Cracks in the Melting Pot:  
Immigration, School Choice, and Segregation *

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Abstract

This paper examines whether the large wave of Mexican immigration to the United States since 1970 has lowered native demand for public education. Our analysis focuses on California – where many of these immigrants settled – accounts for possible endogeneity of immigrant inflows using established settlement patterns, and uses relative outflows of children from a district in an attempt to identify shifts in district choice in response to immigration-induced changes in school quality. We find that between 1970 and 2000, the average metropolitan school district in California lost at least 12 non-Hispanic children to another school district and two to private school within district for every ten additional low-English Hispanic arrivals in its public schools.

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I. Introduction

The boom in immigration from Mexico and other Latin American countries in recent decades has led to striking changes in the demographic composition of the U.S. population. This is true for adults and children alike: By 2000, first- and second-generation Mexican immigrants alone represented 7.5 percent of the U.S. population under 18 – or roughly half of all Hispanic children – up from only one percent in 1970. In California, which has historically been and continues to be the primary destination for these immigrants, shifts in the demographics of children have been even more striking, with first- and second-generation Mexicans accounting for over a quarter of children in the state by 2000 (Figure 1).1

These demographic changes pose challenges for local governments, particularly in the realm of public education. A majority of these children have parents without high school degrees and enter public schools with limited English skills. Even if these children are not placed in the same classrooms as native English speakers, their presence in public schools potentially has negative spillovers for native students. For example, immigration can lead to overcrowding, if school facilities do not adequately expand to accommodate the increase in enrollment, or to reductions in discretionary school spending, if categorical aid fails to cover what could be relatively high costs of educating English Language Learners (ELLs). Such developments may reduce the output of schools for native students. Further, any resulting departures of natives from public schools may reinforce the isolation of immigrants in schools and neighborhoods, potentially hindering their acquisition of human capital (Cutler, Glaeser, and Vigdor, 2008a).

In this paper, we examine these issues using a revealed preference approach, focusing on how increases in the presence of low-English Hispanic immigrants in public schools have shaped

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1 Our calculations from the 1976 Survey of Income and Education and the Decennial Censuses show that California was home to about 53 percent of all Mexican first- and second-generation children living in the United States between 1976 and 1990. This share fell by 2000 as Mexicans spread across the country (Card and Lewis, 2007), but still remained high at 44 percent.
decisions over school district of residence for non-Hispanic households with children since 1970.\textsuperscript{2,3} We center our analysis on California, which as noted, has received more of these immigrant arrivals than any other state over the past few decades. Anecdotally, many of the scenarios described above have played out in the state. California public schools are known to be overcrowded, due in part to an institutional environment that makes it difficult to raise funds for capital improvements (Cellini, Ferreira, and Rothstein, 2010), and in part to enrollment pressures that stem largely from immigration. ELL students are also viewed by Californians as being costlier to educate than natives, even after bilingual education ended with passage of Proposition 227 in 1998 (Baldassare, 2005).

We face two problems in attempting to infer an effect of immigration on school quality from native choice over district of residence. First, immigration may affect other amenities associated with residence in a school district (Saiz and Wachter, 2006) and may raise rents if the supply of housing is not fully elastic (Saiz, 2003, 2007). Native households may therefore find it optimal to relocate in response to immigration even absent any effect of immigration on school quality. Second, immigrant settlement is not random. Lower housing costs may make a declining school district attractive for an immigrant family, or unobserved factors might make a school district more or less attractive for natives and immigrants alike. Either way, simple correlations between immigrant inflows and native outflows will not identify the causal effect of immigration on native location decisions – an issue well established in studies of the native migration response to labor market competition from immigration (e.g., Card and DiNardo, 2000; Card, 2001; Card, 2007).

We approach the latter of these identification problems by constructing an instrument for the arrival of low-English Hispanic public school students to a district based on the spatial

\textsuperscript{2} Several papers have examined the direct effects of immigration on native educational outcomes at the elementary and secondary level (Betts, 1998; Gould, Lavy, and Paserman, 2009). Betts and Fairlie (2003) also estimate how immigration affected private school enrollment rates of natives using data for U.S. metropolitan areas in the 1980s.

\textsuperscript{3} Constraints on data available at the district level prohibit us from looking directly at the native response to immigration from Mexico and other Latin American countries. Indeed, the non-Hispanics in our population data may include immigrants (e.g., from South Asia), and our inability to separate native- and foreign-born Hispanics means that we cannot investigate how native Hispanics responded to these immigrant flows.
distribution of Mexican immigrants in 1970. The idea behind this approach is that existing Mexican communities should have made some districts relatively more attractive for new immigrant settlement irrespective of their other attributes, including school quality. California provides fertile ground for using this approach: Employment opportunities both in urban areas and in agriculture meant that the existing stock of foreign-born Mexicans was spread throughout the state, not concentrated in a few places where they would have already been affecting school quality for natives. Along these lines, we find no evidence that initial Mexican settlement patterns are related to proxies for existing school quality, such as per-pupil property tax revenues and expenditures prior to school finance equalization, or to demand for local public schools revealed by existing levels and prior trends in the distribution of non-Hispanic children across school districts. It is, however, strongly related to growth in the presence of low-English Hispanic students in a district, as predicted.

To isolate the contribution of immigration-induced changes in school quality on district choice, we then embed this instrumental variables strategy in a model that compares the change in a district’s stock of non-Hispanic households with children, which arguably place more weight on school quality when choosing a residence, to that for other non-Hispanic households with relatively young heads. That is, we aim to test whether the arrival of low-English Hispanics to a district as a result of Mexican immigration led to relatively large departures of non-Hispanic households with children. The assumption here is that the comparison households on average reacted in the same way to other changes in district attributes brought about by immigration, and to this end, we choose a comparison group that expresses similar sentiments regarding immigration. However, the use of a comparison group has an additional benefit, allowing for the possibility that existing Mexican settlement may predict future changes in a district’s desirability for all households. Indeed, the only

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4 We omitted older households (those with heads aged 50 or above) both because they tend to express stronger anti-immigrant views, and because Proposition 13 (1978) and subsequent ballot initiatives have affected both the level and age profile of mobility for California residents who owned homes in the 1970s. See below for more discussion.
requirement for identification is that the instrument be unrelated to unobservables that differentially affect the location decisions of non-Hispanic households with children – namely, school quality.

We implement this identification strategy using district-level population and enrollment data from the U.S. Census and other sources. Our estimates imply that between 1970 and 2000, the average metropolitan school district in California lost at least 12 non-Hispanic children for every 10 additional immigration-induced ELL Hispanic arrivals in its schools. This finding implies a greater than one-for-one “displacement rate” at the household level, working against a competing hypothesis that moves of non-Hispanic families were driven solely by a fixed supply of single-family housing. Further, districts that received more ELL Hispanics through immigration also experienced relatively large increases in the private school enrollment rates of non-Hispanic children residing within their boundaries. While we cannot rule out that non-Hispanic households with children were more sensitive to the effects of immigration on local communities along other dimensions, on balance our findings are consistent with an effect of Mexican immigration on school quality.

Unfortunately, our approach does not allow us to pinpoint how school quality may have changed as a result of immigration. Our focus on California nevertheless provides several clues. California maintained bilingual education under a highly egalitarian and potentially under-funded school finance system for most of the period of study. Mexican immigration may therefore have exacerbated crowding in schools or generated reductions in already limited resources for native students. On the other hand, interactions between low-English immigrants and natives within classrooms would have been somewhat rare over much of this period. That said, we cannot rule out that sorting occurred purely on the basis of ethnicity or income, or a potentially misplaced perception about how immigrants impact public education.

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5 This complements existing evidence of such an effect at the metropolitan area level (Betts and Fairlie, 2003)
6 See Brunner and Sonstelie (2006) for a discussion of school finance in California.
Because our estimates are mediated by California institutions, they cannot necessarily be generalized. Still, our findings speak to the potential importance of demand for local public goods – public education in particular – in residential and school segregation for a substantial share of the country’s largest immigrant group.

II. Theoretical Framework

Because school quality is hard to measure, our empirical analysis aims to infer whether Mexican immigration has affected school quality for natives from changes in the decisions of native households over school district of residence. The framework presented in this section, drawing from the insights of Tiebout (1956), illustrates the ways in which immigration may affect location decisions more generally and highlights the conditions under which we might plausibility isolate school quality-influenced district choice. Throughout this section and much of the next, we refer to Mexican immigrants as “immigrants” and non-Hispanics as “natives” for ease of exposition.

Let the indirect utility, \( V \), associated with residence in a particular school district be a function of school quality, \( q \), all other local amenities, \( g \), and housing costs, \( p \).\(^7\) A native household of type \( j \) will choose to reside in a particular school district provided that the resulting utility is at least as large as that associated with residence in the best alternative school district, \( v \):

\[
V'(p, q, g) \geq v.
\]

For all groups \( j \), \( V \) is (weakly) decreasing in \( p \) and (weakly) increasing in both \( q \) and \( g \).

Now suppose that a district experiences an increase in its immigrant share, \( \delta \), in public school enrollment. Equation (1) implies that a native household will choose to move to another district if \( V \) falls below \( v \). A rising immigrant share in enrollment may prompt such a move by reducing the quality of education for native children. However, it may induce cross-district moves for other

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\(^7\)Housing costs include both the (rental) price of housing and taxes. Utility would generally be modeled as (weakly) increasing in income less these housing costs, or potential consumption. We abstract from the effects of income here since cross-district moves within a household’s choice set (a labor market) are not likely to change it.
reasons. For example, immigration may reduce other amenities associated with living in the district, potentially reducing $V$. If the housing stock is not perfectly elastic, the increase in population will also raise housing costs, all else constant, again potentially reducing $V$. The housing market returns to equilibrium when $p$ adjusts sufficiently to restore (1) for all groups remaining in the district.

Thus, the reduced-form relationship between changes in $i$ and the presence of native households in a district does not immediately reveal that natives are changing school district of residence on the basis of changes in $q$. We also cannot make inferences about the effects of immigration on $q$ from changes in $p$, since house prices may also reflect the effects of immigration on other district amenities (Saiz and Wachter, 2006). To isolate the contribution of school quality, we therefore restrict attention to district choice and make the additional assumption that there is some household type ($j=1$) – namely, households with children– that values school quality more than – but $p$ and $g$ identically to – some other subpopulation of native households ($j=0$), in the sense that $\partial V^1 / \partial q > \partial V^0 / \partial q$ and $\partial V^1 / \partial x = \partial V^0 / \partial x$ for $x=p,g$. Under these assumptions, a school district will experience relatively large losses of native households with children only if

\[
\left( \partial V^1 / \partial q \right) \left( \partial q / \partial i \right) < 0 ,
\]

or only if immigration reduces school quality.\(^8\)

Our empirical model is thus designed to test whether school districts that experienced larger increases in $i$ also experienced relatively large declines in households with children – that is, relative to declines for some other subpopulation of households with otherwise similar preferences. If these location decisions are a response to changes in school quality, we may also expect to see larger increases in the private school enrollment rate of natives in districts with larger increases in $i$.\(^9\)

\(^8\) We assume here that $\partial p / \partial i$ is the same for both household types. If immigrant and native families with children tend compete for the same types of houses (e.g., single-family residences), immigration may raise house prices relatively more for “type 1” households, prompting greater relocation of native households with children even in the absence of changes in school quality. We explore this alternative hypothesis below.

