the federal poverty level) are either reimbursed for inter-district transportation costs or provided transportation to the receiving district. In other states, parents are typically responsible for transporting their children to the receiving school district, though some states allow district buses to cross boundary lines at the discretion of the school district.

In terms of the fiscal impacts, in 8 of the 12 states with mandatory inter-district choice programs the receiving district counts a transfer student in their average daily attendance (enrollment) and receives full state aid based on that attendance. In these eight states, the sending district loses the state aid associated with a transfer student and the receiving districts gains the state aid (based on the per pupil allocation for the receiving district) associated with the student. In the remaining states, state aid follows students to the receiving district based on a fixed dollar amount. For example, in South Dakota a sending district in 1998 would lose $3,350 in state aid for every student that transferred out of the district while a receiving district would gain $3,350 for every student transferring into the district.

## 4. Empirical Methods

Our empirical analyses use an instrumental variables model to examine the effects of transfer flows on household sorting and house values. The endogenous nature of district-level transfer flows could bias estimated effects of school choice opportunities towards zero in OLS models. To create our instrumental variables, we use geographic information concerning which school districts share a border. Although some states allow students to transfer to non-contiguous districts, the vast majority of transferring occurs between contiguous districts.[13] We use two types of instrumental variables: (1) the number of contiguous school districts, and (2) baseline demographic differences between districts and contiguous districts. The number of contiguous districts should predict outgoing transfer flows because more variety in nearby district options implies more choices and also a greater chance that at least one nearby district is not at capacity. Baseline demographic differences should predict net transfer flows because parents often sort their children in schools based on race and income. We use demographic differences measured in the 1990 Census to predict district-level student transfer flows during the 1999-2000 school year.

Define $N_{i,1989}^{neighbors}$ as the number of contiguous neighboring school districts for district *i* in 1989. Define $Racial\ Difference_{i,1989}$ as the difference between a district's own fraction of white (and non-Hispanic) residents and the average fraction of white residents in contiguous school districts. This variable should predict transfer flows, as previous studies have found that parents shopping for schools are concerned with racial composition (Schneider and Buckley 2002; Koedel et. al. 2009; Welsch et. al. 2010). While we focus on racial differences, we also present the results from models including differences in residential income as an instrumental variable.

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. Household location decisions and capitalization 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. We can thus predict transfer flows without estimating structural equations for the supply and demand for transfer spaces. Define $T_{is,2000}$ as the net outgoing transfer rate for district $i$ in state $s$ during year 1999, i.e., the number of outgoing transfer students minus the number of incoming transfer students, divided by the total number of residential public school students. We predict net

[13] Examining an intra-district choice program in Charlotte-Mecklenburg County, North Carolina, Hastings and Weinstein (2008) find that student participation rates are sensitive to features of schools located within a short drive of students' homes. Cullen et al. (2005) find that student participation rates in an intra-district high school choice program in Chicago are sensitive to the distance between students' homes and alternative schooling options.

transfer flows, rather than separately predicting inflows and outflows, because there are compelling reasons why variables affecting a flow in one direction could also affect the flow in the other direction. Fortunately, these effects operate in opposite directions, resulting in strong effects on net flows.

Define $Y_{is,t}$ as an outcome of interest during year *t* for school district *i* in state *s*. For the sample of districts in states mandating inter-district open enrollment before 1998, we estimate the following equations using two stage least squares (2SLS):

$$T_{is,1999} = Racial\ Difference_{i,1989}\phi_1 + N_{i,1989}^{neighbors}\phi_2 + X_{is,1989}\phi_3 + (Z_{is,1989} - Z_{is,1979})\phi_4 + \lambda_s + \varepsilon_{is} \quad (7)$$

$$(Y_{is,1999} - Y_{is,1989}) = \beta_1 T_{is,1999} + X_{is,1989}\beta_2 + (Z_{1is,1989} - Z_{1is,1979})\beta_3 + \delta_s + e_{is} \quad (8)$$

where $X_{is,1989}$ is a vector of 1989 demographic variables and per pupil spending for district *i*, $(Z_{is,1989} - Z_{is,1979})$ is a vector capturing previous district-level trends during the 1980's (including trends for the same variables used as second stage dependent variables), $\lambda_s$ and $\delta_s$ capture state fixed effects, and $\varepsilon_{is}$ and $e_{is}$ are random disturbance terms. For models using changes in mean house values as the second stage dependent variable, we also control for mean house values in 1989 and the change in mean house values during the 1980’s.[14]

We estimate equations (7) and (8) for 1,699 districts for which we have net transfer flow information from either state administrative data or the SASS. We then expand on these baseline 2SLS models by estimating two-sample instrumental variables (TSIV) models where the second stage equations include all 2,617 districts in these states with non-missing Census data. This larger sample enables us to test for heterogeneous effects and to estimate models with additional control variables.