\(^9\) $v$ is theoretically (weakly) increasing in the availability of alternative school districts in a metropolitan area, suggesting that, all else constant, if school quality matters, increases in $i$ should have greater impacts within metropolitan areas with
therefore also examine how increases in a district’s immigrant share affect private school enrollment rate of natives residing within its boundaries.

To this point, we have assumed that changes in $i$ at the district level are exogenous – unrelated to unobserved factors that might compel natives with children to depart a district’s public schools with relatively high probability. Nevertheless, equation (1) elucidates several potential sources of endogeneity in $i$. For example, declines in a district’s school quality for other reasons may induce native households with children to relocate. Immigrant families may be attracted to these districts by way of lower rents, particularly if they tend to place relatively less value on schools in their choice of residence. In this case, native departures from a district’s public schools will generate increases in $i$, not vice versa. Alternatively, a positive shock to school quality might attract both native and immigrant households with children, possibly generating a positive correlation between $i$ and the presence of native children in a district. We approach this identification problem using an instrumental variables approach described in the next section.

III. **Empirical Strategy**

**III.A. Basic Model**

Our empirical strategy comes out of the theoretical framework presented above, in which native households with children weigh school quality more in their choice of school district than other households with otherwise comparable preferences. Empirically, we restrict the district choice set to the metropolitan area, which by construction is intended to capture a labor market. It is common to assume that metropolitan areas also define markets for schools (e.g., Hoxby, 2001; Urquiola, 2005; Rothstein, 2006).

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10 The foreign-born do appear attracted to places with lower rents. For example, Boustan (2010) shows that the foreign-born were attracted to center cities that whites had earlier fled in response to black in-migration. Saiz and Wachter (2006) find evidence that immigrants settle in tracts with lower rents.
At an individual level, a linear version of the behavioral model in Section II may be written:

\[
Y_{idmt}^j = \gamma_{i^j} + \beta_{i^j} + \eta_{idmt}^j,
\]

where \( Y_{idmt}^j = 1 \) if native household \( i \) of type \( j \) resides in school district \( d \), which is located in metropolitan area \( m \), at time \( t \). This decision is a function of the share of public school enrollees who are immigrants in \( d \) at time \( t \), \( i_{dt} \), the response to which varies with the type of household: \( j=1 \) for households with school-aged kids (the treatment group), and \( j=0 \) for the comparison group. This decision is also a function of other district characteristics at time \( t \) not differentially valued by household type, captured by \( \gamma_{i^j} \). The effect of interest is \( \theta = \beta_1 - \beta_0 \), the difference between treatment and comparison households in the sensitivity of location decisions to \( i \). If the comparison group on average values other location-specific amenities and housing costs the same as the treatment group – \( \theta \) will only be non-zero only if immigration affects school quality.

We are not able to estimate equation (2) (e.g., using a multivariate logit model) given a lack of individual-level data with sufficient geographic detail. Instead, consider summing (2) across all native households \( i \) of type \( j \) in metro area \( m \) at time \( t \), then dividing by the total number of households of type \( j \) in \( m \) at time \( t \). We then arrive at a model for the share of households of type \( j \) in \( m \) choosing \( d \) at time \( t \):

\[
(2') \quad \frac{N_{idmt}^j}{N_{idm}^j} = \gamma_{i^j} + \beta_{i^j} + \epsilon_{idmt}^j.
\]

Differencing over time within household type, then across types, eliminates the effects of all common factors affecting location at a point in time (the \( \gamma_{i^j} \)) and generates our model of interest:

\[
(2'') \quad \Delta \frac{N_{idm}^1}{N_{idm}^1} - \Delta \frac{N_{idm}^0}{N_{idm}^0} = \theta \Delta i_{dt} + (\Delta \epsilon_{idm}^1 - \Delta \epsilon_{idm}^0).^{11}
\]

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11 \( Y \) in equation (2) might also be interpreted as the latent propensity to live in district \( d \), with the probability of living in the district a non-linear transformation of it. If this transformation is a logistic CDF, then equation (2') will be the same
The dependent variable is the treatment-comparison difference in $\Delta \frac{N_{i,j}}{N_n}$. Restated, the coefficient of interest, $\theta$, therefore captures how much larger a proportion of a metropolitan area’s native households with children choose a district with a marginal increase in $\Delta i_d$. If immigration negatively impacts school quality, we would expect $\theta$ to be negative.

As noted, we face two identification problems in estimating model (2’’). The first is that, for $\theta$ to represent only sorting in response to school quality, it must be the case that the comparison households on average value school quality less and all other factors affecting location decisions the same. The second is that $\Delta i_d$ may be correlated with the error term. We discuss our attempts to deal with each of these issues in turn.

III.B. Comparison Group

In practice, it is challenging both to identify and use a valid comparison group in our analysis. We rule out the use of households with older heads at the outset on several grounds. First, the transactions costs of moving differ dramatically between younger households and households already established in the 1970s – and within the latter group across the life cycle – as a result of California’s Proposition 13 and subsequent ballot initiatives.12 Second, older individuals (e.g., those over age 50) tend to express more negative views about immigration, suggesting that, all else constant, their residential choices may be more sensitive to immigration.13

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12 Proposition 13, passed in 1978, effectively locked in property taxes for existing homeowners by establishing a statewide property tax rate of 1 percent and setting assessed valuations of property at 1975 levels, with a maximum increase of 2 percent per year and no re-assessment. Propositions 60 and 90 in 1986 and 1988, respectively, allowed individuals aged 55 and over to transfer this tax benefit to a new home of equal or lesser value. These measures generated a sharp increase in residential mobility at age 55 (Ferreira, 2009).

13 The General Social Survey asks a variety of questions about views on immigration. We examined the following questions, available in the 1996 and 2000 waves: “How much do you agree or disagree with the statement[s]:” (1) “Immigrants take jobs away from people who were born in America,” (2) “Immigrants increase crime rates,” (3) “America should take stronger measures to exclude illegal immigrants,” and (4) “Immigrants are generally good for America’s economy.” We created a variable that attempted to summarize negative views of immigration, the sum of...
Ideally, we would therefore exclude households with older heads from the analysis, regardless of the presence of children, leaving a comparison of the residential choices of relatively young households with and without children. While a direct test of the quality of this comparison is impossible, support for this approach comes from the fact that households with younger heads tend to express similar views toward immigration, regardless of the presence of children. Many of the comparison households may also have children at some future point in time.

Unfortunately, the available data do not permit us to structure our analysis exactly in this way. Instead, we define our treatment and comparison groups using available data on population by five-year age group at the school district level, discussed in Section IV. Our baseline model considers the treatment group to be 0 to 19 year olds and the comparison group to be 20 to 49 year olds. This model has the obvious drawback of including in the comparison group individuals who are parents, but also has the advantage of simplicity. Further, the fact that the comparison group is partially treated biases us against finding an effect.

We show this directly by estimating another model that arises through an alternative aggregation of equation (2) (see Appendix for derivation):

\[
\Delta \frac{N_{a,m}^d}{N_{a,m}^d} = \gamma'_d + \theta'(\varphi_{a,m} \cdot \Delta \mu_{a,m}) + \Delta \mu_{a,m}^{''}
\]

where \(\Delta \frac{N_{a,m}^d}{N_{a,m}^d}\) represents the share of individuals in age group \(a\) in metropolitan area \(m\) residing in district \(d\), \(\varphi_{a,m}\) represents the fraction of age group \(a\) in households with children, and \(\gamma'_d\) is a district dummy for the “agree” and “strongly agree” responses for (1)-(2), “strongly agree” for (3), and “disagree” and “strongly disagree” for (4), each of which is one for 30-40 percent of respondents. This shows a significant positive linear relationship with age for age groups above but not below 50. Unrestricted age group (5-year bands) effects, conditional on a year effect, are jointly significant at the 5 percent level among those over 50 but not under 50. The mean of this sum, conditional on a year effect, is 0.25 higher (std err = 0.054) for those over 50 than those under 50. All three facts are true (with slight changes in the coefficients) whether one uses age as of the survey year or age as of 2000 (i.e. cohort) as the measure of “age.” Finally, the first principal component of the 16 dummies for all possible responses (“neither agree nor disagree” excluded) to the four questions follows a similar age and statistical significance pattern.

14 If we add a dummy variable for the presence of children in the household to any of the models described in the footnote above, its coefficient is statistically insignificant. This is also true specifically in the sample under 50.
fixed effect (which represents a district-specific trend in this first-differenced specification). Thus, if we regress the district share of the age-specific metropolitan area population on district fixed effects and a new variable, $\varphi \Delta i_{d,t}$ – the change in percent immigrant interacted with the fraction of the age group which has kids – we obtain an alternative estimate. To be comparable to estimates of $\theta$, however, the estimate of $\theta'$ must be adjusted for the fact that it represents the outflows of people – children and adults with kids. After this adjustment, (3) provides an estimate which removes the bias in our main approach from including some parents in the comparison group. \textsuperscript{15}

Of course, we can never be entirely sure that our comparison group satisfies the assumptions outlined above. For example, younger households without children might be less responsive in their choice of residence to immigration-induced changes in other amenities, or may be less apt to compete with immigrants for housing, leading us to overstate the role of immigration-induced changes in school quality in residential choice, or even to conclude falsely that school quality matters at all. However, estimates without a comparison group are sensitive to the sub-sample of cities under consideration, unlike those using a comparison group, suggesting that the use of a comparison group removes effects of other shocks to the cost or quality of particular locations – immigration-induced or not – that would otherwise bias our inferences. We also estimate a model analogous to (2''), but for the native private school enrollment rate, where under reasonable assumptions any relationship would reflect changes in school quality brought about by immigration.

III.C. Instrument

For ordinary least squares (OLS) to produce a consistent estimate of $\theta$, it must be the case that $\Delta i_t$ is unrelated to all unobserved determinants of differences in the district choices of treatment and comparison native households. As described above, immigrant families may be attracted to

\textsuperscript{15} We choose not to use this model at baseline for several reasons. The first is that the model's derivation suggests the use of metropolitan area and time varying estimates of $\varphi_a$, but precise estimates of these are not obtainable from the Census PUMS. The second is that estimation of this model requires that the individual level binary-response model be linear. If this is not the case (e.g., in a logit model), the model cannot be aggregated.
districts that native households with children are already departing, or immigrants and natives alike may be attracted to the same places. However, even if immigrants were randomly assigned to school districts, any resulting departures of natives will inflate $\Delta i_d$. Further, any measurement error in $\Delta i_d$ will attenuate our estimates.

We address biases from reverse causation and omitted variables by constructing a prediction of future immigrant location decisions on the basis of existing immigrant settlement – an approach widely used to examine the impacts of immigration at the metropolitan-area level (e.g., Card and Dinardo, 2000; Card, 2001). Before introducing this prediction, it is useful to recall that our focus is on the impacts of Mexican immigration to California. Our proxy for the presence of such immigrants in a school district is the number of Hispanic ELL students in its public schools, which we observe in 1976 and 2000. In practice, $\Delta i_d$ is thus the 1976 to 2000 change in the share ELL Hispanic among a school district’s public school enrollees. While this measure misses some Mexican immigrant arrivals to a district’s public schools, it captures the dimension of these immigrant inflows that may be particularly salient for natives’ location decisions.

In light of these data constraints, we focus on obtaining of prediction of low-English Mexican arrivals to a district’s public schools:

$$\hat{\Delta I}_d = \frac{M_{d}^{1970} \Delta I}{M_{1970}}$$

where $M_{d}^{1970}$ / $M_{1970}$ is the share of the Mexican-born population (of all ages) residing in district $d$ in 1970, based on school district tabulations from the Census, and $\Delta I$ represents the number of “low English” children of school age in the 2000 Census who were either born in Mexico or whose parents were born in Mexico and arrived in 1976 or later.16 We classify as “low-English” those who

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16Second generation Mexicans accounted for a high share of low-English Hispanics enrolled in public schools in 2000. Bleakley and Chin (2008) show that U.S.-born of immigrants who arrived after age 9 are more likely to be low English.
do not speak English at all, who speak English (but not well), or who speak English well (but not very well), since this definition close to reproduces ELL shares in enrollment at the metropolitan area level and in California overall as observed in administrative data.\textsuperscript{17} Thus, $\hat{\Delta}I_d$ apportions low-English first and second-generation Mexican public school students to school districts as if the settlement patterns of Mexican immigrants in the United States had not changed since 1970.

We address the more mechanical source of bias described above in transforming the prediction in equation (4) into an instrument for $\Delta i_d$. Here, we assume that $\hat{\Delta}I_d$ represents the only source of enrollment change in a district. In particular, our instrument, $Z_d$ is:

\begin{equation}
Z_d = \frac{I_d^{1976} + \hat{\Delta}I_d}{Enr_d^{1976} + \hat{\Delta}I_d} - \frac{I_d^{1976}}{Enr_d^{1976}},
\end{equation}

where $I_d^{1976}$ and $Enr_d^{1976}$ represent the initial (1976) enrollment of ELL Hispanics and total enrollment, respectively, in district $d$. The second component of $Z_d$ is thus the initial share ELL Hispanic in the district, while the first represents what that share would have been in 2000 assuming that nothing else had changed. Considering that $Z_d$ is another noisy estimate of actual growth in immigrant share, using $Z_d$ as an instrument may allow us to remove bias from measurement error as well.

Appendix Table I gives the top ten districts in our sample ranked by both the share of Mexicans in the United States residing in the district in 1970 ($M_d^{1970}/M^{1970}$) and the value of the instrument; data sources are described below. Unsurprisingly, larger districts rank high on the first measure: Los Angeles Unified alone has home to over 16 percent of Mexicans in the United States

\textsuperscript{17} Our TSLS estimates are not sensitive to how “low-English” is defined, or alternatively, to using all Mexican first and second-generation immigrants regardless of English status; this is to be expected given that $\Delta I$ depends in no way on the district. Our use of low-English classification simply serves to facilitate interpretation of the estimates, as shown below. Similarly, using 1976 data to calculate an alternative measure of the change in the number of first- and second-generation Mexicans over 1976 to 2000 has no impact on our estimates.
in 1970.\textsuperscript{18} However, the largest districts are not those with the largest predicted increases in low-
English Hispanic shares. And while districts with the highest values of the instrument appear to be
clustered in the Central Valley, nearly 80 percent of the variation in $Z_d$ in our sample is within, not
across, metropolitan areas. The implied dispersion of existing Mexicans across California is
consistent with the historical antecedents to the modern wave of Mexican immigration, which pulled
Mexican immigrants to the countryside of the state as well as to urban areas.\textsuperscript{19} In general, the
instrument is not related to district size or location, as shown below.

Two-stage least squares (TSLS) estimates of model (2'') will identify $\theta$ if the predicted change in immigrant share defined in (5) is unrelated to unobserved shocks to the desirability of a
district for non-Hispanic children. In particular, it must be the case that established settlement
patterns of Mexican immigrants do not predict subsequent (unobserved) shocks to school quality. It
is impossible for us to test this assumption directly. However, we can test whether $Z_d$ is correlated
with relative outflows of non-Hispanic children from a district over the 1960s or with an uneven
distribution of children and young adults in 1970 – before the big wave of Mexican immigration, but
when the district may already have been in decline. In addition, we will test whether the instrument
is correlated with other district characteristics observed in the early 1970s, some of which may
predict whether a district will later become less attractive for non-Hispanic children.

\section*{IV. Data}

\subsection*{IV.A. Sources, Key Variables, and Sample}

Our data sources on district of residence are the 1970 and 2000 school-district tabulations of
the U.S. Census of Population (hereafter referred to as the School District Tabulations, or SDT).

\textsuperscript{18} Cities outside of California that rank high on this measure include Chicago City (4.4 percent), El Paso ISD (3.1
percent), San Antonio ISD (2.3 percent), and Houston ISD (1.6 percent). Outside of California, however, foreign-born
Mexicans were much more concentrated in 1970, making application of our identification strategy less compelling.
\textsuperscript{19} The Bracero Program brought an estimated 5 million agricultural guest workers to the United States (many of these to
California) between 1942 and 1964.
The SDT provides information on the distribution of foreign-born Mexicans across school districts in 1970, which we use to predict future immigrant arrivals to the district (equation (4)). It also provides non-Hispanic population counts by age group, which we use to construct our main dependent variable – the treatment-comparison difference in the 1970 to 2000 change in the district share of the metropolitan area’s non-Hispanic population. Finally, we use information on school enrollment by type (public/private) and ethnicity in the SDT to construct the 1970 to 2000 change in the private school enrollment rate of non-Hispanics.

Data on public school enrollment by ethnicity and English proficiency at the district level were drawn from the 1976 and 2000 Elementary and Secondary School Civil Rights Surveys, conducted by the Office for Civil Rights in the Department of Health, Education, and Welfare (HEW, later the Department of Education) to monitor compliance with federal civil rights law (hereafter referred to as the OCR). Non-native English speakers became protected under federal law as a result of the Equal Educational Opportunity Act of 1974, and HEW set forth guidelines for accommodation and began monitoring district compliance in 1975. The first year in which data are available for all districts is 1976. We use district enrollment counts reported by OCR to create our proxy for growth in the presence of low-English Mexican immigrants in the public schools – the 1976 to 2000 change in the ELL Hispanic share in enrollment – and in construction of the instrument (equation (5)).

We restrict our analysis to school districts in the 23 standard metropolitan statistical areas (MSAs, by their 1990 definition) in California. For our analysis, we define “districts” so as to have

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20 Because school districts are sometimes missing in these data sets (see below), we obtain the non-Hispanic population at the MSA level using separately-reported county level aggregates downloaded from the National Historical Geographic Information System, rather than by aggregating across school districts.  
21 The Equal Educational Opportunity Act of 1974 defined as a denial of equal educational opportunity “the failure by an educational agency to take appropriate action to overcome language barriers that impede equal participation by students in an instructional program.”  
22 As suggested by Figure I, most of the 1970 to 2000 increase in California’s Mexican first and second generation child population appears to have occurred after 1976, so this likely has little effect on our findings. The 1980 SDT lacks sufficient disaggregation of population counts by age and ethnicity to apply our empirical strategy.
constant boundaries between 1970 and 2000, and aggregate key variables accordingly. \(^{23}\) We lose “aggregated” school districts for several reasons. First, while both the SDT and the OCR are in principle censuses of school districts, the 1970 SDT does not include the smallest school districts in the country (those with under 300 students), and there was some non-response to the 2000 OCR survey. We also limit attention to districts where data quality is high in both years of the OCR. \(^{24}\) By and large, most sample drops on these grounds occur because a district is missing in the 1970 SDT.

We make several additional exclusions to arrive at our estimation sample. There are three district types in California: unified districts, which operate schools at all levels; secondary districts, which operate high schools; and elementary districts, which operate primary and middle schools and feed into secondary districts. Including both secondary and elementary districts in our analysis would be redundant, as they cover the same geographic area. Secondary districts are more comparable in geographic scope to unified districts and, in the SDT, represent elementary districts that are not directly observed due to their small size. We therefore use secondary district boundaries to define observations for our main analysis and aggregate the OCR data accordingly. Our observations for “secondary districts” therefore represent a combination of elementary and secondary districts. \(^{25}\) In our main sample there are a total of 40 combined elementary-secondary districts and 193 unified districts, for a total of 233 observations.

**IV.B. Descriptive Statistics**

Table I shows summary statistics for our estimation sample. Panel A gives statistics on the key variables in our analysis. Consistent with the rise in first and second-generation Mexican

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\(^{23}\) For example, if districts A and B in 1970 merge to form C by 2000, we aggregate A and B to create an observation for C in 1970. We identify school district reorganizations using data from the Elementary and Secondary Education General Information System and the Common Core of Data Public Agency Universe and internet searches. We drop "aggregated" districts involved in reorganizations over the period if any of the component districts are not present in years they should be or, in a few cases, if we were not able to ascertain the nature of the reorganization that occurred.

\(^{24}\) We drop districts for which either of the following holds in either 1976 or 2000: (1) the sum of non-ELL enrollment by race was more than 10 percent above or below reported non-ELL enrollment; or (2) the sum of enrollment by race was more than 10 percent above or below reported enrollment.

\(^{25}\) Our estimates are robust to dropping these observations, as shown below.
children in the state (Figure I), the ELL Hispanic share in public school enrollment rose substantially – 10.7 percentage points, on a base of 2.6 percent (Panel C) – between 1976 and 2000. The standard deviation on this variable is also 10 percentage points, suggesting that there is quite a bit of variation in the growth in immigrant share across districts. The instrument has a comparable mean and standard deviation.

The main outcome – the change in the MSA share of enrollment for the average district – is close to zero for both our treatment group (0-19 year olds) and comparison group (20-49 year olds). This is expected by definition; deviations from zero come about because we are missing districts at times for the reasons described above. The private school enrollment rate of non-Hispanics rose by a considerable 10.3 percentage points, a more than 7-fold increase over its 1970 level of 1.4 percent (Panel B). While suggestive of potentially substantial “native flight” in response to immigration, this could be a consequence of school finance equalization (e.g., Downes and Schoeman, 1998). School finance equalization may have also affected the income heterogeneity of populations within school districts (Aaronson, 1999). In general, immigrant settlement could be correlated with the effects of school finance reform on a district, making equalization a potential confounding factor that we hope to rule out through our identification strategy.

In an effort to do this and to demonstrate the credibility of our identification strategy more generally, we have collected information on district property tax revenues and expenditures per pupil prior to the implementation of the Serrano decisions (for 1971-72 from the 1972 Census of Governments), as well as information on a number of other district characteristics circa 1970 from various historical sources. Descriptive statistics on these variables are shown in Panel C of Table I. The average district in our sample enrolled about 14,849 students and raised $636 per-pupil in

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26 By the definition of low English used to construct the instrument, the share low-English Mexican in the California child population rose from 4.6 percent in 1976 to 16.3 percent in 2000 – a comparable 11.7 percentage point increase (authors’ calculations from the 1976 Survey of Income and Education and the 2000 Census PUMS).
revenues through the property tax and spent $1084 per student, with a standard deviation of around $300 in each. About 8 percent of districts in our sample are in center cities, and 71 percent had a private school serving any grade level as of 1976, the first year in which such information is available.