Table 2 contains the full list of variables included in the $X_{is,1989}$ and $Z_{is,1989}$ vectors, as well as these variables' mean and standard deviation. Note that our $X_{1is,1989}$ vector in equations 7 and 8 also includes squared terms for the first four control variables in the 1989 levels list in Table 2—i.e., household income, number of households, proportion of households with children, and the

[14] Including these extra control variables slightly reduces the power of the instrumental variables in these house value models compared to the models with other dependent variables. The partial F-statistic for the instrumental variables is 12.6 for the house value models in Table 3 (compared to 13.8 for the other models in Table 3).

percent of residents who are nonwhite. The first two columns in Table 2 report summary statistics for the full sample of U.S. school districts located in states that adopted mandatory inter-district choice programs by 1998 (i.e. the second stage sample of the TSIV models); the third and fourth columns report summary statistics for the subsample of these districts for which we have information on net outgoing transfer flows (i.e. the first stage sample of the TSIV models). 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 0.25% or bottom 0.25% of the overall distribution.

## 5. **Results**

### ***5.1 Estimates of the Effects of Inter-District Choice Opportunities***

Table 3 reports estimates of the impact of net transfer outflows on changes in districts' population density, mean resident income, and housing values. In the interest of brevity we report only the estimated coefficient on the net transfer outflow variable but note that all the specifications reported in Table 3 and all subsequent tables include the full set of control variables listed in Table 2. We estimate equation (8) using OLS and equations (7) and (8) using either 2SLS or TSIV.

Panel A of Table 3 reports the OLS results. The estimated effects on housing values and residential income are small and statistically insignificant for these OLS models. 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. Consistent with that prediction, the estimated coefficient in column 3 of Panel A is positive and statistically significant. The point estimate implies that a one percentage point increase in net transfer outflows is associated with a 0.30 percent increase in the number of households residing in the district.

Panels B and C of Table 3 report the 2SLS and TSIV estimates, respectively.[15] The partial F-statistics for the instrumental variables is 13.8 in these models, suggesting that the instrumental variables have respectable power for identifying the effects of choice opportunities. The standard deviation of the predicted net transfer flow is 1.81. Table A.1 in the Appendix

[15] Standard errors for the TSIV estimates reported in Table 3 and all subsequent tables were constructed using the method suggested by Murphy and Topel (1985) and Inoue and Solon (2010).

displays the full set of first-stage and second-stage estimates for the TSIV models used for Panel C of Table 3. The first two rows of column 1 of Table A.1 report the estimated coefficients and standard errors for our instrumental variables. The estimated coefficients on the instruments are consistent with parents preferring to send their children to non-residential districts with fewer minority students and more frequently transferring their children when there are more surrounding districts to choose from. Both of the estimated coefficients are statistically significant at the .01 level. The two instruments are jointly significant at the .0005 level, and the coefficient of the racial difference instrument is statistically significant at the .002 level.[16] Racial differences are very strong predictors of incoming transfer students. Holding a district's own racial composition constant, a ten percentage point increase in the share of residents who are white in neighboring communities increases the predicted net outgoing transfer rate by 0.45 of a standard deviation (0.81 percentage points).

Turning to the second stage estimates reported in Panels B and C of Table 3, the results are striking. All of the reported 2SLS point estimates on the net outflow variable are positive and statistically significant at the 1 percent level. These estimates suggest that the endogenous nature of district-level transfer flows may bias the OLS estimates towards zero. Expanded inter-district choice opportunities increase housing values in initially "low-quality" districts (districts with net outflows) and decreases housing values in initially "high-quality" districts (district with net inflows). The estimates in Panels B and C respectively suggest that a one percentage point increase in net transfer outflows increases mean house values by $1,955 or $1,853 (column 1). A one percentage point increase in net transfer outflows also increases a district's mean resident household income—with point estimates of $745 and $629 (column 2). These empirical results are consistent with the computable general equilibrium results of Nechyba (2000, 2003a, 2003b) and Ferreyra (2007) for change in districts' housing values and residents' income due to the introduction of school vouchers into a previously residentially zoned school system. The results in Panels B and C also suggest that the OLS estimate understates the effect of adoption of inter-district choice programs on population density across school districts. A one percentage point

[16] We also conducted Hansen's J over-identification tests for the 2SLS models and the results were consistent with valid instrumental variables: p-values ranging from .75 to .95 for rejecting the null hypothesis of exogenous instrumental variables.

increase in predicted net transfer outflows is associated with an increase in the number of households of 2.89 or 2.46 percent. In additional 2SLS and TSIV models, we find even larger, statistically significant effects of predicted net transfer outflows on the number of households *with school-aged children* residing in the district. This confirms that families are responding to the policy change.[17]

### *5.2 Falsification Tests*

While the results in Table 3 provide compelling evidence that inter-district transfer opportunities affect housing values and residential location choices, one might be concerned about the validity of the instrumental variables—namely whether characteristics of neighboring school districts are related to future changes in house prices and demographics for reasons other than the adoption of school choice programs. For example, a greater fraction of white households living in one district may increase the probability that wealthy households eventually move into an adjacent poorer district, leading to an associated increase in home values and household income in the poorer district that is unrelated to the adoption of inter-district choice. We investigate this issue in two ways. First, we conduct several falsification tests. Then, as described in the next subsection, we conduct several robustness checks.