V. Identifying Assumptions

Our identification strategy relies on the assumptions that the instrument is related to changes in the actual presence of Mexican immigrants in a district’s public schools, but unrelated to other factors that might have compelled native households with children to locate elsewhere, like a loss in the district’s fiscal autonomy as a result of school finance equalization or existing levels and trends in school quality. Before turning to our main estimates, it is useful to examine the credibility of these assumptions.

The first row of Table II speaks to the former, showing that there is a strong first-stage relationship between the instrument and the change in a district’s ELL Hispanic share in our baseline specification, which includes fixed effects for MSA by district type.27 Figure II shows this relationship graphically. A coefficient of one on the instrument would be expected if predicted arrivals matched actual arrivals, on average, and enrollments were otherwise changing little. The actual first-stage coefficient is statistically distinguishable from zero, at the one percent level, but also significantly less than one. Fewer than the expected number of ELL Hispanic schoolchildren actually show up in districts, possibly due in part to the fact that Mexican immigrants have spread out geographically over time, especially since 1990 (Card and Lewis, 2007).

27 We include MSA by district type fixed effects because both the instrument and private school enrollment vary across MSAs and district types. Districts are also sometimes missing from the sample for reasons described above, so that the sum of district shares of the MSA non-Hispanic population do not sum to one within MSA. Standard errors are clustered on metropolitan area, and t-statistics are compared to critical values in a t-distribution with 21 degrees of freedom (the number of clusters less two, a rule of thumb suggested in Cameron et al., 2008).

18
As a preliminary exploration of the second identifying assumption, the remainder of Table II shows slope estimates from comparable reduced-form regressions of many potential correlates of outcomes on the instrument. By and large, the findings support the credibility of our research design. Consider first the estimates for initial district observables, shown in Panel C. While the instrument is negatively correlated with the likelihood of having a private school by 1976, it is unrelated to myriad other district characteristics. It is not significantly related to initial enrollment, which was used in its construction, or to center city status, despite the tendency of immigrants to cluster in center cities (Cutler, Glaeser, Vigdor 2008b; Boustan, 2010). It is also not correlated with the density of public schools within a district, which we capture with a dummy for whether a school district had an above average number of schools (in 1972) given its enrollment (in the 1976 OCR) and land area. Finally, relationships between the instrument and the school finance variables are statistically insignificant and small, amounting to, for example, only about $20 differences in per-pupil property tax revenues and per-pupil expenditures for a one standard deviation (0.10) change in the instrument. Still, confidence intervals on these estimates (and some of the others discussed above) are fairly wide, so we include all of these characteristics as controls in some specifications below.

Table II nevertheless shows that the instrument is significantly related to the share of public students who are ELL Hispanic in 1976. Mexican immigration may have already been changing the demographics of children in California in the early 1970s (see Figure I). Even if this was not the case, a relationship would not be surprising to see here given that the instrument derives from the

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28 The relationship is also not significant if the level of enrollment is used in place of the natural log of enrollment.
29 While center cities have larger Mexican enclaves, they also have more total public school students so the predicted increase in the low-English share need not be larger in the center city.
30 This could also be interpreted as showing that the instrument is uncorrelated the median voter’s preferences for segregation. We arrive at this prediction by regressing the natural log number of schools on the natural log in enrollment, the natural log of land area, their interaction, and all of the other pre-existing district characteristics listed in Table I, Panel B (except the initial ELL Hispanic enrollment share), and classifying as “high choice” those with non-negative residuals. We explored alternative regression models (e.g., in levels, not logs), as well as considered schools per square mile and schools per child enrolled. All yielded an insignificant relationship with the instrument.
size of a district’s Mexican-born population in 1970. The share of enrollment that was ELL Hispanic was, however, initially small compared to the change that occurred over the next 30 years (Table I), making any changes in district choice among non-Hispanics plausibly related to the predicted change rather than the initial level of low-English Hispanic share. Put differently, if the initial low-English Hispanic share represents a meaningful source of variation in the instrument, it should be the case that the instrument predicts initial levels (and prior trends) in school quality, invalidating our preferred interpretation.

The findings discussed above using proxies for school quality (like funding) suggest that this is not the case, but we can also examine the relationship between the instrument and preferences for a district’s public schools that have already been revealed. Table II Panel B shows that the instrument is unrelated to 1970 levels of our outcome variables; prior trends are considered in a separate table below. Despite having fewer private schools by 1976, the 1970 private school enrollment rate of non-Hispanics was not significantly lower in districts with larger predicted increases in their immigrant share. And while the instrument is negatively related, though not significantly so, to the share of an MSA’s non-Hispanics residing in a district in 1970 regardless of age group, the difference between the estimates for 0 to 19 year olds and 20 to 49 year olds – which represents the 1970 level version of our main dependent variable – is not statistically significant, and fairly precisely estimated. Thus, districts with higher values of the instrument had a similar relative presence of non-Hispanic children and similar private school enrollment rates in 1970, suggesting the quality of education offered did not already vary systematically with the instrument.

VI. Choice of School District

VI.A. Reduced-Form Estimates by Group

To make our identification strategy transparent, we begin the presentation of our findings by decomposing the reduced-form specification of model (2") into its constituent parts – separate
estimates, by age group, of the relationship between the instrument and the 1970 to 2000 change in the district’s share of the metropolitan non-Hispanic population. We do this in the first two columns of Table III, again conditioning on metropolitan area by district type fixed effects. The difference between these estimates, presented in column (3), is the reduced-form parameter of interest.

Panel A presents these estimates for the full sample. Expected increases in a district’s ELL Hispanic share in enrollment resulting from Mexican immigration are negatively related to the share of the MSA’s non-Hispanic children (0 to 19 year olds) it represents: a 0.1 increase in the expected ELL Hispanic share – roughly the mean of the instrument (Table I, Panel A) – is on average associated with a population loss amounting to 0.729 percent of this group’s MSA level population (column (1)). Given that the average district in our sample initially had 7.9 percent of an MSA’s non-Hispanics (Table I, Panel B), this figure represents about 9.2 percent of non-Hispanic children initially residing in the average district (0.729/7.9), and so represents a substantial effect.

On the other hand, not all of this effect may represent population losses due to declines in school quality or more generally, to declines in the attractiveness of the district for children: the comparable figure for 20 to 49 year olds is 0.425 percent of the group’s MSA population (column (2)). Our maintained assumption is that the estimate for the comparison group accounts for all other reasons that immigration might prompt moves from a district. The reduced-form parameter of interest is therefore the difference in these coefficients – a precise -0.0304, shown in column (3). Using the metric described above, this suggests that the average district may have lost up to about 3.8 percent (0.3/7.9) of its non-Hispanic children to location decisions on the basis of school quality for each standard deviation increase in the instrument. Given that many 20 to 49 year olds are parents, this figure arguably understates the effect.
Figure III gives a graphical representation of the reduced-form regression in column (3).

With the exception of large districts, like Los Angeles Unified, San Francisco Unified, and San Diego Unified – which naturally account for larger shares of their respective MSAs’ non-Hispanics at baseline – districts are tightly clustered around the downward sloping line. Below, we show that our findings continue to hold when large center city school districts are dropped from the sample – a finding which is not surprising given that the instrument is uncorrelated with district enrollment and center city status – and under alternative specifications of the dependent variable. We also present TSLS estimates that correspond to the same model, explore the sensitivity of this model to inclusion of controls, and discuss alternative ways to interpret the magnitude of the estimate.

The remainder of Table III includes a falsification exercise that complements that performed in Table II, Panel B. In particular, we test whether there was a similar “effect” on relocation in the 1960s – prior to the big wave of Mexican immigration. If so, it would suggest that it is not the arrival of Mexicans per se that drove the “outflows” of non-Hispanic children, but some other factor correlated with it. Constructing similar measures for the 1960 requires us to use a smaller sample, as detailed geographic tabulations are available only for a subset of counties in the 1960 census. Limiting the sample to the 13 metropolitan areas which are observable in the 1960 census, the 1970 to 2000 finding is still negative and significant and quite similar in magnitude to that found on the full sample: the reduced form effect, in column (3), is -0.0353. Nevertheless, note that the group-specific reduced-form relationships (columns (1) and (2)) with the instrument are much different in this subsample of older cities. The sensitivity of the estimates in column (1) but not (3) to sample suggests the utility of using a comparison group, as our identification strategy accounts for any

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31 Mexicans were arriving California as well in the 1960s, but in much smaller numbers. So it would not be surprising to also see a negative relationship in the 1960s, but it should be considerably smaller to support the preferred interpretation. Unfortunately, we cannot perform a similar exercise for private school enrollment since the 1960 Census tract data do not break out private school enrollment by Spanish origin.

32 In the 1960 Census only metropolitan counties were assigned tracts, the lowest level of geography with sufficiently detailed tabulations for our analysis.
unobservables that make a residence in district more or less desirable for non-Hispanics regardless of age.

The final panel shows the findings from the falsification exercise. Supporting the identifying assumptions of our model, there is no evidence that districts expected to experience greater increases in their ELL Hispanic shares as a result of Mexican immigration were already experiencing greater losses of children in the 1960s: the reduced form coefficient of interest (column (3)) is a small -0.0066 and is not statistically significant. Figure IV shows scatter plots that correspond to the underlying regression (Panel B) and its 1970 to 2000 counterpart for this restricted sample of cities (Panel A). The graph makes clear that the lack of a pre-trend is not driven by some outlying observation. Thus, a significant negative relationship between the instrument and the treatment-comparison difference in the outcome of interest appears limited to the period in which Mexicans were arriving in California in large numbers.

VI.B. Main Findings

Table IV shows our main results, TSLS (Panel A) and OLS (Panel B) estimates of (2"). The TSLS estimate in column (1) corresponds to the reduced form estimates in column (3) of Table III, Panel A, and thus includes fixed effects for MSA by district type and no further controls. The statistically significant point estimate of -0.0705 says that a 10 percentage point increase in the share of a district's public school enrollment that is low-English and Hispanic (again, roughly the mean, or one standard deviation) induces 0.7 percent of a metro area’s – or 8.9 percent of a typical district’s (0.705/7.9) – non-Hispanic children to relocate from the district. Note that this estimate is larger than the reduced form parameter presented in Panel A of Table III because the corresponding first stage coefficient is less than one (Table II, Panel A).33

33 The first-stage coefficient estimates on the instrument are very stable across specifications (available on request).
As anticipated, the TSLS estimates change little with the addition of controls for the pre-existing district characteristics listed in Table I, Panel C (column (2)) and for the initial (1970) level of the dependent variable (column (3)). In fact, they are slightly larger. By contrast, OLS estimates fall in magnitude with the addition of controls (Panel B). They are also considerably smaller at the outset (-0.0215 in column (1)) and not statistically significant in subsequent specifications. Aside from measurement error attenuation of the OLS estimates, a plausible explanation for finding OLS estimates that are larger in magnitude is the omitted variables story given above, whereby third factors attract both Mexican immigrants and non-Hispanic families to the same districts, for example, to those places where there tend to be better employment opportunities or cheaper housing.\textsuperscript{34} Such a phenomenon would bias against seeing any effect.