The idea behind the falsification tests is simple: if the TSIV results are truly causal, then they should only hold during the relevant time period and only for districts in states that adopted inter-district choice programs. Table 4 displays our falsification estimates and the t-statistics for rejecting equality of our actual TSIV estimates and these falsification estimates. Panels A through E present results for five different types of falsification models.[18] Panel A displays results restricting the second-stage sample to all districts located in states that did not adopt any type of inter-district policy during the 1990's. To make our comparison group as similar as

[17] The estimated effects on households with school-aged children are more than 50 percent greater than the corresponding estimates for households without school-aged children, but these differences are not statistically significant at conventional levels (t-statistic of 0.99 for TSIV and 1.32 for 2SLS). The lack of a significant difference may be due to a variety of factors—e.g., limited precision of these estimates; responses from households who expect to have school-aged children soon; responses from other households due to social multiplier effects, changes in local property tax bases, or changes in expectations concerning property value appreciation.

[18] To implement our falsification tests we used the first-stage coefficient estimates reported in Table A1 of the Appendix to construct predicted counterfactual net transfer flows for each falsification sample. The falsification models presented in panel E of Table 4 are slight exceptions, since they must use only 1979 values for the independent variables. For the models in panel E, the F-statistic for the exclusion restriction for the two instrumental variables equals 11.68 for column 1 and 10.88 for columns 2 and 3.

possible to our adopting-state sample, the models in Panel B weight the districts in non-adopting states based on their similarity with districts in the adopting-state sample.[19] Alternatively, the models in Panel C use only the non-adopting states that share a border with at least one adopting state. The models in Panel D further restrict this sample to districts within one hundred miles of this type of state border.

We first note that all of the point estimates reported in Table 4 are much smaller in magnitude (and sometimes opposite in sign) than the corresponding point estimates reported in Panel C of Table 3. Furthermore, the t-statistics for rejecting the equality of our actual TSIV estimates and these falsification estimates are greater than 1.7 for all falsification tests and greater than 2 for all but one, providing reassuring evidence that the TSIV estimates in Table 3 are truly capturing the impact of the expansion of school choice. Also, in spite of smaller standard errors than those in the TSIV models in Table 3, only one out of 15 of the falsification estimates in Table 4 is statistically significant at the .10 level. Overall, there is little reason to suspect a large bias in the actual TSIV estimate due to a secular relationship between the instrumental variables and changes in residential income during the 1990's.

The final falsification tests, presented in Panel E, is different from the others; it uses districts from adopting states but uses 1979 values for the explanatory variables and examines changes in the dependent variables during the 1980's instead of the 1990's (see footnote 17). Predicted transfer flows for programs adopted after 1990 could not have affected changes in house prices and demographics during the 1980's. The small and statistically insignificant point estimates in Panel E confirm that districts' predicted transfer flows were not coincidentally related to pre-policy changes in outcomes.

[19] In particular, we first estimate a probit model including districts from both the adopting-state sample and comparison sample, with the dependent variable equal to one if the district is in an adopting state and with the same $X_{1is,1990}$ vector used above (1989 district-level demographic variables) as the independent variables. We then weight the non-adopting state districts in the falsification regressions by their predicted probabilities from this probit model. Thus, districts located in non-adopting states that are similar to districts located in adopting states receive more weight. On average, the comparison districts are wealthier and less rural than districts in the adopting-state sample. These statistically significant differences are partly due to the disproportionate number of near-state-border districts in the comparison group; nationwide, near-state-border districts tend to be wealthier and less rural than interior districts.

### *5.3 Robustness of the TSIV Estimates*

Table 5 presents results based on several alternative specifications designed to examine the robustness of our results. For comparison purposes, the first row of Table 5 displays the baseline TSIV results from Panel C of Table 3. The remaining rows display second stage estimates from alternative specifications. The column immediately preceding the second stage estimates displays the F-statistics for the exclusion restriction for the instrumental variables. The first issue we investigate is whether our results are sensitive to the inclusion of both administrative data on transfer flows and survey data on transfer flows from the SASS. To examine that issue, Panel B presents estimates based on specifications where we restrict the sample to include only those districts located in states for which we have administrative data on transfer rates. Using state administrative data on transferring, rather than data from SASS surveys, may increase the accuracy of the transfer rate variable but obviously reduces the sample size. Restricting the sample to include only districts with administrative data has little effect on our results. All of the estimates remain statistically significant and are similar in magnitude to our baseline results.