In the remaining columns of Table IV, we present estimates for different subsamples of the data. Column (4) drops center city districts from the sample. The TSLS point estimate is slightly smaller than the one in column (1) – possibly because these districts are on average smaller – but it remains highly significant. This finding confirms that our full-sample estimates are not driven by the sensitivity of this particular outcome measure to district size. Further, it shows that our main findings are not simply being driven by the fact that non-Hispanic families are moving out of center cities.\textsuperscript{35} Mexican immigration appears to have shaped non-Hispanic location decisions within the suburbs of California metropolitan areas.

The final columns break out the estimates by district type, first for unified districts (column (5)) and for the observations that represent a combination of elementary and high school districts (column (6)). Though we cannot reject that the estimates in columns (5) and (6) are identical, the

\textsuperscript{34} Saiz and Wachter (2006) see a similar pattern of TSLS and OLS estimates for native and white non-Hispanic population outflows applying a modified version of the instrumental variables strategy used here for changes in the foreign-born population at the Census tract level.

\textsuperscript{35} And in fact, although it is true that the center city share of the metro area's population is falling in our sample, it is not falling differentially for children.
findings suggest that our main estimates are driven entirely by unified districts. TSLS estimates for unified districts are slightly larger in magnitude but a bit less precise than those for the full sample, while estimates for the combined elementary-high school districts are much smaller and not statistically significant.

VI.C. Additional Sensitivity Analysis

Table V examines various alternative formulations of the dependent variable. The first row repeats the TSLS (column (1)) and OLS (column (2)) estimates that appeared in column (1) of Table IV for comparison purposes. The second row replaces the district’s share of the MSA’s population with the logit transformation of that share. The dependent variable is therefore the 1970 to 2000 change in log odds, which would be appropriate if the individual-level model underlying district choice is a logit model. Both OLS and TSLS estimates are significant in this version, and the marginal effects, evaluated at the mean district share of the MSA’s population, are larger than were estimated in the linear model.36 However, we see the same pattern as in Table IV: TSLS is larger in magnitude than OLS, and OLS is more sensitive to controls (not shown, but available on request).

One difficulty with both our main dependent variable and the change in log odds is that while they have good theoretical justification, their magnitudes are somewhat difficult to interpret. The third and fourth rows use easier to understand dependent variables: the difference in growth rates (change in logs) between the treatment and comparison populations (row (c)) and the change in the share of the non-Hispanic population ages 49 and under who are children (row (d)). In both cases there is again a significant negative relationship with the change in low-English Hispanic share. A one standard deviation increase in low-English Hispanic share of enrollment (0.10) is associated with a 11 percentage point decline in the relative population growth rate of non-Hispanic 0-19-year-olds, and a 3 percentage point decline in the child share in the non-Hispanic population.

36 The share of a metro area’s school aged population in the average district is 0.079, and thus we calculated the marginal effect as coefficient*0.079*(1-0.079).
How similar or different are these estimates? One way to put them in terms that are comparable to each other – and to other research which examines the impact of immigration on native enrollment (Betts and Fairlie, 2003) or native population location in general (e.g., Card and DiNardo, 2000) – is to restate the effects in terms of a “displacement” rate, that is, how many non-Hispanic children “leave” the district for each ELL Hispanic arrival.37

All of the TSLS estimates in Table V imply sizeable losses of non-Hispanic children. To increase the typical district's ELL Hispanic enrollment share by 10 percentage points would have required, starting with 1970 levels, the addition of about 1650 low-English Hispanics, all else constant.38 This 10 percentage point increase, according to our first specification, would lead 8.9 percent of the typical district’s non-Hispanic kids to leave the district, or about 1900 non-Hispanic kids in the typical district.39 So, the coefficient implies that for every 10 additional low-English Hispanics who arrived in a district's public schools, about 12 (=10*1900/1650) non-Hispanic children moved out of the (or chose another) district.40 Most of the other specifications in Table V imply even larger effects. The estimated marginal effect from the logit specification implies 14 non-Hispanics relocate for every 10 low-English Hispanic arrivals in the average district.41 The 11 percent loss of non-Hispanics in the growth specification (row (c)) also implies that 14 non-Hispanics relocate for every 10 low-English Hispanic arrivals. The share kids specification, (d), is smaller, implying only 7 non-Hispanic kids left for every 10 low-English Hispanic arrivals.42,43

37 Though the word “displacement” connotes a fixed capacity, it is important to not take that connotation literally in making this calculation. We discuss the interpretation of our estimates in the next section.

38 The average district in our sample had 14,850 students in 1976, 313 (2 percent) of whom were low-English Hispanics. To raise the low-English Hispanic share by 10 percentage points to 12 percent required the addition of about 1650 low-English Hispanic students, since (313 + 1650)/(14850 + 1650) ≈ 0.12. (When we calculate the displacement rates reported above, we retain more precision in the numbers.)

39 Again, the average district had 21,800 non-Hispanic 0 to 19 year olds in 1970.

40 Since only about two-thirds of non-Hispanic 0-19 year olds are enrolled in public schools, the loss of 12 non-Hispanic 0-19 year olds for every 10 Hispanic ELL enrollees still implies a slight increase in the enrollment in the public schools.

41 We have also computed this using the discrete change in odds of 0.1*-1.2 = -0.12, which makes little difference.

42 The average district had 46,700 0 to 49 year olds in 1970. So a decline in the share of these that were kids by 2.5 percentage points, as implied by the row (d) estimate, for a one-standard deviation increase in low-English Hispanic share, corresponds to the loss of about 0.025*46,700 = 1170 non-Hispanic kids.
Panel B of Table V examines estimates of (5), which take account of the fact that the 20 to 49 year olds we are using as a comparison group are actually partially treated, as many are likely to have children, which may bias downward our estimates. The dependent variable is again the change in the district’s share of the metropolitan area population of a given age group, but rather than two age groups, it stacks up ten 5-year age groups (0-4, 5-9, ..., 45-49). For comparison to the main results, the estimate in row (a) continues to ignore the fact that many 20-49 year olds have kids, and simply interacts the change in the low-English Hispanic share in enrollment (and the instrument for it) with a dummy equal to one for age groups 0-19, and zero for older age groups. Thus, row (a) is estimated almost equivalently to our main estimates (like equation (2'')) so should give us roughly similar estimates to the first row of the table. It does: the TSLS point estimate is -0.0710, which is similar to the main estimate of -0.0705.

Row (b) of Panel B takes the more sophisticated approach of interacting the treatment with a measure of the share of non-Hispanics in the age group in households with school-aged kids (estimated for all non-Hispanic respondents to 2000 PUMS residing in metropolitan California), as in equation (3). This share is one for 0 to 19 year olds, but instead of being zero for adults, it is the share of the age group that is “treated.” As expected, this approach produces a larger point estimate, but to be comparable to earlier estimates, it needs to be adjusted downward to reflect that it now represents flows of both kids and adults. In 1970, about 56 percent of the members of non-Hispanic households with children were children themselves. Multiplying the TSLS estimate by 0.56, we obtain a rescaled estimate of 0.092. It implies 15 non-Hispanic kids are displaced for every 10 low-English Hispanic arrivals.

In summary, our main estimate on balance appears conservative compared to other approaches in Table V.

43 The size distribution of districts is right skewed. If we treat the median, rather than mean, district, as the "average" district, the displacement rates we obtain are similar for most specifications, and larger for our main specification.
VII. School Quality versus Other Explanations

So far, we have shown that districts predicted to see more inflows of Mexican children to public schools in saw relatively large losses of non-Hispanic children. Inferring that these effects are a reaction to some immigration-induced reduction in school quality rests on the assumption that we have identified a credible comparison group – one which removes the effects of all other immigration-related factors that may induce non-Hispanic families with children to choose other districts. Because we can never be entirely sure that this assumption is satisfied, it is useful to explore some competing hypotheses and to provide additional empirical evidence of an effect of immigration on school quality.

VII.A. Crowding in the Housing Market

A key competing hypothesis for the effects we estimate is crowding in the housing market. To understand this, suppose that that housing costs, $p$, in equation (1) differ across household types $j$, and that immigrant families put greater pressure on housing costs for native households with children ($j=1$, i.e., by disproportionately seeking single-family residences) or $\frac{\partial p^1}{\partial i} > \frac{\partial p^0}{\partial i}$. Then even if $p$ is valued in the same way regardless of household type, changes in housing costs resulting from immigration will have a relatively large impact on households with children, possibly triggering settlement in a different district even absent any effect of immigration on school quality.

In an extreme case, where the housing stock is fixed and completely segmented (i.e., $\frac{\partial p^0}{\partial i} = 0$), immigrant households with children displace native households with children at a rate of one-for-one.

It is difficult to rule out this hypothesis, particularly since our analysis is focused on a state reputed for having a low price elasticity of housing supply. Nevertheless, several pieces of evidence imply that our estimates must be accounted for at least in part by something else. First, our estimates imply that more than one non-Hispanic household with children “left” a district for every
additional low-English immigrant household.\textsuperscript{44} So even in the extreme scenario outlined above, at least some the effect we estimate is due to something besides housing. Second, non-Hispanic children and low-English Hispanic children in metropolitan California live in very different types of houses. Our tabulations from the 2000 Census, for example, show that the houses in which low-English Hispanic children reside are much more likely to be rented (63 percent versus 36 percent) and on average have two fewer rooms.

Third, a direct investigation suggests that tight markets for family housing are not greatly affecting our estimates. Ideally, we would have at our disposal some district-level measure of constraints on new construction, such as the initial share of land in a district that is developable (see, for example, Card, Mas, and Rothstein, 2008). Unfortunately, such a measure is not available for 1970. However, we can observe initial population densities at the district level, both overall and by age group, which are strongly correlated with the more sophisticated measures available in later years, but not correlated with our instrument. We find no evidence that the effects of immigration are stronger in districts that initially have higher population densities, though initially denser districts do see significantly less growth in the representation of non-Hispanic children (results available on request).