Since the predicted effects of the adoption of choice should still hold when examining differences across districts within the same local geographic region, the second issue we investigate is whether our results are robust to the inclusion of commuting zone fixed effects -- dummy variables for whether the district is located in one of 191 commuting zones in the 12 states. These commuting zones were developed by the Economic Research Service of the United States Department of Agriculture and are designed to be spatial measures of local labor markets—geographical areas composed of counties with strong commuting ties. The main benefit of using commuting zones, rather than metropolitan statistical areas (MSAs) or core based statistical areas (CBSAs), is that commuting zones are defined for the entire United States. Thus, they allow us to include rural areas that lie outside of traditional metropolitan areas.[20] The inclusion of labor market (commuting zone) fixed effects implies that we are now identifying the impact of school choice opportunities solely from within labor market variation in initial demographic differences between districts and their surrounding neighbors. Parameter estimates

[20] Commuting zones located within metropolitan areas typically contain the same counties as core based statistical areas. Thus, the only real difference between CBSAs and commuting zones is that commuting zones are more comprehensive since they also include rural counties. We use the year 2000 definitions of commuting zones in our analysis.

based on specifications that include commuting zone fixed effects are reported in Panel C of Table 5. Including commuting zone fixed effects does not substantively affect our results: the estimates are slightly smaller in magnitude than our baseline estimates, but these differences are not statistically significant.

In Panels D through G we report results based on specifications that utilize alternative sets of instrumental variables. Panel D displays results from models that add the difference between a district's own mean household income and the average household income in contiguous school districts as a third instrumental variable. The first stage coefficient estimate on this third instrument is positive, as expected, but less statistically significant ($p$=.012) than the other two instrumental variables. The inclusion of this third instrument also lowers the F-statistic for the exclusion restrictions relative to our baseline specifications. Despite that fact, the resulting second stage estimates all remain statistically significant at the one percent level and are slightly larger in magnitude than the baseline estimates. Panel E of Table 5 reports estimates based on specifications that use only racial differences as the instrumental variable. Because initial differences in race are the strongest predictor of transfer flows, this instrumental variables specification has even higher power than our baseline specification with a partial F-statistics of 21.0. The resulting second stage estimates based on this specification continue to be very similar to the original estimates. Panel F shows results from models that return to using the original two instrumental variables, but uses lagged values from 1979 to construct the racial differences. This check may be important if the 1989 racial differences were correlated with measurement error in the 1989 levels of the dependent variables. The results, however, continue to be quite similar to our baseline results. In results not displayed here, we also confirmed that changing the functional form of the number of neighboring districts variable (e.g., adding a quadratic term) neither increases the power of the instruments nor substantively changes our second stage estimates.

Finally, Panel G of Table 5 displays results from models with an additional control variable that is similar to the racial difference instrument but measured at a wider geographic level. Using Census block group data, we constructed a new variable equal to the difference between a district's own percent of white residents and the percent of white residents in all households living outside the district within 30 miles of its border. Given that the contiguous districts are the most likely source of actual transferring behavior, racial differences with a slightly wider range of local communities should not be driving our estimates. The results in Panel G confirm that this is the case: our second stage estimates barely change if both the first and second stage models control for baseline racial differences between a district and households located within 30 miles of the district's border.

Table 6 reports estimates based on several additional specifications that examine whether there are important heterogeneous effects associated with inter-district choice opportunities. We begin by examining whether the effect of inter-district choice opportunities varies depending on whether a district is located inside or outside a metropolitan statistical area (MSA). We expect to find stronger effects of inter-district transfer opportunities in metropolitan areas. There may be greater capitalization of inter-district transfer opportunities into housing values in metropolitan areas where the supply of land is relatively inelastic.[21] The theoretical model developed in section 2 also assumes that inter-district transportation costs are low and that households can easily sort among districts—assumptions which are more likely to hold in metropolitan areas. Metropolitan areas typically contain more districts located in close proximity and, as noted by Figlio et al. (2004), "have much better potential for Tiebout sorting."

Panel A of Table 6 reports the parameter estimates for the predicted net outgoing transfer flow variable for districts located within a MSA; Panel B reports the same parameter estimates for districts located outside a MSA. As expected, the estimated effect of transferring opportunities on house values and district composition are much larger for the sample of districts located within a MSA. The t-statistics reported below Panel B indicate that the difference between the estimates reported in Panels A and B are statistically different from each other at the 10 percent level or better in all three specifications. For districts located within a MSA (Panel A), all of the estimates are statistically significant at the 5 percent level or better. In contrast for districts located outside a MSA (Panel B) none of the estimated coefficients are statistically significant and they are much smaller in magnitude than the corresponding estimates reported in Panel A.