\textbf{VII.B. Private Schooling}

Unlike decisions about where to live, which are affected by a number of factors, the decision to attend private school should be driven mainly by the quality of local public schools. In a more sophisticated version of the framework outlined in Section II, a native household might choose a district with schools that are lower quality as a result of immigration, and send their children to

\textsuperscript{44} To convert the displacements at the child level in the previous section to displacements in terms of households, we need to know the average number of children in non-Hispanic households with kids (1.86 in the 2000 Census), and the average number of low-English Hispanic kids in public schools in the average household with at least one such kid (1.63 in the 2000 Census). Dividing by the ratio of these two (+1.86/1.63) converts to displacement rates in terms of households. These remain above one after the conversion.
private school, if residence in that district yields other benefits (e.g., shorter commutes) that well exceed the costs of private education and of immigration on other aspects of life. This suggests that the non-Hispanic families that decide to reside in a district with a higher immigrant share in public schools might be more apt to send their children to private schools.45

Table VI presents TSLS and OLS estimates of the effect of the ELL Hispanic enrollment share on the 1970 to 2000 change in the non-Hispanic private school enrollment rate; as above, the baseline model controls for MSA by district type fixed effects. Both the TSLS estimates (Panel A) and OLS estimates (Panel B) are positive, indicating increases in low-English Hispanic share in the public schools induces non-Hispanics to depart to private schools, with OLS estimates highly significant and TSLS larger in magnitude but less precise. The TSLS coefficient of roughly 0.2 (column (1)) implies that a 10 percentage point increase in the ELL Hispanic share in public school enrollment prompted about 2 percent of a district's non-Hispanic children to enroll in private school.46 Controlling for the availability of a private school in 1976 – which was correlated with instrument (Table II, Panel C) – raises this estimate somewhat (column (2)), as might be expected, but adding the remaining controls has little impact (column (3)).47

These findings offer some complementary evidence of an effect on immigration on school quality and allow us to paint a more complete picture of how Mexican immigration may have affected non-Hispanic demand for public education in California. Even under the extreme

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45 This prediction assumes that households that choose high immigrant share districts do not have a relatively high latent propensity to send their kids to private school. Put differently, it is possible that non-Hispanics who choose high-immigrant districts would send their children to private school at higher rates regardless of whether immigration affects school quality. For this to explain all of the effect we see, however, selection would need to be extreme.

46 This response is close to the effect that Betts and Fairlie (2003) found at the MSA level for secondary students – that two natives enroll in private school for every 10 immigrant arrivals. Our study differs from theirs in several ways beyond the unit of observation, so this comparison should be made cautiously. Note that we are looking within a school district rather than a metropolitan area; so it is possible some of our earlier “relocation” response may include families who move out of a district and go on to enroll in private school somewhere in the metropolitan area. In addition, our estimates are identified in part off of children born in the United States to low-English immigrant parents, and we restrict attention to Hispanic immigrants to California, whereas Betts and Fairlie look at first-generation immigration of all types across the country.

47 We have also estimated the model for sub-samples defined by center city status and district type. Estimates were statistically indistinguishable across groups (though they are available on request).
assumption the lion’s share of our findings for district choice are driven by crowding in the housing market, our estimates imply that public schools in the average California school district lost two non-Hispanic students to other districts and two to private schools for every 10 additional low-English Hispanic students.

VIII. Conclusions

This paper has examined whether the large wave of predominantly Mexican, low-English Hispanic immigration to California since 1970 has reduced non-Hispanic demand for public education, as revealed through changes in the distribution of non-Hispanic children across school districts through 2000. Our empirical approach accounts for endogeneity of immigrant inflows using 1970 patterns of settlement among Mexican immigrants and attempts to compare the district choices non-Hispanic households with and without children in an attempt to isolate district choice in response to school quality. Supporting the credibility of our research design, districts predicted to receive more Mexican immigrants in their schools through 2000 on the basis of 1970 Mexican settlement were not already losing non-Hispanic children in the 1960s, and were comparable to other districts along many observable dimensions.

We find that districts that gained more low-English Hispanic students lost more non-Hispanic children than young adults to other districts in the same metropolitan area between 1970 and 2000. Not all of this effect can be explained by crowding in the housing market, and districts with larger increases in immigrant shares saw larger increases in their non-Hispanic private school enrollment rate over 1970 to 2000 as well. Both findings support our interpretation that the loss of non-Hispanic children was brought about at least in part by changes in school quality. While cannot rule out that non-Hispanic parents reacted more strongly to other aspects of residence in a district that changed as a result of immigration, we attempted to minimize this possibility in choosing a comparison group that expressed anti-immigrant sentiments to a similar degree.
That said, a limitation of our approach is that if Mexican immigration changed school quality for non-Hispanics in California, it is unclear how. For example, rising enrollments from immigration – combined with inadequate funding for new construction and a need, for most of the period, to educate English learners in separate classrooms – may be a primary contributor to crowding and poor physical conditions in California public schools. On the other hand, our estimates might also simply reflect distaste for ethnic or socioeconomic diversity in schools.\textsuperscript{48,49} Either way, our findings do suggest that public education may be a potentially important determinant of the residential choices of natives in response to immigration, and in turn, the residential isolation of immigrants.\textsuperscript{50} How this sorting plays out in the human capital accumulation of natives and immigrants alike – and whether it is manifested at all in other settings – are important topics for future research.

\textsuperscript{48} Such preferences could, to some extent, be exercised by sorting across schools attendance areas within district (Alesina, Baqir, and Hoxby, 2004; Kane, Riegg, and Staiger, 2006; Weinstein, 2009). Unfortunately, we cannot explore this using a similar research design since we lack a mapping of Census tracts to school attendance zones.

\textsuperscript{49} Shocks to school demographics that preceded the modern wave of immigration – most notably, through court-ordered racial desegregation – sparked “white flight” to the suburbs and private schools (Reber, 2005; Baum-Snow and Lutz, 2009; Boustan, 2009).

\textsuperscript{50} See Cutler, Glaeser, and Vigdor (2008b) for a century-long overview.
IX. Data Appendix

School District Level Data: Sources and Construction of Key Variables

A. 1970 Fourth Count (Population) School District Data Tapes

For 1970, school district level data on total population, by age and ethnicity, and private school enrollment, by ethnicity and level, were drawn from the 1970 Fourth Count (Population) School District Data Tapes. These data permit identification of all school districts in the country with at least 300 students (as of the 1969-70 school year).

Counts of school district residents by gender were originally reported for the total population and for the “Spanish Heritage” (hereafter referred to as Hispanic) population in the following age bins: under 3, 3-4, 5, 6, 7-9, 10-13, 14, 15, 16, 17, 18, and 19, 20, 21, to 22-24 (Table 17). For comparability with the 2000 data, we aggregated resident counts for the total population and the Hispanic population into five-year age bins (0-4, 5-9, 10-14, 15-19, and 20-24). We computed corresponding resident counts of non-Hispanics with the difference.

The original data also report counts school district residents aged 3 to 34 enrolled in private school, by level (kindergarten, elementary, and secondary), for the total population and for the Hispanic population (Table II8). For comparability with the 2000 data, we combine the kindergarten and elementary counts. To arrive at one private enrollment figure for the total population and for Hispanics, we also drop data on private school enrollment of individuals at levels not served by the district. We compute private enrollment counts for non-Hispanics by taking the difference between the total and Hispanic figures.

The final data we draw from this source are counts of “persons of foreign stock by nativity and country of origin,” for the total population and for the Spanish Heritage population (Table II2), used in construction of the instrument. Specifically, we use these data to construct \( \frac{M_d^{1970}}{M^{1970}} \) in equation (4) – the share of Mexicans residing in district \( d \) in 1970.

B. Census 2000 School District Tabulation

For 2000, school district level data on total population and private school enrollment, by age and ethnicity, were drawn from the Census 2000 School District Tabulation (STP2), available at <http://nces.ed.gov/surveys/sdds/downloadmain.asp>. All operating districts are included in the age-specific resident counts, but private enrollment counts are missing for districts with 49 or fewer children.

Counts of school district residents by gender were originally reported for the total population in one-year age bins through age 21 and for ages 22-24 (Table P8 for Total – Population and Households (TT)) and for the Hispanic/Latino population for the age categories 0-4, 5-9, 10-14, 15-17, 18-19, 20, 21, and 22-24 (Table 145H for TT). As in 1970, we aggregated resident counts for...
the total population and the Hispanic population into five-year age bins (0-4, 5-9, 10-14, 15-19, and 20-24) and computed the corresponding resident counts for non-Hispanics with the difference.

Counts of school district residents in private school, by gender, were available separately for all children and for Hispanic/Latino children either enrolled in or of age to be enrolled in the grades served by the district (Tables P8 and 145H for Children (CO): Relevant Children – Enrolled Private) for the age categories 0-4, 5-9, 10-14, 15-17, 18-19. For comparability with the 1970 data, we aggregate across all age categories and across gender to create one private enrollment figure each for the total population and for Hispanics.

To avoid disclosure, cell values are also rounded so that exact values cannot be inferred; generally, this rounding is to the nearest 5, or to 4, when the population count is under 5. On a few occasions, rounding leads to (small) negative values.

C. Fall 1976 Elementary and Secondary School Civil Rights Survey

For 1976, school district level data on the number of ELL students, by ethnicity, were drawn from the Fall 1976 Elementary and Secondary School Civil Rights Survey, fielded by the Office for Civil Rights in the Department of Health, Education, and Welfare and recently decoded from binary to Stata format by Denckla and Reber (2006). The 1976 OCR survey covered all elementary and secondary school districts in the United States.

The original data give counts of “pupils whose primary language is other than English” in total and by race/ethnicity. Our treatment variable is constructed using the number of Hispanics (of all races) with this designation. Race/ethnicity categories are American Indian/Alaskan Native, Asian/Pacific Islander, Black non-Hispanic, White non-Hispanic, and Hispanic.

D. 2000 Elementary and Secondary School Civil Rights Compliance Report District Summary

For 2000, school district level data on the number of ELL students, by ethnicity, were drawn from the 2000 Elementary and Secondary School Civil Rights Compliance Report District Survey, fielded by the OCR in the U.S. Department of Education and downloaded from <http://www.ed.gov/about/offices/list/ocr/data.html>. The 2000 OCR survey covered all elementary and secondary school districts in the United States, with tabulations rounded to the nearest 5, to avoid disclosure.
X. Derivation of alternative estimation equation

To get to our alternative estimation strategy which takes account of the fact that adults in our comparison groups are "partially treated" because many have kids, sum up equation (2) instead to age groups, indexed with $a$, by family type $j$ within each metropolitan area:

\[(6) \quad N_{dmt}^{ja} = N_{mt}^{ja}(\gamma_{dt} + \beta_j i_{dmt}) + \sum_{i=amj} \eta_{i,dmt}.'\]

We do not observe family type in our data, just age group. So further summing (6) across age family types and dividing by the total age-specific population in metro area $m$ and year $t$, we have that:

\[(6') \quad \frac{N_{dmt}^{a}}{N_{m}^{a}} = \frac{N_{mt}^{a}}{N_{m}^{a}}(\gamma_{dt} + \beta_0 i_{dmt}) + \frac{N_{mt}^{a}}{N_{m}^{a}}(\gamma_{dt} + \beta_1 i_{dmt}) + u_{dmt}^{a} \]

\[
= \gamma'_{dt} + (\beta_1 - \beta_0) \varphi_{amt} \cdot i_{dmt} + u_{dmt}^{a},
\]

where $\gamma'_{dt} = \gamma_{dt} + \beta_0 i_{dmt}$ and $\varphi_{amt} = N_{mt}^{a}/N_{m}^{a}$, and $u$ is a linear combination of the $\epsilon$'s. Note that $\varphi_{amt}$ the fraction of age $a$ individuals families with kids, in $m$ in year $t$, which in theory can be obtained from other data without detailed geography. In practice, to get a more precise estimate we dispense with the time-metro variation in this fraction, using only the variation across age groups. Also define $\theta' = (\beta_1 - \beta_0)$ as our parameter of interest. Putting these into (6') and differencing gets us to our alternative estimation equation (which is the same as (3) above):

\[(6'') \quad \frac{\Delta N_{dmt}^{a}}{N_{m}^{a}} = \gamma'_{dt} + \theta' \varphi_{a} \cdot \Delta i_{dmt} + \Delta u_{dmt}^{a} .\]

The estimate of theta from (6''), however, requires one further adjustment to make it comparable to our main specification. Note that our main specification estimates the number of kids who leave a district due to immigration-induced changes in school quality (relative to adults). This specification instead estimates the number of people (of all ages) in households with children who leave a district due to immigration-induced changes in school quality (relative to households without children). So to be strictly comparable to $\theta$ estimated using our main approach, $\theta'$ needs to be adjusted downward by a factor representing the fraction of people in households with children who are children.
References


Table I. Descriptive Statistics for California School Districts

<table>
<thead>
<tr>
<th>A. Key Variables</th>
<th>Mean (1)</th>
<th>St. Dev. (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Δ public enr. share low-English Hispanic, 1976-2000</td>
<td>0.107</td>
<td>0.098</td>
</tr>
<tr>
<td>Instrument †</td>
<td>0.114</td>
<td>0.110</td>
</tr>
<tr>
<td>Δ share of MSA’s non-Hispanics in district, 1970-2000:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>0-19 Year Olds (T)</td>
<td>-0.004</td>
<td>0.040</td>
</tr>
<tr>
<td>20-49 Year Olds (C)</td>
<td>-0.003</td>
<td>0.044</td>
</tr>
<tr>
<td>T-C Difference</td>
<td>-0.001</td>
<td>0.014</td>
</tr>
<tr>
<td>Δ Non-Hispanic private enrollment rate, 1970-2000</td>
<td>0.103</td>
<td>0.051</td>
</tr>
</tbody>
</table>

| B. Pre-existing levels of outcomes                                               |          |              |
| Share of MSA’s non-Hispanics in district, 1970:                                  |          |              |
| 0-19 Year Olds (T)                                                               | 0.079    | 0.120        |
| 20-49 Year Olds (C)                                                              | 0.079    | 0.125        |
| T-C Difference                                                                   | 0.000    | 0.013        |
| Non-hispanic private enrollment rate, 1970                                      | 0.014    | 0.015        |

| C. Other pre-existing district characteristics                                    |          |              |
| public enr. share low-English Hispanic, 1976                                     | 0.026    | 0.044        |
| public school enrollment, 1976                                                    | 14849    | 41486        |
| per-pupil property tax revenue, 1971-72                                          | 636      | 292          |
| per-pupil total expenditures, 1971-72                                            | 1084     | 259          |
| =1 if elementary-high organization                                               | 0.172    |              |
| =1 if above-average #public schools | enrollment, land area, 1972           | 0.579    |              |
| =1 if at least one private school, 1976                                           | 0.712    |              |
| =1 if center city, 1972                                                           | 0.094    |              |
| Number of Observations                                                            | 233      |              |
| Number of MSAs                                                                    | 23       |              |

Notes: * The unit of observation either a unified school district or a combination of school districts that serve the same geographic area and all elementary and secondary grades (generally one secondary district plus a number of elementary districts). The sample includes all such observations with complete data on the characteristics listed. See text and Data Appendix for more details on sample construction and description of data sources. † The instrument is the 1976 to 2000 predicted change in low-English Hispanic public school enrollment arrived at by apportioning low-English Hispanic first- and second-generation immigrants from Mexico and enrolled in public school in 2000 to the school districts where immigrants of all ages from Mexico settled in 1970. An individual is classified as "low-English" if reported to speak English "not at all", "not well," or "well" and "second generation" if native-born to at least one Mexican-born parent (or if both parents are foreign-born, to a Mexican-born mother) who arrived in the U.S. in 1976 or later.
### Table II. Are the Identifying Assumptions Satisfied?
The Instrument and District Observables

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Coefficient (standard error) on the Instrument</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>A. Change in immigrant share</strong>&lt;br&gt;Δ public enr. share low-English Hispanic, 1976-2000</td>
<td>0.431*** (0.099)</td>
</tr>
<tr>
<td><strong>B. Pre-existing levels of outcomes</strong>&lt;br&gt;Share of MSA's non-Hispanics in district, 1970:&lt;br&gt;0-19 Year Olds (T)</td>
<td>-0.238 (0.196)</td>
</tr>
<tr>
<td>20-49 Year Olds (C)</td>
<td>-0.242 (0.206)</td>
</tr>
<tr>
<td>T-C Difference</td>
<td>0.005 (0.017)</td>
</tr>
<tr>
<td>Non-hispanic private enrollment rate, 1970</td>
<td>0.000 (0.007)</td>
</tr>
<tr>
<td><strong>C. Other pre-existing district characteristics</strong>&lt;br&gt;public enr. share low-English Hispanic, 1976</td>
<td>0.253*** (0.040)</td>
</tr>
<tr>
<td>ln(public school enrollment, 1976)</td>
<td>-1.69 (1.95)</td>
</tr>
<tr>
<td>per-pupil property tax revenue, 1971-72</td>
<td>-197.2 (188.8)</td>
</tr>
<tr>
<td>per-pupil total expenditures, 1971-72</td>
<td>-172.5 (167.6)</td>
</tr>
<tr>
<td>=1 if above-average #public schools</td>
<td>0.103 (0.296)</td>
</tr>
<tr>
<td>=1 if at least one private school, 1976</td>
<td>-1.350*** (0.416)</td>
</tr>
<tr>
<td>=1 if center city</td>
<td>0.001 (0.332)</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>233</td>
</tr>
<tr>
<td>Number of MSAs</td>
<td>23</td>
</tr>
</tbody>
</table>

*Notes:* Each entry gives the coefficient (standard error) on the 1976 to 2000 predicted change in low-English Hispanic public school enrollment arrived at by apportioning low-English Hispanic first- and second-generation immigrants from Mexico and enrolled in public school, in the 2000 Census of Population, to the school districts where immigrants of all ages from Mexico settled in 1970, according to the 1970 Fourth Count (Population) School District Data Tapes. An individual is classified as "low-English" if reported to speak English "not at all," "not well," or "well" and "second generation" if native-born to at least one Mexican-born parent (or if both parents are foreign-born, to a Mexican-born mother) who arrived in the U.S. in 1976 or later. All regressions are based on the full sample of California unified and combined elementary x high school districts (233 districts in 23 MSAs), and include fixed effects for MSA by district type. Standard errors (in parentheses) are calculated to be robust to arbitrary error correlation within MSA. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.
<table>
<thead>
<tr>
<th>Dep. Var.: Δ Share of MSA's non-Hispanic Population in District</th>
<th>Ages 0-19 (T)</th>
<th>Ages 20-49 (C)</th>
<th>Difference (T-C)</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) (2) (3)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Coefficient (standard error) on instrument</td>
<td>-0.0729</td>
<td>-0.0425</td>
<td>-0.0304**</td>
</tr>
<tr>
<td></td>
<td>(0.0546)</td>
<td>(0.0548)</td>
<td>(0.0140)</td>
</tr>
<tr>
<td>Number of Districts</td>
<td>233</td>
<td>233</td>
<td>233</td>
</tr>
<tr>
<td>Number of MSAs</td>
<td>23</td>
<td>23</td>
<td>23</td>
</tr>
</tbody>
</table>

**A. Full Sample: 1970-2000**

| Coefficient (standard error) on instrument                   | -0.0041       | 0.0312         | -0.0353*        |
|                                                               | (0.0664)      | (0.0712)       | (0.0163)        |
| Number of Districts                                         | 161           | 161            | 161             |
| Number of MSAs                                               | 13            | 13             | 13              |


| Coefficient (standard error) on instrument                   | -0.0217       | -0.0151        | -0.0066         |
|                                                               | (0.0344)      | (0.0319)       | (0.0093)        |
| Number of Districts                                         | 161           | 161            | 161             |
| Number of MSAs                                               | 13            | 13             | 13              |

**C. 1960-1970**

| Coefficient (standard error) on instrument                   | -0.0729       | -0.0425        | -0.0304**       |
|                                                               | (0.0546)      | (0.0548)       | (0.0140)        |
| Number of Districts                                         | 233           | 233            | 233             |
| Number of MSAs                                               | 23            | 23             | 23              |

Notes: The dependent variable in the first two columns is the change in the share of an MSA's non-Hispanic population residing in the district, for the age group specified in the column and over the period and for the sample of districts specified in the panel. Sources for the district-level data are (by year): the Census 2000 School District Tabulation (2000), the 1970 Fourth Count (Population) School District Data Tapes (1970), and Census tract data compiled by the National Historical Geographic Information System (1960). 1960 tracts are matched to school districts using the procedure described in the Data Appendix. MSA population counts by age group are aggregated from county-level data from NHGIS (1970 and 2000) and tract-level data from NHGIS (1960). The instrument is the 1976 to 2000 predicted change in low-English Hispanic public school enrollment arrived at by apportioning low-English Hispanic first- and second-generation immigrants from Mexico and enrolled in public school, in the 2000 Census of Population, to the school districts where immigrants of all ages from Mexico settled in 1970, according to the 1970 Fourth Count (Population) School District Data Tapes. An individual is classified as "low-English" if reported to speak English "not at all", "not well" or "well" and "second generation" if native-born to at least one Mexican-born parent (or if both parents are foreign-born, to a Mexican-born mother) who arrived in the U.S. in 1976 or later. All regressions are based on the based on the full sample of California unified and combined elementary x high school districts (233 districts in 23 MSAs), unless otherwise noted, and include fixed effects for MSA by district type. Standard errors (in parentheses) are calculated to be robust to arbitrary error correlation within MSA. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.
Table IV. TSLS and OLS Estimates for Non-Hispanic District Choice