In additional analyses, we tested whether these effects are even larger if we exclude central city districts. Our rationale for this specification is twofold. First, most of the states that adopted mandatory inter-district choice programs during the 1990's also permit intra-district

[21] See Hilber and Mayer (2009) for recent empirical evidence on land supply elasticity and potential capitalization effects related to local public schools.

choice (i.e. freedom to attend any school within a district). Since larger central city districts typically contain many schools within the district, the presence of intra-district choice might diminish the appeal of inter-district choice opportunities. Second, previous studies suggest that school quality is less likely to be fully capitalized into housing values in larger districts (Hoyt, 1999; Brasington, 2001). Once we exclude central cities from the sample, the estimated coefficient on predicted transfer flows in the house value specification slightly increases in magnitude from 2,460 to 2,804, the estimated effect on mean households income slightly increases from 663 to 706, and the estimated effect on population size barely changes. These results are consistent with smaller capitalization effects in central cities than their suburban counterparts, though the small number of central city districts in these states prevent us from drawing strong conclusions.

In panels C and D of Table 6, we split our sample based on districts' geographic size, measured in terms of their land areas.[22] Examining this type of heterogeneity is one way to explore the potential importance of transportation costs. Inter-district commutes will tend to be shorter, on average, in geographically smaller districts. We might therefore expect families living in smaller districts to place a relatively high value on their inter-district choice opportunities. Panel C presents results for districts whose geographic size is below the median; Panel D presents results for districts whose geographic size is above the median. The estimated effects of transfer opportunities are in fact larger for geographically small districts. The t-statistics reported below Panel D indicate that the differences between the estimates reported in Panels C and D are statistically different from each other in both the house value and household income specifications. For smaller districts (Panel C) all of the estimated coefficients are statistically significant at the .01 level.

## 6. Conclusions

Theoretical models of residential sorting suggest that the adoption or expansion of school choice programs can have significant effects on housing markets and residential sorting. In this paper, we provide the first direct empirical test of whether those predicted effects occur. Theory

[22] One might be concerned that splitting the sample based on the geographic size of a district essentially mimics the samples based on districts located within a MSA and districts located outside a MSA. However, 58% of district located outside a MSA are actually above the median in terms of geographic size so the two sets of samples are actually quite distinct.

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. Theory also predicts that housing values may rise in initially low-quality districts and decline in initially high-quality districts.

Our empirical analysis confirms that these effects are present even in a context in which rates of participation in a choice program are fairly low—with choice participants composing 4.8 percent of the average district's enrollments. This moderate expansion of public school choice causes non-trivial changes in households' location patterns 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 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's (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 10.9% increase in housing values and a 7.4% increase in mean household income 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 values between the highest- and lowest-wealth districts and a 1.1% decline in the ratio of household income in these districts. Our estimates of the effect of expanded inter-district choice opportunities on housing values and income reported in Panel A of Table 6 lie in between these authors' estimates of the general equilibrium effects associated with modest private school voucher programs. On average, inter-district choice would increase home values by 5.2% and mean income by 3.0% for metropolitan-area districts in the top third of the predicted net outflow distribution. Our population density estimate in Panel A of Table 6 also suggests that more than 3.2% of households in metropolitan areas relocate due to the adoption of inter-district public school choice.[23] 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.

More generally, these findings have important implications for the public economics literature. The widely-cited Tiebout (1956) model features residential sorting based on local governments' provision of excludable public goods. Previous studies have used structural and computable general equilibrium models to examine the potential importance of Tiebout (1956) sorting, or have provided indirect evidence on sorting by examining how excludable public goods are capitalized into housing values.[24] Our results provide direct empirical evidence that reducing the link between residential location and excludable public goods affects the distribution of households across communities.

[23] This 3.2% point estimate provides a lower bound based on the assumption that *all* residential moves in response to choice opportunities affect districts' population densities via property abandonment and new construction; in reality, residential moves should also consist of households moving into previously-occupied homes.

[24] Please see Oates (2006) and Epple (2008) for recent reviews of the Tiebout sorting literature.

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Table 1: States' Inter-District Open Enrollment Policies Adopted Prior to 1999

| State | Year passed | Percent of Enrollments Composed of Inter-district Transfer Students in the Average District during the 1999-2000 School Year |
|---|---|---|
| Arizona | 1994 | 4.5%* |
| Arkansas | 1989 | 1.3%* |
| Colorado | 1994 | 9.8% |
| Delaware | 1996 | 4.0%* |
| Iowa | 1989 | 5.5% |
| Minnesota | 1989** | 8.1% |
| Nebraska | 1989 | 7.7%* |
| Oklahoma | 1990 | 6.3%* |
| South Dakota | 1997 | 3.2%* |
| Utah | 1993 | 0.7%* |
| Washington | 1993 | 3.7%* |
| Wisconsin | 1997 | 1.3% |
| All 12 States Combined | | 4.8% |

*Notes to Table 1:* Policy information is based on state legislation that describes 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 a mandatory inter-district open enrollment policy prior to 1998. The following states adopted inter-district programs prior to 1998 but did not require districts to participate: California (adopted in 1994), Connecticut (1997), Idaho (1990), Kentucky (1992), Massachusetts (1991), Michigan (1996), Montana (1993), North Dakota (1993), Ohio (1993), Oregon (1991), Tennessee (1992), and Texas (1995).

* For these states, average percent of enrollments composed of inter-district transfer students is estimated based on districts sampled in the Schools and Staffing Survey; we use the Schools and Staffing Survey's cross-sectional sampling weights, (so that the samples are representative for each state), and we drop eight districts that reported incoming transfer students equal to more than half of their enrollments.

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

Table 2: Summary Statistics

| | Districts in Adopting States | | | |
|---|---|---|---|---|
| | TSIV Sample | | OLS and 2SLS Sample | |
| | N=2,613 | | N=1,702 | |
| | Mean | SD | Mean | SD |
| DEPENDENT VARIABLES | | | | |
| *Change between 1989 and 1999 in…* | | | | |
| Mean House Values | $45,502 | $30,721 | $49,274 | $30,808 |
| Mean Household Income | $16,533 | $6,042 | $17,273 | $5,899 |
| Households (Change as a %) | 17.20% | 23.12% | 17.04% | 21.51% |
| CONTROL VARIABLES | | | | |
| *Change between 1979 and 1989 in…* | | | | |
| Mean Household Income ($thousands) | 11.99 | 4.84 | 12.64 | 4.89 |
| Households (Change as a %) | 13.04% | 34.08% | 12.12% | 29.95% |
| Households with Children (Change as a %) | -8.08% | 29.99% | -9.53% | 26.73% |
| Percent Non-White | 1.64 | 3.88 | 1.29 | 3.21 |
| *1989 Levels* | | | | |
| Mean Household Income ($thousands) | 29.51 | 8.29 | 30.75 | 8.61 |
| # of Households (thousands) | 4.57 | 13.95 | 6.18 | 16.91 |
| Proportion of Hholds with Children | 0.39 | 0.07 | 0.38 | 0.07 |
| Percent Non-White | 9.99 | 15.68 | 8.41 | 14.68 |
| District Spending per Pupil | $4,720 | $1,286 | $4,803 | $1,128 |
| Proportion of Residents over age 64 | 0.15 | 0.06 | 0.15 | 0.05 |
| Proportion Urban | 0.25 | 0.36 | 0.31 | 0.38 |
| Proportion of Heads of Household who… | | | | |
| do not have a high school diploma | 0.27 | 0.11 | 0.25 | 0.09 |
| have a Bachelor's degree | 0.13 | 0.08 | 0.14 | 0.08 |
| own their home | 0.75 | 0.09 | 0.74 | 0.09 |

*Notes to Table 2:* The samples include districts in the 12 states mandating district participation in a statewide inter-district choice program prior to 1998 (see Table 1). The models examining 1989 to 1999 changes in mean house values also include 1989 mean house values and changes in mean house values between the 1980 and 1990 Censuses as control variables. For the TSIV sample, the mean (and standard deviation) of the 1989 mean house value variable is $54,801 ($29,179) and the changes in mean house values between the 1980 and 1990 Censuses is $13,999 ($15,224).

Table 3: OLS, 2SLS, and TSIV Estimates of the Effects of Inter-district Transfer Opportunities on Residential Sorting and Housing Values

| | Model | (1) | | (2) | | (3) | |
|---|---|---|---|---|---|---|---|
| | | Change in Mean House Values | | Change in Mean Household Income | | Percent Change in # of Households | |
| A. | OLS | -25 | | 3 | | 0.30 | *** |
| | | (71) | | (20) | | (.098) | |
| B. | 2SLS | 1955 | *** | 745 | *** | 2.89 | *** |
| | | (703) | | (205) | | (.683) | |
| C. | TSIV | 1853 | *** | 629 | *** | 2.46 | *** |
| | | (607) | | (171) | | (.857) | |

*Notes to Table 3:* Each estimated coefficient comes from a separate regression. The models used for columns 2 and 3 of Panels A and B have sample sizes of 1,699 school districts (1,696 districts for column 1). The models used for Panel C include 1,699 school districts in the first stage (1,696 for column 1) and 2,620 districts in the second stage (2,613 for column 1). Standard errors are in parentheses below each estimated coefficient. For the models in Panel C, we calculate efficient standard errors using the methodology suggested by Murphy and Topel (1985) and Inoue and Solon (2010). Please consult Table A.1 in the Appendix for first-stage coefficients for the independent variables and for the full set of second stage coefficients for Panel C above.