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Sample: All (Unified+Combined Elem/High)</td>
<td>Not Center City Unified Elem/High</td>
<td>Mean (C), 1970</td>
<td>0.079</td>
<td>0.060</td>
<td>0.073</td>
<td>0.111</td>
<td></td>
</tr>
<tr>
<td>Coefficient (standard error) on: Δ public school enrollment share low-English Hispanic, 1976-2000</td>
<td></td>
<td>-0.0705**</td>
<td>-0.0802*</td>
<td>-0.0790*</td>
<td>-0.0674**</td>
<td>-0.0764*</td>
<td>-0.0164</td>
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<tr>
<td></td>
<td>low-English Hispanic, 1976-2000</td>
<td>(0.0318)</td>
<td>(0.0441)</td>
<td>(0.0438)</td>
<td>(0.0288)</td>
<td>(0.0385)</td>
<td>(0.0206)</td>
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<tr>
<td>RMSE</td>
<td>0.0147</td>
<td>0.0147</td>
<td>0.0147</td>
<td>0.0119</td>
<td>0.0153</td>
<td>0.0101</td>
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</tr>
<tr>
<td>First stage partial F-stat on instrument</td>
<td>19.0</td>
<td>25.8</td>
<td>27.7</td>
<td>18.2</td>
<td>12.4</td>
<td>14.1</td>
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</tr>
<tr>
<td>Coefficient (standard error) on: Δ public school enrollment share low-English Hispanic, 1976-2000</td>
<td></td>
<td>-0.0215**</td>
<td>-0.0035</td>
<td>-0.0040</td>
<td>-0.0183*</td>
<td>-0.0218**</td>
<td>-0.0068</td>
</tr>
<tr>
<td></td>
<td>low-English Hispanic, 1976-2000</td>
<td>(0.0100)</td>
<td>(0.0107)</td>
<td>(0.0105)</td>
<td>(0.0101)</td>
<td>(0.0102)</td>
<td>(0.0100)</td>
</tr>
<tr>
<td>RMSE</td>
<td>0.014</td>
<td>0.0132</td>
<td>0.0133</td>
<td>0.0111</td>
<td>0.0144</td>
<td>0.0101</td>
<td></td>
</tr>
<tr>
<td>Number of Districts</td>
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<td>233</td>
<td>233</td>
<td>211</td>
<td>193</td>
<td>40</td>
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<tr>
<td>Number of MSAs</td>
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<td>23</td>
<td>23</td>
<td>23</td>
<td>23</td>
<td>19</td>
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<tr>
<td>Controls:</td>
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<td>MSA fixed effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
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<td>District type fixed effect</td>
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<td>X</td>
<td>X</td>
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<tr>
<td></td>
<td></td>
<td>MSA by district type fixed effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>T-C diff. in dep. var., 1970</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: "T" represents 0-19 year olds and "C" represents 20-49 year olds. The first row gives the share of an MSA's non-Hispanic 20-49 year olds residing in the average district in 1970. The instrument used in panel A is the 1976 to 2000 predicted change in low-English Hispanic enrollment arrived at by apportioning low-English Hispanic first- and second-generation immigrants from Mexico and enrolled in public school in 2000 to the school districts where immigrants of all ages from Mexico settled in 1970. An individual is classified as "low-English" if reported to speak English "not at all," "not well" or "well" and "second generation" if native-born to at least one Mexican-born parent (or if both parents are foreign-born, to a Mexican-born mother) who arrived in the U.S. in 1976 or later. Standard errors (in parentheses) are calculated to be robust to arbitrary error correlation within MSA (23 MSAs, unless otherwise noted). ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively. † Pre-existing district characteristics include: natural log of 1976 total enrollment, per-pupil property tax revenue and per-pupil total expenditure (1971-72), and indicators for center city district (1969-70), above-average number of public schools given land area and enrollment (1972), and with at least one private school (1976).
Table V. Sensitivity of the Estimates for Non-Hispanic District Choice

<table>
<thead>
<tr>
<th>Dependent Variable is:</th>
<th>TSLS (1)</th>
<th>OLS (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>A. Choice of Outcome</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(a) T-C Diff: $\Delta s_{nh}$ (Share of MSA non-Hispanic Pop. in District), 1970-2000</td>
<td>-0.0705*** (0.0318)</td>
<td>-0.0215** (0.0100)</td>
</tr>
<tr>
<td></td>
<td>RMSE</td>
<td>0.0147</td>
</tr>
<tr>
<td>(b) T-C Diff: $\Delta \ln(t_{nh}/1-t_{nh})$, 1970-2000</td>
<td>-1.213*** (0.357)</td>
<td>-0.281** (0.110)</td>
</tr>
<tr>
<td></td>
<td>Marginal Effect</td>
<td>-0.0880</td>
</tr>
<tr>
<td></td>
<td>RMSE</td>
<td>0.209</td>
</tr>
<tr>
<td>(c) T-C Diff: $\Delta \ln(pop_{nh})$, 1970-2000</td>
<td>-1.077*** (0.319)</td>
<td>-0.242** (0.098)</td>
</tr>
<tr>
<td></td>
<td>RMSE</td>
<td>0.195</td>
</tr>
<tr>
<td>(d) $\Delta pop_{nh}/T_{nh} \text{pop}_{nh}$, 1970-2000</td>
<td>-0.250*** (0.077)</td>
<td>-0.062** (0.024)</td>
</tr>
<tr>
<td></td>
<td>RMSE</td>
<td>0.0445</td>
</tr>
<tr>
<td>B. Alternative Specification</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(a) C age groups not treated</td>
<td>-0.0710** (0.0315)</td>
<td>-0.0259* (0.0119)</td>
</tr>
<tr>
<td></td>
<td>RMSE</td>
<td>0.0153</td>
</tr>
<tr>
<td>(b) C age groups treated in prop. to frac of group in HHs with 0-19 year olds</td>
<td>-0.1640** (0.0702)</td>
<td>-0.0620** (0.0297)</td>
</tr>
<tr>
<td></td>
<td>RMSE</td>
<td>0.0153</td>
</tr>
</tbody>
</table>

Notes: "T" represents 0-19 year olds and "C" represents 20-49 year olds. Each entry represents a different regression. All regressions are based on the full sample of California unified and combined elementary-high school districts (233 districts in 23 MSAs). Specifications in Panel A are estimated using one observation per district, include fixed effects for MSA by district type, and differ only in the dependent variable. The explanatory variable of interest is the 1976 to 2000 change in the share Hispanic low-English in public school enrollment, instrumented (in column (1)) with the predicted change arrived at by apportioning low-English Hispanic first- and second-generation immigrants from Mexico and enrolled in public school in 2000 to the school districts where immigrants of all ages from Mexico settled in 1970. (See notes to earlier tables for more details.) Marginal effects (row b) are calculated by multiplying the regression coefficient by $sn_{nh}(1-s_{nh})$. Specifications in Panel B are estimated using 10 observations per district (one for each 5-year age group for ages 0-49) and include fixed effects for district and for age group by MSA by district type. The dependent variable is the 1970-2000 change in the share of the MSA's non-Hispanics in that age group residing in the district. The explanatory variable of interest is the 1976 to 2000 change in the share Hispanic low-English in public school enrollment interacted with either an indicator for aged 0-19 (row a) or the share of individuals in the age group residing in households with 0-19 year olds (in California in the 2000 Census) (row b). The instrument is constructed analogously using the predicted change in share Hispanic low-English described above. Standard errors (in parentheses) are clustered on MSA. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.
### Table VI. TSLS and OLS Estimates Non-Hispanic Private School Enrollment

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean, 1970</td>
<td>0.0137</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

#### A. Two Stage Least Squares

**Coefficient (standard error) on:**
- Δ public school enrollment share
  - low-English Hispanic, 1976-2000
  - Coefficient: 0.184* (0.089)
  - Coefficient: 0.260** (0.100)
  - Coefficient: 0.274** (0.104)

| RMSE | 0.0418 | 0.0433 | 0.0427 |
| First stage partial $F$-stat on instrument | 19.0 | 17.3 | 27.7 |

#### B. Ordinary Least Squares

**Coefficient (standard error) on:**
- Δ public school enrollment share
  - low-English Hispanic, 1976-2000
  - Coefficient: 0.061*** (0.028)
  - Coefficient: 0.074*** (0.029)
  - Coefficient: 0.069** (0.031)

| RMSE | 0.0403 | 0.0398 | 0.0391 |
| Number of Districts | 233 | 233 | 233 |
| Number of MSAs | 23 | 23 | 23 |
| Controls: | | | |
| MSA by district type fixed effects | X | X | X |
| Has private school, 1976 (=1) | X | X |
| Other pre-existing district characteristics† | | X |

**Notes:** The first row gives the share non-Hispanics of school age attending private school in the average district in 1970. The dependent variable in Panels A and B is the 1970 to 2000 change in the district’s non-Hispanic private school enrollment rate. Data sources are (by year): the Census 2000 School District Tabulation (2000) and the 1970 Fourth Count (Population) School District Data Tapes (1970). The instrument used in panel A is the 1976 to 2000 predicted change in low-English Hispanic enrollment arrived at by apportioning low-English Hispanic first- and second-generation immigrants from Mexico and enrolled in public school in 2000 to the school districts where immigrants of all ages from Mexico settled in 1970. See notes to earlier tables and text for more details. Standard errors (in parentheses) are calculated to be robust to arbitrary error correlation within MSA (23 MSAs). † Other pre-existing district characteristics include: the 1970 non-Hispanic private school enrollment rate, the natural log of 1976 total enrollment, per-pupil property tax revenue and per-pupil total expenditure (1971-72), and indicators for center city district (1969-70) and above-average number of public schools given land area and enrollment (1972).
Appendix Table I. Construction of the Instrument

<table>
<thead>
<tr>
<th>Share of Mexicans in 1970:</th>
<th>Instrument:</th>
</tr>
</thead>
<tbody>
<tr>
<td>Rank</td>
<td>Value</td>
</tr>
<tr>
<td>A. CA Districts Ranked by Share of U.S. Mexican-Born Population, 1970</td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>0.1626</td>
</tr>
<tr>
<td>2</td>
<td>0.0124</td>
</tr>
<tr>
<td>3</td>
<td>0.0094</td>
</tr>
<tr>
<td>4</td>
<td>0.0091</td>
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<tr>
<td>5</td>
<td>0.0066</td>
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<tr>
<td>6</td>
<td>0.0064</td>
</tr>
<tr>
<td>7</td>
<td>0.0058</td>
</tr>
<tr>
<td>8</td>
<td>0.0051</td>
</tr>
<tr>
<td>9</td>
<td>0.0050</td>
</tr>
<tr>
<td>10</td>
<td>0.0045</td>
</tr>
<tr>
<td>B. CA Districts Ranked by Predicted Change in Hispanic-low-English Share in Public School Enrollment, 1976-2000</td>
<td></td>
</tr>
<tr>
<td>95</td>
<td>0.0006</td>
</tr>
<tr>
<td>26</td>
<td>0.0023</td>
</tr>
<tr>
<td>36</td>
<td>0.0018</td>
</tr>
<tr>
<td>6</td>
<td>0.0064</td>
</tr>
<tr>
<td>77</td>
<td>0.0007</td>
</tr>
<tr>
<td>61</td>
<td>0.0010</td>
</tr>
<tr>
<td>44</td>
<td>0.0015</td>
</tr>
<tr>
<td>55</td>
<td>0.0012</td>
</tr>
<tr>
<td>127</td>
<td>0.0003</td>
</tr>
<tr>
<td>54</td>
<td>0.0012</td>
</tr>
</tbody>
</table>
Figure I. Trends in the Mexican Share of the Child Population in the United States

Note: Sources are SIE (1976) and Census PUMS. Sample includes 0-17 year olds and Mexicans include both the Mexican-born and the children of Mexican-born parents.
Figure II. First-Stage Relationship
1970 to 2000 Change in ELL Hispanic Pub. Enr. Share

Notes: Regression adjusted for MSA x district type fixed effects. Sample includes unified and combined HS-elem districts in 23 MSAs in CA.
Figure III. Reduced-Form Relationship
T-C: 1970-2000 Change in Share of MSA's non-Hispanics in District

Notes: T=0-19 year olds. C=20-49 year olds. Regression adjusted for MSA x district type fixed effects. Sample includes unified and combined HS-elem districts in 23 MSAs in CA.
Figure IV. Pre-Trends in Outcomes?
T-C Diff: Change in Share of MSA's non-Hispanics in District

Notes: T=0-19 year olds. C=20-49 year olds. Regression adjusted for MSA x district type fixed effects. Sample includes 13 MSAs in CA with 1960 data available.