* indicates significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level

Table 4: Falsification Tests for the Effects of Inter-district Choice

| Model | (1) | | (2) | | (3) | |
|---|---|---|---|---|---|---|
| | Change in Mean House Values | | Change in Mean Household Income | | Percent Change in # of Households | |
| Based on Districts in… | Estimate | T-stat for falsif. test | Estimate | T-stat for falsif. test | Estimate | T-stat for falsif. test |
| A. ...Nonadopting States | 212<br>(281) | 2.5 | 209 **<br>(83) | 2.2 | 0.277<br>(.208) | 2.5 |
| B. ...Nonadopting States, Weighted by Similarity to Adopting State Districts | 105<br>(560) | 2.1 | 138<br>(141) | 2.2 | 0.179<br>(.344) | 2.5 |
| C. …Nonadopting States Contiguous with Adopting States | -23<br>(424) | 2.5 | 152<br>(149) | 2.1 | 0.450<br>(.47) | 2.1 |
| D. …Nonadopting States Contiguous with Adopting States and within 100 miles of the Relevant State Border | -183<br>(499) | 2.6 | 99<br>(173) | 2.2 | 0.413<br>(.549) | 2.0 |
| E. …Adopting States but Using Pre-adoption Changes in the Dependent Variables | 205<br>(289) | 2.5 | 129<br>(113) | 2.4 | 0.298<br>(.895) | 1.7 |

*Notes to Table 4:* Each Panel displays counterfactual estimated effects of school transfer opportunities on each dependent variable. Standard errors are in parentheses below each estimated coefficient and are calculated using the methodology suggested by Murphy and Topel (1985) and Inoue and Solon (2010). The models in Panels A through D use the same first stage estimates as the TSIV models in Table 3 to calculate counterfactual predicted transfer flows for out-of-sample districts. The model in Panel E uses twice lagged (1980) values of the instrumental variable to predict transfer flows for in-sample districts but uses dependent variables that measure pre-policy (1980's) changes in the dependent variables for the in-sample districts. The "t-statistics for falsification tests" are the t-statistics for rejecting a null hypothesis of equality between the falsification estimate and the corresponding estimate in Panel C of Table 3. The sample sizes of the second stage of the models are 5121, 5121, 1163, 783, and 2612 for Panels A through E respectively (slightly smaller for column 1).
* indicates significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level

Table 5: Robustness Checks for the Effects of Inter-district Choice

| | Model | F-Statistic for IV exclusion restriction | (1) Change in Mean House Values | | (2) Change in Mean Household Income | | (3) Percent Change in # of Households | |
|---|---|---|---|---|---|---|---|---|
| A. | BASELINE (TSIV with 2 IV's) | 13.8 | 1853 | *** | 629 | *** | 2.46 | *** |
| | | | (607) | | (171) | | (.857) | |
| B. | 2SLS with ADMINISTRATIVE DATA | 8.4 | 2106 | ** | 654 | *** | 1.91 | *** |
| | | | (829) | | (225) | | (.726) | |
| C. | TSIV controlling for COMMUTING ZONE FIXED EFFECTS | 14.6 | 1530 | *** | 545 | *** | 2.28 | ** |
| | | | (540) | | (170) | | (1.15) | |
| D. | TSIV with 3 IV's (ADDING INCOME DIFFERENCES) | 10.1 | 2467 | *** | 997 | *** | 3.09 | *** |
| | | | (691) | | (272) | | (.952) | |
| E. | TSIV with only 1 IV (RACIAL DIFFERENCES) | 21.0 | 1599 | ** | 654 | *** | 2.08 | *** |
| | | | (675) | | (206) | | (.796) | |
| F. | TSIV with 2 IV's using 1980 CENSUS to MEASURE RACIAL DIFFERENCES | 11.2 | 1903 | *** | 620 | *** | 2.73 | *** |
| | | | (680) | | (189) | | (1.035) | |
| G. | TSIV with 2 IV's ADDING CONTROLS FOR 30 MILE RACIAL DIFFERENCES | 15.1 | 1790 | *** | 584 | *** | 1.92 | *** |
| | | | (532) | | (160) | | (.536) | |

*Notes to Table 5:* For columns 2 and 3, the second stage sample is 2,620 for the models in Panels A, C, D and E; 1,087 for the models in Panel B; 2,671 for the models in Panel F; 2,572 for the models in Panel G. The samples are slightly smaller for the models used for column 1. Standard errors are in parentheses below each estimated coefficient and are calculated using the methodology suggested by Murphy and Topel (1985) and Inoue and Solon (2010).
* indicates significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level

Table 6: Heterogeneous Effects of Inter-district Choice

| | Model | (1) | | (2) | | (3) | |
|---|---|---|---|---|---|---|---|
| | | Change in Mean House Values | | Change in Mean Household Income | | Percent Change in # of Households | |
| A. | TSIV for Metropolitan Area Districts | 2460 | *** | 663 | *** | 3.19 | ** |
| | | (872) | | (225) | | (1.38) | |
| B. | TSIV for Non-Metropolitan Area Districts | 638 | | 163 | | 0.26 | |
| | | (602) | | (153) | | (0.53) | |
| | T-Stat for Difference in Panel A and Panel B Estimates | 1.72 | | 1.84 | | 1.98 | |
| C. | TSIV for Districts Below Median Size (land area) District | 2565 | *** | 967 | *** | 2.84 | *** |
| | | (837) | | (282) | | (0.99) | |
| D. | TSIV for Districts Above Median Size (land area) District | 615 | | 325 | ** | 1.67 | ** |
| | | (608) | | (163) | | (0.78) | |
| | T-Stat for Difference in Panel A and Panel B Estimates | 1.88 | | 1.97 | | 0.93 | |

*Notes to Table 6:* Estimates are from models similar to those used for Panel C of Table 3. The model used for Panel A limits the second stage sample to 1,590 districts located in metropolitan statistical areas. The model used for Panel B limits the second stage sample to 1,019 districts that are not located in a metropolitan statistical area. The models used for Panels C and D split the sample based on the land area of a district. Panel C contains results for the 1,306 districts with land area below the median land area of districts in the sample, while Panel D contains results for the 1,314 districts with above-median land area. Standard errors are in parentheses below each estimated coefficient and are calculated using the methodology suggested by Murphy and Topel (1985) and Inoue and Solon (2010) .
* indicates significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level

Appendix: Table A.1 Estimated coefficients for the TSIV models in Panel C of Table 3

| | 1 | 2.1 | 2.2 | 2.3 |
|---|---|---|---|---|
| Dependent variables: | $T_{is,2000}$ | Change in Mean House Values | Change in Mean Household Income | Percent Change in # of Households |
| $T_{is,2000}$(Net outgoing transfer rate) | | 1853 *** | 629 *** | 2.46 *** |
| | | (607) | (171) | (.857) |
| (Neighboring Districts' % White – Own % White Residents) | -8.11 *** | | | |
| | (1.91) | | | |
| Number of Neighboring Districts | 0.170 *** | | | |
| | (.065) | | | |
| *Change between 1979 and 1989 in…* | | | | |
| Households (Change as a %) | -0.016 | 0.663 | 19 | 0.160 |
| | (.013) | (51) | (20) | (.103) |
| Households with Children (Change as a %) | 0.027 * | -27 | -10 | -0.057 |
| | (.016) | (62) | (24) | (.115) |
| Mean Household Income | -0.035 | 1494 *** | -429 ** | 0.782 |
| | (.091) | (373) | (191) | (.533) |
| Percent Non-White | 0.094 * | -120 | -91 | 0.066 |
| | (.053) | (166) | (61) | (.305) |
| Mean House Value | -0.036 | -800 *** | | |
| | (.03) | (182) | | |
| *1989 Levels* | | | | |
| # of Households (thousands) | 0.065 *** | -417 *** | -56 ** | -0.425 *** |
| | (.022) | (101) | (28) | (.131) |
| " " " squared | -3.34 *** | 20392 *** | 3015 ** | 16 *** |
| | (1.19) | (5078) | (1394) | (5.729) |
| % of Households with Children | 1.33 | -77040 | -24903 | -212 ** |
| | (16) | (48434) | (22022) | (90) |
| " " " squared | -3.85 | 63933 | 29750 | 273 *** |
| | (17) | (54440) | (21443) | (90) |
| Mean Household Income ($thousands) | 0.051 | -1815 *** | -21 | 0.267 |
| | (.112) | (570) | (206) | (.567) |
| " " " squared | -0.0005 | 5.26 | 5.63 | -0.007 |
| | (.001) | (7) | (3.565) | (.004) |
| Percent Non-White | -0.070 ** | -62 | 20 | -0.098 |
| | (.036) | (115) | (32) | (.22) |
| " " " squared | 8.56 * | -15589 | -11875 ** | -34 |
| | (4.68) | (16014) | (4722) | (34) |
| Percent Urban | 0.654 | -9459 *** | -998 | -2.32 |
| | (.555) | (2213) | (621) | (2.34) |
| Mean House Value | 0.005 | 1280 *** | | |
| | *(.023)* | *(149)* | | |
| District Spending per Pupil ($thousands) | 0.0005 *** | -0.821 | -0.864 ** | -0.001 |
| | (.00018) | (.909) | (.342) | (.002) |
| Proportion of Residents over age 64 | 10 * | -94664 *** | -25377 ** | -108 ** |
| | (5.49) | (23108) | (10099) | (45) |
| Proportion of Heads of Households without high school diploma | -6.95 ** | 21090 * | 7025 * | 6.99 |
| | (3.14) | (11474) | (4056) | (23) |
| Proportion of Heads of Households with a Bachelor's degree | -9.70 ** | 26087 | 26056 *** | 57 ** |
| | (4.02) | (16334) | (6818) | (24) |
| Proportion of Heads of Households who own their home | 4.29 * | 6291 | 9169 *** | 26 * |
| | (2.31) | (10030) | (3285) | (15) |
| N= | 1,696 | 2,613 | 2,620 | 2,620 |

Notes to Table A.1: Column 1 displays estimates for the first-stage equation for the TSIV model with second stage estimates displayed in column 2.1. The first-stage equation for the models in columns 2.2 and 2.3 are similar, except they do not control for prior levels and changes in house values. All equations also include controls for state fixed effects (not shown here).
* indicates significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level